LACUS FORUM XXVIII

Evidence in Linguistics

UQÀM

Université du Québec à Montréal

Contents

I. Featured Lectures

II. The Nature of Evidence

III. The Application of Evidence

FACT OR OPINION: A SOCIOLINGUISTIC VIEW OF NATIVE-SPEAKER INTUITIONS AS EVIDENCE IN LINGUISTICS

CHARLES BOBERG
McGill University

THIS PAPER ADDRESSES THE CONFERENCE THEME—the nature of evidence in linguistics—from a sociolinguistic point of view. In particular, it examines the extent to which native-speaker intuitions, the standard kind of evidence in much of contemporary theoretical linguistics, can provide reliable evidence of a speaker's grammar. It will attempt to show, by means of examples from a study of sound change in progress in North American English, that what native speakers tell a linguist about their grammar only sometimes aligns with the facts about that grammar that might be arrived at through empirical observation.

1. INTRODUCTION: THREE KINDS OF EVIDENCE. There are three kinds of evidence in linguistics. Given the centrality of the question of what counts as evidence to the nature of scientific inquiry, we can to some extent propose a typology of evidence as the basis for a corresponding typology of linguistic science that is somewhat different from the usual one. Historical linguistics has traditionally made use of written texts and comparative reconstruction, the only kind of evidence available on language as it was spoken before the advent of the tape recorder. By contrast, branches of linguistics concerned primarily with the linguistic present rather than the linguistic past have preferred to study spoken language. Of these, traditional dialectology and much of contemporary theoretical linguistics have usually collected data by consulting the intuitions of native speakers about their grammars, elicited by means of questionnaires, fieldwork and grammaticality judgments, rather than by observing the unconscious use of language in everyday acts of communication. This approach of course requires an assumption that speakers have conscious access to their grammars, and that their intuitions about them are accurate and can be coherently and intelligibly reported.

The third kind of evidence in linguistics is the empirical observation of speech, in which the immediate object of study is linguistic production itself, rather than speakers' reports or opinions about language. The approach of examining what speakers do rather than what they say they do has been favored by phonetics and sociolinguistics, for different reasons. In phonetics, it is necessitated by the obscurity of subphonemic differences in sound. Without phonetic training, an informative discussion of the subject matter is impossible. Even if speakers can hear the relevant differences in their own speech (which sometimes they cannot), they possess no vocabulary with which to comment on them. The sociolinguistic preference for empirical data is more a matter of choice. Data on social variation in language *could* be (and sometimes *is*)

gathered by eliciting native speakers' intuitions about it, and sociolinguistics could be primarily about the influence of social factors on speakers' opinions about language. The innovators of the field, however, pursued instead an empirical methodology, because they explicitly rejected the assumption that speakers' intuitions are an accurate and reliable source of data on grammar.

2. SOCIOLINGUISTICS AND THE EMPIRICAL OBSERVATION OF LANGUAGE. One of the cornerstones of the sociolinguistic enterprise has been an insistence upon the empirical observation of language in its social context—of language used in everyday acts of communication among members of a speech community—as the primary method for studying the rules and representations of grammar. This insistence arises from what Labov (1972:209) has termed the *observer's paradox*: that the nature of what we want to observe is distorted by the very act of observation. Many sociolinguistic studies have shown that speakers shift their production in the direction of what they perceive to be the standard or most prestigious variety of the language when they know they are being observed, or when they are compelled to pay attention to their speech by direct questions about it. The most that direct elicitation of linguistic data can therefore hope to achieve is an indication of a speaker's opinion of the correct way to speak. It cannot usually determine how people speak when they are not being observed and not paying attention to their speech—the *vernacular*, in sociolinguistic parlance. Because the primary purpose of language is communication among members of the speech community rather than the discussion of language with linguists, vernacular speech is held by sociolinguists to be the primary object of linguistic study. Moreover, sociolinguistic studies have shown the vernacular to be the most consistent and systematic level of linguistic production, free of self-conscious and sporadic attempts at correction, and therefore the best source of data on a speaker's grammar (ibid 208).

Three principal methods have been devised for overcoming the observer's paradox and gaining access to the vernacular. Perhaps the most famous is the *rapid and anonymous* survey technique introduced by Labov in his study of the vocalization of /r/ in New York City department stores (Labov 1966). This approach attacked the problem of observation simply and directly by collecting data in a context in which speakers were not aware that they were being observed. By sacrificing the need for background information on speakers beyond what could be visually observed, and by limiting the object of study to a single linguistic variable, Labov disguised his investigation as an inquiry about the location of merchandise in a store (items known to be on the *fourth floor*). This allowed him to observe the unconscious and unreflecting use of language by a wide sample of members of the speech community for ordinary purposes connected with their daily activities. Even in this study, a small shift in the direction of the standard (/r/-insertion) was found to occur when store employees were asked to repeat their response.

The second and most common method of eliciting data on the vernacular is the *sociolinguistic interview*, a lengthy interaction between linguist and informant in

which a wide range of speech styles is elicited, as in the urban surveys carried out by Labov in New York City (1966) and Trudgill in Norwich (1974). Elicitation techniques range from the discussion of minimal pairs and the reading of word lists, which elicit very formal, self-conscious speech, to encouraging the subject to tell stories about emotionally charged personal experiences. These narratives elicit a style that is as close to the vernacular as is possible in the presence of a linguist and a tape recorder, because the subject is so concerned with the task of communicating the emotional content of the story that very little attention can be paid to linguistic form. Rather than studying only vernacular speech, then, sociolinguistic interviews control speech style as an independent variable, to be considered along with purely linguistic factors and the external, social characteristics of the speaker in the study of language.

A third method of overcoming the observer's paradox is less commonly practiced, because it requires a much greater investment of time. This is the ethnographic tradition of *participant-observation*, borrowed from anthropology, in which the linguist, perhaps over the course of a year or more, becomes sufficiently involved in the speech community under study that the presence of an observer is no longer remarkable to its members. The observer may, in fact, play a role in the community other than that of observer; this role may even be a condition of the possibility of discreet observation. In these circumstances, a long-term study can be made of daily linguistic interactions that are presumably free of self-monitoring on the part of speakers. Examples of this approach are Cheshire's study of playgrounds in Reading (1978) and Eckert's study of a high school in Livonia, near Detroit (1988).

3. PRACTICAL LIMITATIONS ON EMPIRICAL OBSERVATION. As valuable as empirical observation may be, it is not always practical. It has already been pointed out that rapid and anonymous surveys cannot gather background information on speakers beyond what can be observed visually (sex, approximate age, occupational status and physical context). In a study where residential history is essential to establish, for example, a rapid and anonymous survey would not be feasible. Moreover, rapid and anonymous studies impose severe restrictions on the type of variable that can be examined. It is relatively easy to elicit an interaction that will feature a phonological variable, but much harder to ensure that a morphological or syntactic variable will be part of the elicited response. Further restrictions are imposed by ethical objections to surreptitious tape recording: rapid and anonymous data must be transcribed on the scene, by hand, immediately after they are gathered. This requirement means that the study has to focus on only one or two variables, since a larger range of observations would be impossible to remember and transcribe accurately. Finally, rapid and anonymous surveys and participant-observer studies require social and linguistic access to the speech community, which is not always available when linguists work on languages other than their own.

Sociolinguistic interviews can overcome many of the limitations of rapid and anonymous surveys. Indeed, Labov (1972:60–62) has observed that these two methods have complementary strengths and weaknesses, so that convergence of the results

obtained by each method provides strong confirmation of the accuracy of the data. However, sociolinguistic interviews and the analysis of long samples of recorded speech require considerable time and resources on the part of both linguists and subjects, which are sometimes in short supply or difficult to obtain. Conducting interviews may in fact be impossible, if the linguist is studying a language spoken in a distant place but cannot travel there. Even when it is possible, there are limitations on what sort of data can be effectively studied in natural speech. For instance, whereas most phonetic variables occur frequently in any quantity of speech, the development of syntactic theory sometimes depends on the grammaticality of certain crucial constructions that occur so rarely in natural speech that collecting and analyzing vernacular data would be overly time-consuming. In these cases, directly eliciting grammaticality judgments may seem the only reasonable way to proceed.

Even the study of phonetic and phonological variation can sometimes benefit from the use of word lists and reading passages to ensure an adequate quantity of data on the realization of variables in less frequent allophonic environments, or to eliminate confounding variables like stress and phonological context in the comparison of such environments. The study of phonological mergers and splits, either as part of the synchronic description of the phonemic inventory of a language, or as part of a study of sound change in progress, necessarily involves the elicitation of native-speaker judgments, since Labov (1994:293–418) has shown that speakers' *perception* of phonemic contrasts is sometimes quite different from their *production* of the same contrasts, and that the status of both must be established if a clear understanding of the nature of phonemic systems and of the mechanisms of phonemic change is to be arrived at. *Minimal pairs* that feature the phonological variable in identical contexts are therefore the phonologist's equivalent of the grammaticality tests of syntacticians: speakers are asked whether two words rhyme, or sound the same or different, in order to establish the phonemic status of the single sound by which they might differ.

When any one of the above conditions applies, even sociolinguists may be tempted to rely on native-speaker intuitions, either explicitly articulated in grammaticality judgments and minimal pair tests or performed in the reading of word lists or passages, as evidence of the linguistic facts they seek to uncover. The remainder of this paper will present data that illuminate the conditions under which the cautious use of intuitions as evidence in linguistics is more or less reliable. The focus will be on the case of establishing the presence or absence of phonemic contrast, since this usually involves both the elicitation of judgments from the speaker and an independent assessment of contrast by the analyst, therefore providing the possibility of a direct comparison between the two approaches.

4. THE STUDY OF PHONEMIC CONTRAST: DATA FROM THE TELSUR PROJECT. A crucial issue in the description of any language is the establishment of its phonemic inventory, the set of contrasting sounds that underlies its ability to relate meaning to sound in a linguistic signaling system. It is well known that phonemic inventories are neither diachronically stable nor uniform across dialects. Phonemic contrasts can arise

or disappear over time, and a contrast made in one dialect can be absent in another. Labov has shown that one of the most fundamental and consequential ways in which dialects of North American English differ is in the number and nature of phonemic contrasts they maintain in their vowel systems (Labov 1991). Some dialects maintain a contrast between two historical developments of Middle English short-*a*, so that *past* and *pat* have different vowels, while most North Americans have the same vowel in these words. A more general divide in North American English is between dialects that distinguish /ɑ/ and /ɔ:/, the vowels of *cot* and *caught*, and dialects in which they are merged as a single phoneme. Beyond these general, unconditioned mergers, there are conditioned neutralizations of contrast that occur in specific phonetic environments in certain dialects. These include the merger of /i:/ and /ɪ/, /e:/ and /ɛ/, and /u:/ and /ʊ/ before /l/ (*peel/pill, sail/sell, fool/full*); of /ɛ/ and /æ/ and /ɔ/ and /o:/ before /r/ (*merry/marry*; *for/four*); and of /ɪ/ and /ɛ/ before nasals (*pin/pen*; *him/hem*).

These variables of phonemic inventory, together with the systematic shifts in the phonetic realization of vowels to which they are structurally related, are the primary focus of an extensive survey of regional variation and change in North American English now underway at the Linguistics Laboratory of the University of Pennsylvania, known as the TELSUR Project, directed by William Labov. During the 1990s, TELSUR used a combination of sociolinguistic interviews conducted over the telephone and computerized acoustic analysis to assemble a set of phonetic and phonological data on North American English that covers the entire continent, an achievement that had so far eluded the field survey tradition of American dialectology. Approximately 800 speakers were interviewed, and over half of these interviews were subjected to acoustic analysis. The results are now being compiled in an *Atlas of North American English: Phonetics, phonology and sound change* (Labov, Ash & Boberg, in press), which will contain maps representing the geographic distribution of the variants of each variable, together with analyses of the sound changes now in progress in North American English, and a taxonomy of North American English dialects based on acoustic phonetic data.

TELSUR relies on three complementary sets of data in its assessment of the phonemic contrasts present in each dialect. One is an acoustic analysis of the phonetic realization of each potential phoneme in spontaneous speech. The second is both acoustic and auditory analysis of minimal pairs in which the speaker is asked to say two words that differ only with respect to the contrast in question, thereby eliminating all possible contextual influences on phonetic production. The third set of data comes from speakers' judgments of the contrast involved in each elicited pair: do the words sound the same or different; do they rhyme or not? The most obvious comparison to be made, then, is between the data representing the analyst's observations of a particular phonemic contrast (the speaker's *production*) and the speaker's judgments of the same contrast (the speaker's *perception*).

5. ANALYST OBSERVATIONS VS. SUBJECT INTUITIONS IN MINIMAL PAIRS. Table 1 (overleaf) shows data obtained from interviews with over 700 subjects across North

Perception (subject judgment)	Production (analyst judgment)	cot/caught		Don/dawn		pin/pen		full/fool	
same	same	217	30%	292	41%	181	25%	53	7%
close	same	13	2%	18	3%	6	1%	4	1%
different	same	12	2%	6	1%	2	0%	2	0%
same	close	57	8%	25	4%	30	4%	20	3%
close	close	23	3%	39	6%	37	5%	30	4%
different	close	22	3%	22	3%	35	5%	24	3%
same	different	29	4%	21	3%	13	2%	14	2%
close	different	40	5%	31	4%	39	5%	38	5%
different	different	316	43%	253	36%	367	52%	528	74%
TOTAL		729	100%	707	100%	710	100%	713	100%
Perception ahead		126	17%	77	11%	82	12%	72	10%
Production ahead		47	6%	46	7%	43	6%	30	4%
Total disagreements		173	24%	123	17%	125	18%	102	14%

Table 1. Perception vs. production data for four minimal pairs, from sociolinguistic interviews with native speakers of North American English, tape recorded for the Atlas of North American English *(Labov, Ash & Boberg, in press).*

America, focusing on three contrasts in four minimal pairs. The first contrast is between /ɑ/ and /ɔː/, first before /t/ in *cot* vs. *caught*, and then before /n/ in *Don* vs. *dawn*. The second contrast is between /ɪ/ and /ɛ/ before /n/, in *pin* vs. *pen*. The third is between /ʊ/ and /uː/ in *full* vs. *fool*. Both the subject's and analyst's judgments of the pairs (i.e., perception and production of contrast) are classified with three terms: *same*, meaning no difference in sound; *close*, meaning a marginal, uncertain or inconsistent difference; and *different*, meaning a clear and consistent difference. This gives us nine possible combinations of subject and analyst judgments, each of which is listed in the table. These can be grouped for purposes of analysis into cases of agreement or disagreement between subject and analyst. The cases of disagreement can be further divided between cases where the analyst judges the merger to be more advanced than the subject (production ahead of perception), and cases where the analyst judges the merger to be less advanced than the subject (perception ahead of production).

The first point to be made about the data in Table 1 is that in the majority of cases, the subject and analyst *do* agree on the status of the phonemic contrast. However, the last line of the table shows that in a significant minority of cases, ranging from 14 per cent for the pair *full-fool* to 24 per cent for *cot-caught*, they do not agree. The author's personal experience in evaluating thousands of minimal pair tests for the TELSUR Project suggests that many of the disagreements arise from perfectly trivial, non-linguistic factors like distraction, boredom, fatigue, insincerity, confusion or simple error on the part of subjects. These factors nevertheless play a part in determining the confidence with which we rely on native speakers' intuitions about their grammar as

evidence in linguistics. Another important factor, contributing to the large number of 'close' judgments in the perception column, is subjects' failure or refusal to state their intuitions in appropriately precise terms. When asked if two words sound the same or not, many subjects answer something like, 'they're similar', 'they're pretty close', or 'yeah, they're pretty much the same', the exact meaning of which is often difficult to interpret.

A further set of factors that interfere with the accuracy of subjects' responses arises from the observer's paradox referred to above. Sometimes one or both of the sounds involved in a minimal pair is the target of negative social evaluation, causing speakers' judgment of contrast to reflect their perceptions of the evaluative norms of the speech community rather than their own usage. Another effect of linguistic insecurity is that speakers who are self-conscious about their level of education may claim that there is a difference in sound between two words simply to demonstrate their knowledge that the words are spelled differently. Many subjects begin their response to a minimal pair question with, 'well, they're not spelled the same…' Moreover, to a self-conscious subject, it may seem safer to claim an ability to discriminate a small difference between two things even if it may not be there, than to admit an inability to discriminate a difference that is potentially present.

6. THE NATURE OF ANALYST-SUBJECT DISAGREEMENTS ABOUT PHONEMIC CONTRAST. If the factors just enumerated were the only ones operating to produce subject-analyst disagreements about phonemic contrast, we would expect to find more or less random patterns of disagreement, and more or less similar rates of disagreement in each region. A closer analysis of the data from Table 1 shows that this expectation is not supported, and that a better understanding of the nature of asymmetries between perception and production can help us to assess the risks involved in relying on native-speaker intuitions in different situations.

A comparison of the frequency of disagreements in which perception is ahead of production to the frequency of the opposite type of disagreement, production leading perception, suggests that the relationship between these frequencies is not random. In all four cases, involving three different mergers that affect different parts of North America, it is more common for perception to be ahead of production than the opposite. The bias in favor of perception leading production ranges from under 60% in the case of *Don* and *dawn* to a ratio of almost three to one in the case of *cot* and *caught*. This state of affairs is the opposite of the prediction that would follow from the above hypothesis that subjects feel safer claiming to hear doubtful distinctions than admitting that they cannot hear a difference between two words. In fact, subjects are on average twice as likely to fail to report a difference in production noted by the analyst, as to claim that they make a distinction that the analyst cannot hear. In other words, mergers appear to advance more quickly in perception than in production.

These data support the observations of Labov (1994:310–70) that, as a general principle, socially and geographically expanding mergers affect the perceptual status of a contrast before they affect its productive status. Herold (1990) called this

phenomenon a merger by expansion. Contrary to the intuitive supposition that mergers represent a loss of information because words that were once distinguished can no longer be told apart, Herold's study of the merger of the vowels in *cot* and *caught* in Eastern Pennsylvania (also discussed in Labov 1994:321–24) caused her to suggest that, from the point of view of speakers who have historically maintained a distinction but who have recently come in contact with an expanding merger, mergers in fact represent a gain of information. This is because, under conditions of dialect contact, when speakers who themselves maintain a distinction hear the merged production of other speakers, they are liable to misunderstand the speakers with a merger as they try to match the phonetic production associated with a single phoneme to their own two-phoneme perceptual model. Speakers with a merger, by contrast, rely on contextual cues rather than sound differences to tell the two word classes apart. Mergers are irreversible (Labov 1994:311): once a speaker has lost the lexical distinction between two word classes, it cannot be reacquired except by a process of rote memorization that resembles second language acquisition (like English-speakers learning gender in French or German). Therefore, the only way to resolve the problem of misunderstandings is for the members of the community who still maintain a distinction to abandon that distinction at the perceptual level: to stop listening for a difference in sound and rely on contextual cues instead, like speakers with a merger. Once they have done so, misunderstandings diminish, which represents a gain of information. This merger in perception, of course, represents the first stage of an eventual complete merger in the affected community. The speakers with a perceptual merger will retain the distinction they acquired as children in their own production, leading to the sort of asymmetry reflected in some of the results of Table 1, but *their* children will likely exhibit a complete merger in both production and perception.

The observation that perception-only mergers are more common than production-only mergers in the data of Table 1, and the association of perception-only mergers with the geographic diffusion of mergers in progress, suggests a further approach to these data. We might ask whether subject-analyst disagreements are equally common in all regions of North America, or whether they are concentrated in regions where mergers are known to be in progress. For an answer, we will focus our analysis on the pair from Table 1 that shows the highest rate of disagreement: *cot* vs. *caught*. A regional breakdown of response types for this pair is given in Table 2.

Table 2 lists five dialect regions chosen for the diversity of their status with respect to the merger of *cot* and *caught*. The Inland North is the belt of industrial cities around the American side of the Great Lakes, such as Chicago, Detroit, Cleveland and Buffalo. The Mid-Atlantic region extends from New York City down to Philadelphia and Baltimore. The West refers to the Mountain and Pacific Coast regions of the United States, including cities like Denver, Phoenix, Los Angeles and Seattle. Canada is self-explanatory. The Midland, for purposes of this analysis, is the strip of territory that extends between the Inland North and the South, from Columbus, Cincinnati and Indianapolis to St. Louis and Kansas City. TELSUR's research has established that the Inland North and Mid-Atlantic regions exhibit a stable and consistent distinction

Perception (subject judgment)	Production (analyst judgment)	Inland North		Mid-Atlantic		West		Canada		Midland	
same	same	0	0%	1	3%	64	74%	25	69%	28	22%
close	same	0	0%	0	0%	3	3%	3	8%	1	1%
different	same	0	0%	0	0%	0	0%	2	6%	5	4%
same	close	0	0%	0	0%	9	10%	5	14%	16	13%
close	close	0	0%	0	0%	4	5%	0	0%	11	9%
different	close	0	0%	0	0%	2	2%	1	3%	8	6%
same	different	0	0%	1	3%	0	0%	0	0%	8	6%
close	different	6	9%	1	3%	1	1%	0	0%	11	9%
different	different	64	91%	36	92%	4	5%	0	0%	40	31%
TOTAL		70	100%	39	100%	87	100%	36	100%	128	100%
Perception ahead		6	9%	2	5%	10	11%	5	14%	35	27%
Production ahead		0	0%	0	0%	5	6%	6	17%	14	11%
Total disagreements		6	9%	2	5%	15	17%	11	31%	49	38%

Table 2. *Perception vs. production of the contrast between* cot *and* caught*, by dialect region, from sociolinguistic interviews with native speakers of North American English, tape recorded for the Atlas of North American English (Labov, Ash & Boberg, in press).*

between *cot* and *caught*, and the West and Canada an equally solid merger. The status of the distinction in the Midland, however, is much less clear. The major Midland cities are historically distinct, but several of them appear to be in the midst of a merger in progress, with older speakers maintaining a distinction and younger speakers losing or having lost it. This was found to be the case in Cincinnati, for instance, by Boberg and Strassel (1995).

The regional analysis of Table 2 shows that there is a fairly clear relationship between the status of the merger and the frequency of subject-analyst disagreements. In three out of the four areas where phonemic contrast enjoys a stable, consistent status as either present or absent, disagreements are relatively infrequent, below the continental average of 24 per cent. This is particularly true where a solid distinction is maintained: minimal pair tests in the Inland North and Mid-Atlantic regions produce only nine and five per cent disagreements, respectively. Ask a speaker in Detroit or Philadelphia whether *cot* and *caught* sound the same, and you will very likely get a clear distinction in both production and perception. In fact, speakers in these areas are sometimes puzzled by the purpose of the question, and cannot imagine how the two words could possibly sound the same.

Regions that are known to have stable and consistent mergers show much less certainty in judgment than regions with a solid distinction. It is not clear at this point why this should be so, unless it relates to the feeling of insecurity, mentioned above, that would lead speakers to claim that there is a difference in sound between two words because they know they are spelled differently, in order to avoid seeming

uneducated. Disagreements are relatively uncommon in the West, below the continental average, but are surprisingly frequent in Canada, which may suggest that the Canadian merger is less recent than has commonly been supposed.

The region with the highest frequency of disagreements, however, is clearly the Midland, at 38 per cent: more than a third of Midland subjects report intuitions about phonemic inventory that do not agree with the observations of the analyst. Given the large number of Midland subjects in the sample, this suggests that much of the subject-analyst discrepancy evident in the overall data was due to this single region.

7. CONCLUSIONS. Two conclusions can be drawn from the preceding observation. The first is that, in situations where the phonemic inventory is stable, native speakers' intuitions about it are in fact fairly reliable. The second is that, where the phonemic inventory is undergoing change, speakers' intuitions about it are often at odds with the facts observed by a linguist. The causes of disagreement in the latter circumstances go well beyond trivial factors like attention and sociolinguistic factors like linguistic insecurity: they clearly involve a genuine confusion on the part of many subjects about the status of an element of linguistic structure that is subject to variation and change in their community. The very high frequency of subject-analyst disagreements in the Midland is clearly problematic for a linguistic methodology that depends on native speaker intuitions as its primary source of evidence. These data show that while it may sometimes be necessary or even desirable to turn to native speakers' intuitions as evidence in linguistics, such evidence should always be interpreted with caution and even skepticism, and should be checked against empirical data whenever possible.

We have focused here on the question of phonemic contrast, but it is not hard to imagine how the conclusions of this study would extend to other questions and levels of linguistic structure. Where elements of morphology or syntax are diachronically unstable, speakers' intuitions about the grammaticality of those elements are likely to be equally unreliable. The unreliability, as indicated above, may arise from linguistic insecurity and awareness of the negative social connotations of certain ways of speaking, or from simple confusion and uncertainty caused by a mixture of competing grammars in the speaker's environment.

We shall leave the last word on this subject to one of TELSUR's informants, who illustrates brilliantly the confused judgments that are often produced by dialect mixture. What he says in the transcript that follows may seem amusing to English-speaking readers, because as English speakers ourselves we have access to a set of facts that clearly contradict his misinterpretation of variation in the lexical incidence of a phoneme as the basis of a semantic distinction. However, one can only imagine how much more difficult it would be to avoid being seriously misled by this sort of evidence, if one were studying a language one knew relatively little about, in a foreign culture. The speaker is a 35-year-old truck driver (TELSUR subject no. TS 116) from south-central Michigan, a traditionally Inland Northern region that experienced, in the decades after World War II, an influx of Midland and Upper Southern migrants looking for work in Northern factories, and therefore exhibits dialect mixture of the

sort we are interested in. The interviewer (the author) asks the subject to give a synonym for *pigs*, in an attempt to gather data on his pronunciation of the word *hogs*. This pronunciation varies regionally, along with that of many other words containing <o> before /g/, between the /ɑ/ of *cot* and the /ɔ/ of *caught*. In the transcript that follows, we will write *hog* for the former, and *hawg* for the latter pronunciation.

Interviewer: What's another word for *pigs*?
Subject: Hogs.
I: Yeah.
S: Or hawgs.
I: Right. Now which, how do you say that word?
S: Depends on what kind; which one you're talkin' about. Uh, there's some kinds that they, they pronounce 'em as hawgs, some kinds they, they pronounce as hogs. It's like a different breed. Neighbor down the street, he's a hog farmer, and he calls 'em hogs. Okay? Now, he also says there's a breed that they call hawgs. And hogs.
I: Are there different places where these breeds come from?
S: Yup.
I: Which one's which?
S: Uh, I don't know.

REFERENCES

BOBERG, CHARLES & S. STRASSEL. 1995. Phonological change in Cincinnati. *Penn working papers in linguistics* 2:25–35.

CHESHIRE, JENNY. 1978. Present tense verbs in Reading English. In *Sociolinguistic patterns in British English*, ed. by Peter Trudgill, 52–68. London: Edward Arnold.

ECKERT, PENELOPE. 1988. Adolescent social structure and the spread of linguistic change. *Language in society* 17:183–207.

HEROLD, RUTH. 1990. *Mechanisms of merger: the implementation and distribution of the low back merger in eastern Pennsylvania*. Unpublished University of Pennsylvania dissertation.

LABOV, WILLAM. 1966. *The social stratification of English in New York City*. Washington D.C.: Center for Applied Linguistics.

——. 1972. *Sociolinguistic patterns*. Philadelphia: University of Pennsylvania Press.

——. 1991. Three dialects of English. In *New ways of analyzing sound change*, ed. by Penelope Eckert, 1–44. New York: Academic Press.

——. 1994. *Principles of linguistic change, vol. 1, Internal factors*. Oxford: Blackwell.

——, SHARON ASH & CHARLES BOBERG. in press. *Atlas of North American English: phonetics, phonology and sound change*. Berlin: Mouton.

TRUDGILL, PETER. 1974. *The social differentiation of English in Norwich*. Cambridge: Cambridge University Press.

~

**WINNER OF THE PRESIDENTS'
2001 POST-DOCTORAL PRIZE**

~

PRESIDENTS' POST-DOCTORAL PRIZE

The Presidents' Post-Doctoral Prize is awarded annually to the lecture judged to make the greatest contribution to linguistic knowledge by an author who has completed a doctorate within the preceding ten years but who does not yet have faculty tenure. The judging panel consists of the current LACUS President and Vice President along with all past presidents in attendance at the meeting.

EVIDENCE IN LINGUISTICS:
THREE APPROACHES TO GENITIVES IN ENGLISH

STEFAN TH. GRIES
Southern Denmark University

OVER THE PAST SEVERAL DECADES[1], syntactic research has been dominated by generative linguistics, the main research technique of which are well-formedness judgements of sentences[2]. The resulting methodological difficulties and points of criticism have been amply documented in, e.g., Cowart (1997) and Schütze (1996). However, given the large amount of harsh criticism that is frequently directed at naïvely gathered acceptability judgements (usually accompanied by commitments to corpus analyses; cf. many references cited in Schütze 1996 and contemporary cognitive-linguistic and/or functionalist studies), it is surprising to see that there is only a relatively small number of studies that explicitly compare the ways in which different methodologies yield different (kinds of) data. Since my point is mainly methodological in nature, I decided to investigate a phenomenon that has already been thoroughly studied, namely what I will, for ease of exposition, refer to as the English genitive alternation.

(1) a. the speech   of   the President
     b. $NP_{Possessed}$ of $NP_{Possessor}$ (= *of*-genitive)
(2) a. the President   's   speech
     b. $NP_{Possessor}$'s $NP_{Possessed}$[3]   (= *s*-genitive)

Many variables influencing native speaker's choices of constructions have been identified (cf., e.g., Altenberg 1982; Leech, Francis, and Xu 1994; and especially Stefanowitsch 1997 for overviews); for practical purposes, I will concentrate on three only, namely:

- the syllabic lengths of $NP_{Possessor}$ and $NP_{Possessed}$ (cf., e.g., Poutsma 1914) such that short NPs tend to precede long NPs (to be represented as short » long);
- the animacy of the two NPs' referents (cf., e.g., Poutsma 1914; Jespersen 1949; Hawkins 1981);
- the (discourse-)givenness of the referents of the two NPs such that NPs encoding given referents tend to precede NPs encoding new referents (to be represented as given » new; cf., e.g., Altenberg 1980, 1982; Standwell 1982).

The different kinds of data to be discussed are:

(i)   intuitions from informed linguists representing the generative approach, where it often seems that the only informant is the investigating linguist himself;

	Spoken data	Written data	Row totals
of-genitives	75	75	150
s-genitives	75	75	150
Column totals	150	150	300

Table 1. Composition of the sample of corpus data.

(ii) corpus data (both spoken and written);
(iii) acceptability judgements from linguistically naïve native speakers.

First, I will show how the results of the different kinds of data relate to one another. Second, I will show how, counter to popular reasoning, syntactic research benefits from the investigation of both *carefully elicited* judgements and *balanced* corpus data. Note once more that the focus is not on finding out something (new) about the genitive constructions—the analysis of the genitive, whatever results it may yield, is merely a means of making a methodological point.

1. METHODS.

1.1. INFORMED LINGUISTS. In order to obtain informally-gathered intuition data from informed linguists, I presented several linguists with the variables' proposed effects and some example sentences and asked them, on the basis of their intuitions as linguists and native speakers, to formulate generalizations

- concerning the power of the variables in determining the choice of construction;
- concerning the (frequency) distribution of the particular features under investigation and the existence of genitive types that are defined by significant co-occurrences of particular variables' values.

1.2. CORPUS DATA. Using MonoConc Pro 2.0, the pseudo-random sample of genitive constructions given in Table 1 was drawn from the British National Corpus (BNC, first edition). Each instance of a genitive was coded with respect to the above variables, that is the syllabic lengths of the two NPs, the (degrees of) animacy of the referents of the two NPs, and the discourse-givenness of the referents of the two NPs[4]. On the resulting data, I carried out a multifactorial ANCOVA in order to (i) estimate each variable's impact on the choice of the genitive and (ii) investigate the expected two-way interactions of variables and the genitive construction (cf. section 2.2.1)[5]. Also, I determined the most significant clusters of variables describing typical genitives (cf. section 2.2.2).

1.3. ACCEPTABILITY JUDGEMENTS. Given the six variables (three for $NP_{Possessor}$ and three for $NP_{Possessed}$) to be analysed, I developed a factorial token set (using Cowart's 1997: 48f. terminology), as shown in Table 2. Thus, for a fully factorial set, 2×4×4×3×3=288

Variable	Levels
Genitive	*of* vs. *s*
Animacy of the $NP_{Possessor}$ ($A_{Possessor}$)	human, animate+non-human, concrete+inanimate, abstract
Animacy of the $NP_{Possessed}$ ($A_{Possessed}$)	human, animate+non-human, concrete+inanimate, abstract
Length of $NP_{Possessor}$ ($L_{Possessor}$) relative to length of $NP_{Possessed}$ ($L_{Possessed}$)	$L_{Possessor} > L_{Possessed} +1$ $L_{Possessor} = L_{Possessed} \pm1$ $L_{Possessor} < L_{Possessed} -1$

Table 2. Independent variables manipulated in the questionnaire.

individual tokens had to be developed. To that end, I combined each of the genitives with each degree of animacy of both $NP_{Possessor}$ and $NP_{Possessed}$ and systematically varied the lengths of the two NPs as well as their referents' degrees of givenness (by means of a sentence preceding the sentence with the genitive to be judged)[6]. In order to increase the likelihood of representative results, various controls were implemented. For example, since the frequency of linguistic elements can distort the results, the frequency of the nouns figuring in the genitives was controlled for by only admitting the 2.5% most frequent words of English (according to the Cobuild electronic dictionary E-Dict). Also, in order not to base the interpretation of the results on a single token set (results might then be due to individual lexical items only), a different though analogously designed token set was developed, yielding a total of 576 experimental items. Then, the list of experimental items was interspersed with 576 filler items of other syntactic constructions with varying degrees of acceptability. The questionnaire was standardised such that each subject received a different set of randomly ordered stimuli and fillers and the required judgement process was explained and exemplified. This included that the scale of grades to be used by the subjects was anchored only at its endpoints (cf. Schütze 1996: 189, n. 12; Cowart 1997:71).

The subjects that participated in this experiment voluntarily were all native speakers of English without training in linguistics and unaware of the exact purpose of the analysis. The resulting acceptability ratings were then analysed using AN(C)OVAS in order to determine how each variable's two-way interaction with the construction influences (or fails to influence) the acceptability ratings.

2. RESULTS AND DISCUSSION.
2.1. INFORMED LINGUISTS. As to the first question (the degree to which the variables analysed influence the choice of construction), the results are fairly heterogeneous. The following rank-orderings of variables were obtained[7]:

(3) a. $A_{Possessor} > A_{Possessed} > (L_{Possessor} \approx L_{Possessed} \approx D_{Possessed} \approx D_{Possessed})$
      b. $D_{Possessor} > A_{Possessor} > L_{Possessor} > D_{Possessed} > (A_{Possessed} \approx L_{Possessed})$
      c. $(A_{Possessor} \approx A_{Possessed}) > (D_{Possessor} \approx D_{Possessed}) > (L_{Possessor} \approx L_{Possessed})$
      d. $A_{Possessor} > (L_{Possessor} \approx D_{Possessor}) > A_{Possessed} > (L_{Possessed} \approx D_{Possessed})$

Two tendencies can be observed: First, animacy is fairly consistently considered to be among the most important determinants of the constructional choice. Second, $NP_{Possessor}$ is, on average at least, considered to be important and $NP_{Possessed}$ is not. Note also, however, that there is an interaction such that if $NP_{Possessed}$ is important, then it is only in terms of its degree of animacy. On the whole, however, the results are heterogeneous: there is no consistent ranking of variables or NP kinds and we find that variables equated by some linguists are not equated at all by others.

As to the second question (the frequency distribution of features co-occurring [frequently/significantly]), the results were fairly homogeneous. Consider (4) and (5) for the feature clusters (for *of*-genitive and *s*-genitive respectively) claimed to be prominent (blanks indicate that the respective variable was not included in the expected significant type by the informants).

(4) a.   animate $NP_{Possessed}$
      b. long   new $NP_{Possessed}$
      c.   new   abstract $NP_{Possessed}$
(5) a. short   animate $NP_{Possessor}$
      b. short   given   (animate) $NP_{Possessor}$
      c. short   given   human $NP_{Possessor}$
      d.   inanimate $NP_{Possessed}$

These proposals as to frequency distributions of feature clusters also yield interesting results. First, linguists' estimations concerning the *s*-genitive and the *of*-genitive focussed on $NP_{Possessor}$ and $NP_{Possessed}$ respectively. This is somewhat surprising since both genitives obviously consist of $NP_{Possessor}$ and $NP_{Possessed}$, and I do not know how to explain this unanimous focus on one NP in each construction. Second, possessors in *s*-genitives are in general considered to be short, given, and animate (thus supporting the predictions of given » new and short » long). On the other hand, $NP_{Possessed}$ in *of*-genitives is supposed to be long and new (with disagreement concerning animacy). This, however, ties in with the predictions concerning $NP_{Possessed}$ of the *s*-genitive since a long and new $NP_{Possessed}$ in *of*-genitives violates both short » long and given » new. In other words, giving even such a simple constellation of variables and expected effects to experienced linguists seems to pose computational problems such that the subjects ultimately failed to account for the predicted two-way interaction and produced unexpected and contradictory predictions. Finally, the results of both the variable ranking and the expected feature clusters do coincide to some extent in that both strategies lead us to expect that $NP_{Possessor}$ is more important than $NP_{Possessed}$.

$A_{Possessed}$ ╲ $A_{Possessor}$	abstract		concrete		human		Row totals		
	of	s	of	s	of	s	of	s	total
abstract	80 $(^+)$	37 $(^-)$	9	8	3	2	92	47	139
concrete	22 $(^+)$	0 $(^-)$	20 $(^+)$	1 $(^-)$	0	0	42	1	43
animate + human	9 $(^{---})$	58 $(^{+++})$	1 $(^{---})$	35 $(^{+++})$	6	9	16	102	118
Column totals	111	95	30	44	9	11	150	150	300
	206		74		20				

Table 3. Genitives relative to animacy in the corpus data (as a (3 × 3) × 2 table)[9].

2.2. CORPUS DATA

2.2.1. VARIABLE STRENGTHS. As a first step, before we look at the individual variables' effects, let us look at whether the variables singled out for attention correlate with the choice of genitive constructions in the data in any way worth mentioning. Without belabouring statistical technicalities, the overall correlation is fairly high and highly significant, showing that the variables included in the analysis indeed contribute strongly to the alternation[8].

Let us first look at the impact of animacy on the choice of genitive constructions. Consider Table 3, which provides the frequencies of each genitive construction depending on $A_{Possessor}$ and $A_{Possessed}$. The distribution of constructions is, as can be easily seen, different from chance (R_{mult}=.64; $F_{7, 292}$=29.3; p<.001). The cells responsible for this effect contain plusses/minuses (depending on whether the observed frequency is higher/lower than the expected one), the numbers of plusses/minuses indicate the significance level of the cells' deviations from the expected frequencies as determined by a configural frequency analysis (cf. Krauth 1993).

On the level of row and column totals, two results are immediately obvious: first, animate/human $NP_{Possessed}$s are rare and the more human an NP's referent is, the less likely it is to occur as $NP_{Possessed}$. That humans are rarely $NP_{Possessed}$ is, on the one hand, not surprising, given how we conceptualise possession (cf. Taylor 1995:202ff., 1996). On the other hand, it is interesting to note in passing that 206 out of 300 genitive constructions (nearly equally *of*- and *s*-genitives) have an abstract entity as $NP_{Possessed}$ rather than a concrete object (as would be expected from such prototype-based approaches to possession and genitives in English). No similarly clear bias, however, can be observed for $NP_{Possessor}$: animate and human possessors occur often (though abstract possessors are most frequent) and concrete possessors occur only rarely.

Let us finally turn to significant individual (pairs of) cells and, thus, two distinct usage patterns of genitive constructions. On the basis of the data, two significant patterns of genitive usage can be identified.

(6) $NP_{abstract}$ of $NP_{abstract}$ / $NP_{concrete}$
(7) $NP_{animate/human}$'s $NP_{abstract/concrete}$

On the one hand, the *of*-genitive is significantly preferred when both NPs are abstract, when both NPs are concrete, and when the $NP_{Possessor}$ is concrete and $NP_{Possessed}$ is abstract. On the other hand, the *s*-genitive is preferred when $NP_{Possessor}$ is animate/human. These patterns are so strong that, once we know what $NP_{Possessor}$ looks like, we can predict (92+42+102) 236 out of 300 (78.7%) genitive constructions correctly. $NP_{Possessed}$, however, does not play a prominent role when it comes to deciding on a construction.

Space limitations do not permit detailed inspection of the corpus data with respect to the types just mentioned (cf., however, section 2.2.2 below), but a brief comment will serve to indicate the ways an analysis could be continued rewardingly. One such possibility is the analysis of semantic relations between the two NPs involved: The pattern in (6) admits a variety of semantic relations between $NP_{Possessor}$ and $NP_{Possessed}$ such as attribute/holder of attribute, part/whole, etc.; (cf. Stefanowitsch (1997) for an illuminating inventory of relations and their distribution) whereas the semantic relations of the pattern in (7) are most often that of possessor/possession, agent/action and attribute/holder of attribute. That is, even a cursory glance at real data shows the implausibility of assuming that the two constructions are synonymous or used interchangeably; this implies that, at least on the basis of our data, there is no need to derive one construction from the other in any way whatsoever.

The next variable to be investigated is concerned with the syllabic lengths of the two NPs involved in the genitive. According to previous studies, we would expect to find a two-way interaction between the NP ($NP_{Possessor}$ vs. $NP_{Possessed}$) and the genitive construction (*of*-genitive vs. *s*-genitive) such that short » long. A 2-way ANOVA, however, shows that the overall correlation between the kinds of NP and genitives is significant ($F_{3,\,596}$=2.97; p=.031), but not in ways we would expect:

- there is a significant main effect such that the two genitives differ with respect to the average lengths of the NPs: *of*-genitives are formed out of longer NPs than *s*-genitives ($F_{1,\,596}$=7.14; p=.008);
- the predicted two-way interaction is insignificant ($F_{1,\,596}$=.7; p=.405) and the observed tendency is even in the opposite direction of what syntactic-weight approaches would predict; cf. the left part of Figure 1.

That is to say, approaches to the genitive placing a strong emphasis on heaviness of constituents are not supported by the data, a result I found somewhat astonishing. But before we jump to conclusions too hastily, recall that many analyses of corpus data are based on written data only – the present corpus, however, is balanced with respect to the medium so we can easily filter out this effect. Consider Table 4, where (within each medium and across all examples) for each construction the average lengths of $NP_{Possessor}$ and $NP_{Possessed}$ are compared.

A 3-way ANOVA including the medium (spoken vs. written) yielded two results worth further discussion. First, the analysis revealed that the NPs in the written part of the corpus are on average significantly longer ($F_{1,\,592}$=11.96, p<.001). Second, and

	oral		written		total	
	of-genitive	*s*-genitive	*of*-genitive	*s*-genitive	*of*-genitive	*s*-genitive
NP$_{Possessor}$	3.7	3.1	4	3.9	3.8	3.5
NP$_{Possessed}$	3.1	3	4.5	3.2	3.8	3.1

Table 4. *Average NP syllabic lengths of the genitives in the corpus data.*
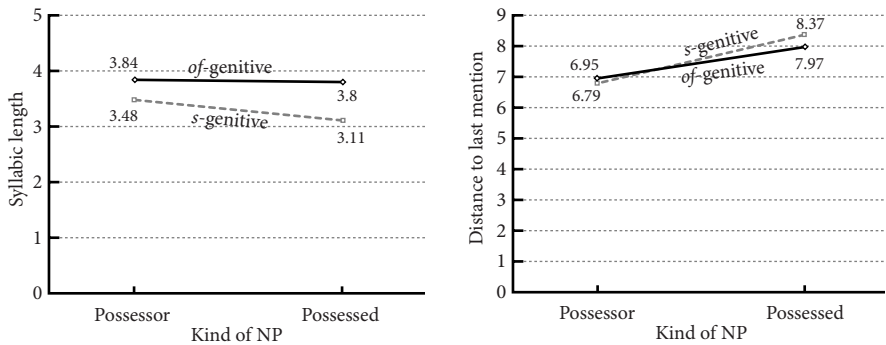
Figure 1. *Interaction plots: (Genitive × NP) for lengths (left) and (Genitive × NP) for DTLM (right).*

more interestingly, there is a significant 3-way interaction ($F_{1, 592}$=4.48, p<.034) such that:

- for the written data, the two-way interaction is even more in the unexpected direction;
- for the oral data, the two-way interaction is nearly as expected: with *of*-genitives, NP$_{Possessor}$ is longer than NP$_{Possessed}$ – with *s*-genitives, there is practically no difference.

That is to say, we must be careful not to leave aside medium-specific differences: the results for written and oral data diverge so strongly that the unexpected overall results may hide the expected results of the oral data, if the medium is not accounted for carefully. This is an important lesson to learn for corpus-based analyses of syntactic phenomena, especially when one tries to account for syntactic phenomena in terms of processing restrictions or similar variables where medium differences can be decisive.

Finally, let us deal with the discourse givenness of the two NPs and their effect on the choice of genitive. Consider Table 5 (overleaf), where the average values of the distance to last mention (DTLM) in clauses are given.

Again, previous studies lead us to expect a two-way interaction between NP (NP$_{Possessor}$ vs. NP$_{Possessed}$) and the genitive construction (*of*-genitive vs. *s*-genitive) such

	oral		written		total	
	of-genitive	*s*-genitive	*of*-genitive	*s*-genitive	*of*-genitive	*s*-genitive
NP$_{Possessor}$	5.9	7.3	8	6.3	6.9	6.8
NP$_{Possessed}$	7.2	9.1	8.7	7.6	8	8.4

Table 5. Average DTLM scores of the NPs in the genitives of the corpus data

that given » new. Again, however, the 3-way ANOVA (this time including the medium right from the start), though highly significant ($F_{7, 592}$=7.7; p<.001), shows that:

- the two genitives differ with respect to the average givenness of their NPs such that the average NP$_{Possessor}$ is more given than the average NP$_{Possessed}$ ($F_{1, 596}$=20.57; p<.001);
- the predicted two-way interaction is not significant ($F_{1, 596}$=.95; p=.329 ns), but the observed tendency is indeed in the predicted direction; cf. the right part of Figure 1.

While the second result is easy to account for (since it is, though non-significant, at least in the correct direction) I find it difficult to account for the first one. An explanation might be that we simply speak about possessors more often since, as we have seen above, they tend to be human. If we speak about them more often, then of course the distance between the different occasions on which we refer to them are closer to one another, resulting in the observed main effect of DTLM. It remains to be seen to what extent the analysis of the acceptability judgements can shed light on this issue.

2.2.2. TYPES OF GENITIVES AS DETERMINED BY SIGNIFICANT FREQUENCY. While the previous section has investigated each variable on its own, let us now look at the genitive types defined by significant feature clusters of all variables simultaneously. While the overall number of significant types (as determined by a hierarchical configural frequency analysis) is too large to be discussed in detail, the most important types for *of*-genitives and *s*-genitives are given in (8) and (9) respectively (the interval variables [length and DTLM] were dichotomised on the basis of their arithmetic mean within each register).

(8) a. NP$_{concrete}$ new   short   of   NP$_{concrete}$ new
     b. NP$_{abstract}$ new   of   NP$_{human/animate}$ short
(9)   NP$_{human/animate}$ short   's   NP$_{concrete}$ short

On the whole, the types already obtained by the analysis of A$_{Possessor}$ and A$_{Possessed}$ alone are supported—given the above corpus results on length and givenness, it is not surprising to see that the identifiable types do not unanimously support the expected tendencies (short » long and given » new).
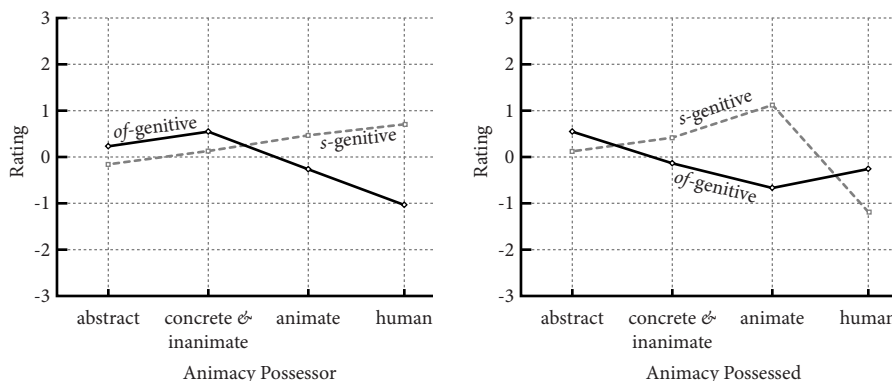
Figure 2. *Interaction plots: Genitive × $A_{Possessor}$ (left) and Genitive × $A_{Possessed}$ (right).*

2.3. ACCEPTABILITY JUDGEMENTS. As we have seen, the corpus analysis yielded fairly heterogeneous results such that some previous accounts were supported whereas some tendencies one might have taken for granted were not. Let us now turn to the results of the survey and find out to what extent the results fit together. First, again, the overall correlation between all variables and the choice of construction is highly significant[10]. We will now proceed in the same order as for the corpus data and start with $A_{Possessor}$.

While no significant main effect can be found, for the two-way interaction between the genitive and $A_{Possessor}$, we obtain a clear and significant pattern ($F_{3, 499}$=7,15; p<.001): animate and human possessors are preferred in the *s*-genitive whereas abstract as well as concrete and inanimate possessors are preferred in the *of*-genitive. These findings are virtually identical to and, thus, strongly support the results obtained in and interpretations derived from the corpus analysis (cf. the left part of Figure 2)[11].

A somewhat different picture emerges from the analogous analysis of $A_{Possessed}$. First, there is a significant main effect showing that the more human $NP_{Possessed}$ is, the less acceptable are both constructions ($F_{3, 498}$=6.18; p<.001), an effect we are already familiar with from the corpus data. More importantly, however, is the (significant) two-way interaction ($F_{3, 498}$=9.09; p<.001) between the genitive constructions and $A_{Possessed}$. In the corpus analysis, $NP_{Possessed}$ does not differentiate between the two constructions. The results of the acceptability judgements support these results for abstract and concrete $NP_{Possessed}$, which again obtain virtually identical ratings in both constructions. However, animate $NP_{Possessed}$ are preferred in *s*-genitives, whereas human ones are preferred in *of*-genitives. This is interesting in two respects: first, it shows that there is a strikingly high general coincidence of corpus and judgement data. Second, it shows that, where the corpus data have not provided relevant information (recall no cases of animate $NP_{Possessed}$ were found), the judgement data help us to describe the constructional preferences in such cases (cf. the right part of Figure 2)[12]. (Space does not permit the discussion of the marginally significant interaction Genitive × $A_{Possessor}$ × $A_{Possessed}$.)

Let us now turn to the lengths of NPs and degrees of givenness of the NPs' referents. In sum, the results of the questionnaire study again support those of the corpus analysis: while there is a tendency in the direction of short » long, the two-way interaction between the length of the two NPs and the construction clearly fails to reach standard levels of significance ($F_{2, 500}$=1.53; p=.72). Thus length does not seem to play a role in the choice of construction. With distance to last mention the situation is slightly different: the interaction between the distances to last mention and the construction is significant ($F_{2, 500}$=3.28; p=.039), such that there is a tendency of given possessors to be preferred in the *s*-genitive. This is, however, only a tendency, as post hoc tests (Scheffé) reveal no significant differences between the six arithmetic means (as opposed those of $A_{Possessor}$ and $A_{Possessed}$). However, we still need to test whether the main effect noted above (the average $NP_{Possessor}$ is less given than the average $NP_{Possessed}$) has been verified experimentally. In accordance with the corpus data, there is in fact an non-significant tendency in this direction ($F_{2, 500}$=1.81; p=.17): *s*-genitives are preferred when $NP_{Possessor}$ is more given than $NP_{Possessed}$. Thus, while the two kinds of results are as yet inconclusive, the *a posteriori* hypothesis I proposed above could at least be explanatorily adequate.

3. CONCLUSION.

3.1. INTERIM SUMMARY. The intuitions of informed linguists did not convey a unified picture: while we find agreement between the importance of variables and NP types, we obtain contradictory results for the frequent/typical clusters to be expected. The results of the corpus analysis are highly heterogeneous in how the results relate to previous approaches or more general predictions, something often found once natural data are analysed. Still, though, the corpus data have proven useful in several respects: variables could be weighted according to their importance for the alternation, it was possible to identify constructional types, and we saw how the neglect of medium differences can influence (not to say, distort) the results. On the whole, the corpus data correspond to the experimental acceptability judgement data. For most of the variables, virtually complete overlap between the kinds of results was found and, in the case of $A_{Possessed}$, the judgement data even add precision to the corpus findings.

Let us now turn to the more central question, that of how these results relate to the linguists' intuitions? On the positive side, we find that the informants' expectation as to the relevance of the variables was, though far from unanimous, accurate, at least to some degree: $A_{Possessor}$ is indeed the strongest variable determining the choice of construction. Also, the intuitions that (i) $NP_{Possessor}$ of *s*-genitives would frequently be animate/human and short as well as (ii) $NP_{Possessed}$ of *of*-genitives would frequently be new (counter to discourse-functional predictions!) are borne out by the corpus data. On the (I believe somewhat stronger) negative side, however, we find that, on the whole, the linguists failed to predict:

- the complete overall irrelevance of length and givenness to the choice of construction that was found in both the corpus data and the acceptability

judgements (recall the comparisons of means in the ANOVAs) as well as the relative irrelevance of $A_{Possessed}$ to the choice of construction;

- the relevance of the difference between animate and human $NP_{Possessed}$ found in the judgement data;
- the fact emerging from the corpus data that $NP_{Possessed}$ tends to be abstract in both constructions[13].

These results, I submit, strongly support the claim that informed linguists' intuitions on (syntactic) phenomena are inadequate. Obviously, such intuitions can serve as a good, easy-to-obtain and (at times) even accurate starting point of the analysis, but the analyst must be willing to (i) discard every single working hypothesis in the light of evidence to the contrary and (ii) integrate the more fine-grained information of corpus data and methodologically sensible questionnaire studies into his account. The following section addresses this issue in slightly more detail.

3.2. CONCLUSION (AND A GUIDELINE). Given the course of the analysis, I believe the following conclusions are warranted. On the one hand, individual intuitive data may, but need not, provide valuable insights into a phenomenon. Given the overwhelming empirical evidence pointing to potential threats to the objectivity, validity and reliability of intuition data thus obtained, however, I believe that empirically more sensible strategies are required. On the other hand, simply abandoning acceptability judgements in general seems premature, to say the least, since, once gathered in scientifically appropriate ways, they strongly coincide with or even improve on the often desired alternative of corpus data. (For a completely different study where equally refined judgement data are compared to corpus findings with similar results, cf. Gries ms.) Note especially that this coincidence of results has been found for cases where variables have turned out to be important *and* cases where variables turned out to be unimportant.

In sum, on the basis of the above results and the conclusions that can be drawn from the empirical process as such, I suggest the following strategy (of methodologically different but converging evidence) to incorporate all the above methods in a single methodology for a thorough analysis of syntactic phenomena. This strategy does not totally abandon naïvely collected judgement data, but rather treats them as a heuristic exploratory device, the implications of which are subjected to a wide array of methodologically more reliable strategies.

(i)   Collect ideas of what variables influence the phenomenon under investigation on the basis of relevant literature as well as introspective data (including people's intuitions) and formulate hypotheses;

(ii)   obtain carefully-balanced corpus data (recall the effect of the medium) relevant to the phenomenon under investigation in order to (a) perform exploratory data analysis and (b) gather evidence bearing on one's hypotheses;

(iii)   depending on the results of step (ii), conduct methodologically sound experiments (i.e. conforming to standards outlined in Cowart 1997 and Schütze 1996) on those aspects of the phenomenon for which (a) no corpus data could be obtained and/or (b) one's hypotheses were not supported[14];

(iv)   repeat steps (ii) and (iii) until you obtain mutually confirming results or identify additional factors.

One important question remains, however: what do we do when the different strategies (e.g. corpus data and judgements) do not yield converging evidence? That is, if there is no single a priori hypothesis in support of a particular interpretation or if each of the two different results can be explained with reference to two mutually exclusive hypotheses, then which of the results (and hypotheses) should be preferred and on what grounds?

The ultimate answer to this question is probably contingent on a variety of factors (such as personal taste, preference for methods of data collection and evaluation, the willingness to admit that the contradictory results cannot be reconciled at present). I would advocate accepting the hypotheses whose supporting results have been obtained most naturally. In other words, if results from corpus data contradict results from acceptability judgements and both could be explained equally well but differently, I would always tend to accept the hypothesis supported by the corpus data: the production of linguistic utterances/texts that happen to end up in a corpus occurred under completely natural circumstances and is, thus, less likely to be subject to experimental bias than questionnaire data (and many other experimental designs). Moreover, I would in general consider corpus data to be more precise in the sense that factors such as register, prescriptive attitudes and medium can be filtered out, whereas we can never be sure to what extent they influence subjects' reactions in experimental settings (even if subjects are advised not to let such factors influence their reactions). Nevertheless, I hope (i) to have shown how, counter to common criticism, careful experimentation by means of acceptability judgement data can support our analysis of linguistic phenomena and (ii) that these findings stimulate further research of this kind.

data reached me only after my return. However, in all cases but one (where results have undergone a slight change), the results have not changed at all in the light of these additional data.

2 In general, two kinds of well-formedness judgements are distinguished, namely grammaticality judgements and acceptability judgements (i.e. judgements concerned with competence and performance respectively). My study is concerned with acceptability judgements only. However, I believe that both kinds of judgements are difficult to distinguish on a principled basis since, e.g., different versions of generative grammar do not always agree on what factor is a matter of competence or performance. For instance, the introduction of semantic concepts such as theta roles into generative grammar enables generative grammarians to claim that particular semantic phenomena (i.e. phenomena outside of the grammar) can suddenly be explained in grammatical terms.

3 I will use the expressions $NP_{Possessor}$ and $NP_{Possessed}$ throughout the remainder of the paper for expository reasons although in many cases it is not (prototypical) possession that is denoted.

4 The degree of animacy of the NPs' referents was measured using the following scale: human > animate and non-human > concrete and inanimate > abstract. The discourse-givenness of the NPs' referents was measured using the distance to last mention (DTLM) of the referent in the preceding ten clauses. For the purposes of this analysis, expressions qualified as clauses when they contained a noun phrase or a clause as a grammatical subject together with a finite verb; when they were participial or gerundival clauses (e.g., the non-italicised part in *The new rules forbid more than one to put up a sign,* a rule usually ignored); or when a new conversational turn started. However, in order not to be too overly restrictive and proceed with too little context, the following cases were not counted as clauses even if they met one or more of the above-mentioned criteria: question tags; discourse markers such as *you know*, *as it were*, *I mean*; cleft sentences and false starts.

5 We need to analyse interactions rather than main effects because of the different orders of NP types ($NP_{Possessor}$ vs. $NP_{Possessed}$) in the constructions. For example, the preference *short » long* means that possessors should be short and long in the case of *s*-genitives and *of*-genitives respectively, a paradigm case of a two-way interaction.

6 It is well-known that there are also semantic restrictions on the use of the two different genitives. While these semantic variables are not focused upon in the present study, one still needs to take them into account so as not to bias the results systematically. In order to avoid such a skewing in the data, wherever possible I preferred semantic relations between the two NPs that, according to previous corpus-based analyses (Stefanowitsch 1997, to appear), are known to occur in both genitives; such examples include possessor/possessed, component/whole, attribute/holder of attribute, location/thing at location and family relations.

7 In the representations of variable strengths in (3), '>' and '≈' mean 'is more important than' and 'is equally important as'; parentheses are used to support the grouping of similarly influential variables visually.

8 R_{mult}=.65; $F_{14, 286}$=15.33; p<.001; the analysis was an ANCOVA (Type VI sums of squares, no constant, sigma-restricted model).

9 Animate and human possessors were subsumed under a single value because there were only very few animate possessors and no animate possessed at all.

10 R_{mult}=.77; $F_{238, 268}$=1.65; p<.001; the analysis was an ANCOVA (Type VI sums of squares, no constant, sigma-restricted model).

[11] This behaviour of human and animate NP$_{Possessor}$s provides post hoc support for grouping these classes together.

[12] Note also that the acceptability judgements show that, while human and animate NP$_{Possessor}$ behave identically, human and animate NP$_{Possessed}$ do not.

[13] Also, the linguists formulated no register-/medium-specific predictions. Admittedly, I did not ask for those, but it is plausible to assume that the heterogeneity of the above results would not have been resolved by asking the linguists to include even more information in their already very heterogeneous intuitions.

[14] Needless to say, I do not advocate experiments where acceptability is the only dependent variables. Alternatives involve operation and selection tests (Quirk & Svartvik 1966), reading and reaction time studies, ambiguity tests, paraphrasing and many more.

REFERENCES

ALTENBERG, BENGT. 1980. Binominal NPs in a thematic perspective: genitive vs. *of*-constructions in 17th century English. In *Papers from the Scandinavian symposium on syntactic variation*, ed. by Sven Jacobson, 149–72. Stockholm: Almqvist & Wiksell.

———. 1982. *The genitive v. the* of-*construction: a study of syntactic variation in the 17th century English*. Lund: Gleerup.

Collins Cobuild e-dict CD-ROM. 1999. Harper Collins.

COWART, WAYNE. 1997. *Experimental syntax: applying objective methods to sentence judgements*. Thousand Oaks: Sage Publications.

GRIES, STEFAN TH. ms. Towards a corpus-based identification of prototypical instances of constructions. IFKI, Southern Denmark University.

HAWKINS, ROGER. 1981. Towards an account of the possessive constructions: NP's N and the N of NP. *Journal of linguistics* 17:247–69.

KRAUTH, JOACHIM. 1993. *Einführung in die Konfigurationsfrequenzanalyse*. Weinheim: Beltz, Psychologie-Verlags-Union.

JESPERSEN, OTTO. 1949. *A modern English grammar on historical principles,* vol. 7, *Syntax*. Copenhagen: Munksgaard.

LEECH, GEOFFREY, B. FRANCIS & X. XU. 1994. The use of computer corpora in the textual demonstrability of gradience in linguistic categories. In *Continuity in linguistic semantics*, ed. by Catherine Fuchs & Bernard Victorri, 57–76. Amsterdam, Philadelphia: John Benjamins.

POUTSMA, H. 1914. *A grammar of late modern English*, part II, *The parts of speech*. Groningen: Noordhoff.

QUIRK, RANDOLPH & JAN SVARTVIK. 1966. *Investigating linguistic acceptability*. The Hague: Mouton.

SCHÜTZE, CARSON T. 1996. *The empirical base of linguistics: grammaticality judgements and linguistic methodology*. Chicago: University of Chicago Press.

STANDWELL, G. B. J. 1982. Genitive constructions and functional sentence perspective. *International review of applied linguistics* 20:257–61.

STEFANOWITSCH, ANATOL. 1997. *On the genitive constructions in English: a cognitive linguistic approach*. Unpubl. M.A. thesis, University of Hamburg.

———. to appear. Constructional semantics as a limit to grammatical alternation: The two genitives of English. In *Determinants of grammatical variation in English¸* ed. by Günter Rohdenburg & Britta Mondorf. Berlin: Mouton de Gruyter.

TAYLOR, JOHN R. 1995. *Linguistic categorization: prototypes in linguistic theory*. Oxford: Clarendon Press.

———. 1996. *Possessives in English: an exploration in cognitive grammar*. Oxford: Clarendon Press.

∾

~

WINNER OF THE PRESIDENTS'
2001 PRE-DOCTORAL PRIZE

~

PRESIDENTS' PRE-DOCTORAL PRIZE

The Presidents' Pre-Doctoral Prize is awarded annually to the lecture judged to make the greatest contribution to linguistic knowledge by an author who has not yet completed a doctorate. The judging panel consists of the current LACUS President and Vice President along with all past presidents in attendance at the meeting.

EVIDENCE FOR FIRST LANGUAGE ATTRITION IN RUSSIAN SIGN LANGUAGE IN ISRAEL

JUDITH YOEL
University of Haifa

WHEN AN INDIVIDUAL IMMIGRATES to another country where there is a different mother tongue, he/she stands a good chance of losing some first language skills. This loss is called language attrition and has been studied in many populations of hearing people who use spoken language. Since research has illustrated beyond a doubt that sign languages are true languages in every sense of the word, the following question arises: if attrition can occur in a spoken language, why would it not occur in a sign language too? It is this question that motivated my research.

I investigated the first language attrition of Russian Sign Language (RSL) among deaf immigrants to Israel from the Former Soviet Union. This is the first systematic investigation into the attrition of sign language. Evidence for attrition is provided as observed through interference and lexical gaps. This evidence can further be broken down into three patterns: 1) attrition-related behaviour that has previously been documented in the study of oral languages, 2) attrition-related behaviour unique to sign language, and 3) non-linguistic behaviour.

1. THE COMMUNITY. This research was made possible by an unprecedented situation of mass immigration. The collapse of the Soviet Union has brought one million Jews to Israel since 1988 (Remennick 1998:445). These immigrants have changed the face of Israeli society, demographically, culturally, and linguistically. Every sixth person in Israel today has Russian as their mother tongue. Russian is the most common language after Hebrew and Arabic. Many Russian immigrants are reluctant to give up their language and culture. Their reluctance to integrate and surrounding intolerance have led to a subculture within Israeli society. This has been termed the 'self-isolation' or 'sociocultural ghettoization of Russians'. (Zilber 1977, cited in Reminnick 1998:25).

Among these newcomers are an estimated 1000 deaf people. Deaf immigrants from the former Soviet Union are in a position quite unlike the aforementioned majority of (hearing) Russian immigrants. Their situation encourages acculturation and assimilation. The small population is dispersed, with little political representation. There is a lack of RSL institutions and education of the deaf in Israel disregards RSL; there is little, if any, ethnic awareness. The limited number of employment opportunities available require Israeli Sign Language (ISL) or Hebrew, not RSL. In addition, deaf Russian immigrants bring with them a low and weak self-image, both of their community and as individuals.

2. MY RESEARCH QUESTION. I hypothesized that with little opportunity to use and maintain RSL, the second sign language, (ISL), would encroach upon the domain of the first sign language, (RSL), the outcome being first language attrition, similar to the first language attrition seen in oral languages when one language displaces another. In this study I looked for evidence of attrition and at the forms attrition took. Furthermore, I hypothesized there would be more attrition among immigrants who had been in Israel for a longer period of time and more amongst those who had immigrated as children, as opposed to adults.

3.1. THEORIES USED TO INVESTIGATE ATTRITION. These hypotheses were investigated through two theories, first, the Theory of Interference, second, the Model of Ethnolinguistic Vitality (Allard & Landry 1986).

3.2. THE THEORY OF INTERFERENCE. The Theory of Interference is one of the most widely held explanations of forgetting a language. It assumes that there will be a reduction in the linguistic system, based on language conflict. (Freed 1980, Lambert & Freed 1982). It posits interplay between the two languages, in this case RSL and ISL, and predicts that the existing patterns of L1 are modified, mapped, and reorganized in favour of L2, i.e., in the direction of the now-dominant language from the non-dominant language. Items in L1 are eventually lost, replaced by items in L2.

3.3. THE MODEL OF ETHNOLINGUISTIC VITALITY. The Model of Ethnolinguistic Vitality, as proposed by Allard and Landry (1986) predicts the degree of language loss based on sociological and psychological factors (Allard & Landry 1992). The model focuses on the group and in this case, on those variables that will contribute to the disappearance of deaf Russian immigrants as a separate entity, culturally and linguistically. Linguistically, the result is the development of the second language to the detriment of the first. This model is viewed on three levels: 1) the social level (composed of demographic, political, cultural, and economic capital), 2) the socio-psychological level (which includes interpersonal contacts, contact with the media, and educational support), and 3) the psychological level (made up of the individual's language aptitude and 'cognitive affective disposition', or in other words, the individual's opinions, judgements and assessments of their own personal situation) (ibid:171).

4. METHODOLOGY. This study focused on the lexicon, the most widely and commonly studied and most easily observed element of language attrition studies. It was limited to frequently used lexical items, based on the widely held view that high frequency vocabulary appears to be vulnerable in attrition.

Twenty deaf immigrants whose L1 is RSL were located as subjects. All were deaf from birth or medically and legally deaf who had lost their hearing at an early age. All had received a typical deaf education by Russian standards, which was residential schools for all. The subjects, eight males and fourteen females, formed a cross section of the deaf immigrant population. They ranged in age from 11–60 upon immigration,

and had been in Israel for between one and twelve years. The number of subjects is too small for statistical generalizations and this research remains within the framework of individual loss.

There were three stages. There was a preliminary self-report questionnaire followed by two experimental situations. The self-report questionnaire included demographic information and questions about attrition and attitudes toward both RSL and ISL. In addition to providing essential background information, it allowed subjects to express themselves in ways that may not have been observed in the tests. Both of the tests had been used previously in oral language attrition studies (Lambert & Freed 1982, Waas 1996). Both were applicable to sign language. In Test # 1, subjects were shown 22 large, colourful illustrations of everyday items on cards. They were instructed to sign what they saw in the picture. In Test # 2, subjects had sixty seconds in which they had to sign as many different animals as possible. Subjects signed to a native speaker of RSL and were recorded on videotape. The data was analyzed by three different professional interpreters of RSL, coming from three different parts of the former Soviet Union (to take dialect variation into account), and by an interpreter of ISL, all working independently of one another. A similar but smaller group of hearing Russian immigrants completed the same questionnaire and tests.

5.1. RESULTS. The results confirmed the hypothesis that language attrition would occur—and indeed it did. The self-report questionnaire confirmed a situation of language conflict. While RSL was dominant in domestic situations, ISL was dominant in social situations and employment. The majority of subjects admitted, some reluctantly, having experienced attrition of RSL since their arrival in Israel.

5.2. INTERFERENCE AND LEXICAL GAPS. ISL clearly interfered with RSL. For example, in signing 'polar bear', instead of signing BEAR WHITE as it is in RSL, the subject signed BEAR [OF THE] NORTH, a literal translation from ISL signed in RSL. Gaps in the lexicon also appeared, for example, with the picture of a rainbow. One subject declared that there is no sign for rainbow in RSL and proceeded to finger spell the word. Finger spelling, an orthographic representation of Russian letters spelled out on the fingers, is not considered true sign language. Another subject, having forgotten the sign for rainbow, produced an incorrect sign related to neither RSL nor ISL. The remaining subjects and the interpreters confirmed that an RSL sign for rainbow does exist.

5.3. ATTRITION-RELATED BEHAVIOUR PREVIOUSLY OBSERVED IN ORAL LANGUAGE. The deaf subjects studied exhibited behaviour previously documented in research on oral attrition. There was complete borrowing; there were blends. Compelled to produce something and lacking access to the correct form, subjects produced some sort of hybrid, often a nonsensical form. This parallels the lexical innovations recorded in oral languages. Paraphrasing was used to compensate for language loss, as was the substitution of a target word by a more general or semantically similar word or concept, e.g., TAXI for bus, BRIDGE for rainbow and KETTLE for teacup. There was

transfer based on semantic and phonological features. Phonological errors appeared an incorrectly formed signs, with the wrong hand configuration, location, movement or orientation. Subjects also confused one- and two-handed signs.

5.4. LINGUISTIC BEHAVIOUR UNIQUE TO SIGN LANGUAGE. Linguistic behaviour unique to sign language consisted of finger spelling and gesture in the place of or in combination with sign language. While Deaf people use both finger spelling and gesture, they are not considered to be true signs, though it is acceptable to use them in combination with signs. The visual modality of language permitted the use of these where attrition occurred. Finger spelling was used in the place of inaccessible signs. In some instances, it prompted recall and the finger spelling was then followed immediately by the correct sign. One subject looking at a picture of an airplane extended his arms from his body in a manner that indicated wings. This gesture, though its intention is clear, bears little resemblance to one handed sign formed with the first and last finger extended and moving upwards. The gestures in this study often deviated from the spatial boundaries for sign language and were used when the sign needed was not accessible.

5.5. NON-LINGUISTIC BEHAVIOUR. Non-linguistic behaviour that also provided evidence of attrition consisted of requests for help from the interpreters and obvious physical discomfort with the task. One subject hit herself on the forehead with an open palm, another twirled his forefinger and another repeatedly extended both arms, palm upwards, as if to indicate, 'I do not know' or 'I can not remember'. There were numerous unnatural pauses and overt comments made during the tests, such as, 'My RSL is not very good' and 'I don't remember anything in RSL'.

6. CONCLUSIONS. The hearing Russians who completed the same tests orally behaved more favourably, as predicted by the Model of Ethnolinguistic Vitality for their group. They have support for spoken and written Russian in Israel, exposure to it and ample opportunity to use and maintain their mother tongue. Thus, they produced their L1 relatively fluently and efficiently, illustrating substantially less evidence of attrition. Their average overall rate of error was 7%. The average overall rate of error for the deaf immigrants was 27.7%.

No conclusive evidence was found regarding my original hypothesis that one's length of residence in Israel may be related to the amount of attrition. It appears there were too many confounding variables. The connection between the age of immigration and the amount of attrition, however, did produce somewhat more significant results, indicating that those who had immigrated as children *generally* suffered more language loss than those who had arrived as adults. But this information must be carefully considered for additional factors, such as whether these children had reached puberty, assuming it is a critical stage in language development, and how long they attended Israeli schools for the deaf, something the adults did not have the opportunity to do.

7. DISCUSSION. The results of this preliminary study show that a sign language suffers L1 attrition for the same reasons and in patterns similar to those of spoken language. Sign language research has shown that sign languages are natural languages with grammatical structures, rules, and patterns of acquisition similar to those of spoken languages. Thus, this study provides additional and novel evidence for the claim that sign and spoken languages function like one another when it comes to human communication and interaction. This documentation of attrition in sign language therefore offers new perspectives, not only on how attrition is related to sign language, but on the human capacity for language as well.

REFERENCES

ALLARD, REAL & RODRIGUE LANDRY. 1992. Ethnolinguistic vitality beliefs. In *Maintenance and loss of minority languages*, ed. by Willem Fase, Koen Jaspaert & Sjaak Kroon, 171–95. Amsterdam: John Benjamins.

LAMBERT, RICHARD & BARBARA FREED (eds.). 1982. *The loss of language skills.* Rowley MA: Newbury House

REMENNICK, LARISSA. 1998. Identity quest among Russian Jews in the 1990's: before and after immigration. In *Jewish survival*, ed. by Ernest Krauze & Gita Tulea, 241–59. Moncton: Transaction Publishers.

WAAS, MARGIT. 1996. *Language attrition downunder: German speakers in Australia.* Frankfurt: Peter Lang.

A CORPUS STUDY ON THE (NON-)PHYSICALITY OF LINGUISTIC OBSERVATIONS

Douglas W. Coleman
University of Toledo

THIS STUDY is a continuation and extension of the author's ongoing analysis of the nature of linguistic data as found in a theoretical linguistics corpus. Earlier stages in this work have been reported in other articles by the author (Coleman 1997, 1999, and 2000).

1. CORPORA. For the current study, there were two corpora. The first: *Language*, vol. 67, nos. 1–4, main text of all main section articles. The second: articles from various journals with texts available in electronic form; all available on-line via subscription through the OhioLink system; the first article spanning ≥10 pages from the most recent complete volume of every twenty-fifth journal in the on-line category 'life sciences' until I had 16 articles. One article was in an incompatible file format and had to be rejected. Corpus items are designated by journal name abbreviation + vol. + no. + starting and ending page numbers, e.g. **AB 59-1;1.10**. Corpora are designated 'TL' (theoretical linguistics, those items from *Language*) or 'LS' ('life sciences', those items from the journals in the Academic Press 'life sciences' category). (Information on the corpora is presented in Table 1, overleaf.) A list of all items in the LS corpora is found in Appendix I. Articles from *Language* appear in the References section if cited.

The discrepancy in word count is clear, though the number of articles is comparable. A Wilcoxon W (Mann-Whitney U) test of median article lengths shows that articles in TL have significantly more words than those in LS (W=204.0, p<0.0001).

2. HYPOTHESIS. Patterns of lexical usage identifying 'data' as something physical (having objective existence) or not (existing subjectively only) will reveal significant differences between the TL and LS corpora.

3. METHOD. TL articles were (1) scanned, (2) spell-checked, (3) pre-processed to remove hyphenation for accurate automatic keyword-recognition, (4) concordanced for key words using a SNOBOL4 program written by the investigator. Then (5) keywords were tabulated (counted and usages categorized) and analyzed both statistically and qualitatively. Capitalization was ignored in the identification of tokens of a given lexical type. Since LS articles were already in *.pdf files (Adobe Portable Document Format), tabulation was more direct, based on use of Adobe Acrobat Reader's find-text function. Data was analyzed in a StatGraphics spreadsheet. Below, cited *tokens* are given in context, set off in bold italics; *types*, in contrast, are indicated in small caps; e.g., see the token of the type DATA in Figure 3 (below).

Corpus	No. of Words	No. of Articles	Min. Size	Max. Size
TL	217,450	14	7278	35543
LS	89,025	15	2153	9515

Table 1. *Corpora.*

(44)			SINGULAR	PLURAL
	a.	'cat'	kazh	kizhier
	b.	'squirrel'	kazh-koad [cat-wood]	kizhier-koad

Figure 1. *Excerpt from [TL: L 67-4;675.725].*
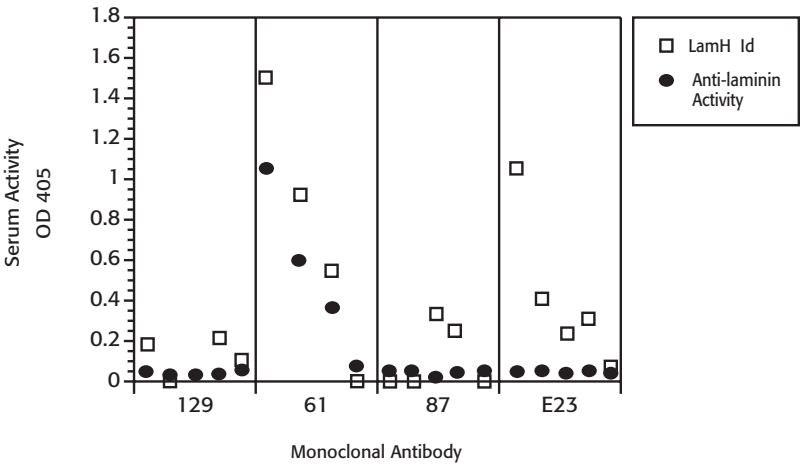
Figure 2. *Excerpt from [LS: IMG 51-1;20.29]. (This figure has been redrawn in this volume for clarity.)*

The type-token distinction is critical here. By *type*, I refer to a generalized category of observations. By *token*, I refer to a single observable instance of a type. As I point out in Coleman (2000), an example such as that offered by Stump (1991) in Figure 1 does not constitute a single observation, but rather stands for the entire class of purported past observations of four grammatical forms *as well as* any and all potential future ones. As such, it is a generalized type. The example offered by Ward et al. (1991) in Figure 3, marked as an ungrammatical sentence, cannot constitute an observable entity at all. It is not only a generalized type, but one which in principle can never be observed at all. In contrast, a data point (e.g., one in the form '•') on the graph from Fitzsimons et al. (2000) seen in Figure 2, represents a single observation of two properties (which the authors label 'Serum Activity OD 405' and 'Monoclonal Antibody', respectively) of a given mouse in their experiment. Hence, any confusion about tokens vs. types (data vs. examples) involves confusion about what one is actually observing vs. what conclusions one is drawing from observations.

> …the same sort of pragmatic influence fails to salvage the following ***example***, where there is clearly no difficulty in interpreting the anaphor:
>
> (21)   *I'll eat *oysters* on occasion, but I'm not really much of a *them* lover. On the basis of such ***data,*** we can reject the view that…

Figure 3. *Excerpt from [TL: L 67-3;439.474].*

> In the Gambian ***example*** in Fig. 1, the opposite end of the spectrum is illustrated…

Figure 4. *Excerpt from [LS: AB 59-1;1.10].*

4. DATA TOKENS (*DATA/DATUM*). Authors from both corpora were equally likely to use a token of the type DATA as those from the other (W=78.0, p>0.24). They were equally likely to refer to 'the data' in a generic sense or to non-specific, topically-defined domains of DATA than were LS authors (W=102.5, p>0.32). Such references seem to occur frequently when authors are discussing some methodological aspect to their work. These two factors varied across individual author, but not by corpus.

Things typically referred to via DATA tokens in the TL corpus were things like Stump's (1991:696) item (44), seen in Figure 2. Things typically referred to via DATA tokens in the LS corpus more closely resembled that in Figure 2, from Fitzsimons et al. (2000:24), their figure labelled 'Serum anti-laminin and LamH-idiotype (Id) reactivity after administration of trans-gene hybridomas to histocompatible normal mice'.

Authors in the TL corpus tend to use DATA and EXAMPLE interchangeably. Consider the excerpt from Ward et al. (1991:452) in Figure 3.

A similar usage is the item already seen in Figure 1. Although Stump (1991) refers to it individually as an ***example*** on page 695 ('The examples in 44 illustrate this'), it is clearly within the scope of what is referred to as ***data*** at the start of the article ('All of the Breton ***data*** cited here…', p. 675). Indeed, it is striking that some TL authors seem to use tokens of DATA and EXAMPLE in complementary distribution, as if *[-specific]* and *[+specific]* variants of each other.

In addition to the numerous tokens of DATA and EXAMPLE used coreferentially, there are also many where they refer to the same types of things in the articles, typically numbered text items marked with asterisks, bracketing, prosodic contours, and so on. I presented many such examples in Coleman (2000). I will beat this (I hope) dead horse no further.

Authors in the LS corpus essentially never use DATA to refer to the same things in their papers as they do when using EXAMPLE. There is one case that is unclear: in AB 59-1;1-10, the author refers to a figure containing three graphs as showing ***data***—there are two tokens of DATA in the caption at the foot of the graphs (Figure 5, overleaf). But elsewhere at one point, she says that it contains an ***example*** (Mace 2000:4). No other such cases in which DATA and EXAMPLE are or are possibly conflated appear in the LS corpus.

Figure 1. Cross-sectional data on the life history of rural Gambians based on data collection by Sir Ian McGregor from Keneba and Manduar villages between 1950 and 1975. (a) Average annual weight gain as a proportion of total weight, between birth and 25 years of age for males and females. (b) Annual mortality hazard for males and females over the life span. (c) Age-specific fertility (numbers of live births per year) for males and females over the life span (3-year running means).

Figure 5. Excerpt from [AB 59-1:1.10] (Mace 2000:3). (This figure has been redrawn in this volume for clarity.)

In Coleman (2000), I used the argument structure analysis scheme of Toulmin (1964) to analyze the place of what authors in TL and AL (applied linguistic) corpora refer to as DATA. TL authors tend to use what they refer to as DATA to provide 'confirming evidence' (the term used by Botha (1973:35). The tendency among the LS authors is to use their DATA for hypothesis-testing, i.e., for *dis*confirmation of a hypothesis, much like those of the AL corpus analyzed in Coleman (2000).

Examples cited by TL authors are typically not tokens, *per se*. An EXAMPLE (DATA) such as Stump's (1991:696) 'Breton *data*' in Figure 1 is not offered as an event (a token) having been observed at a specific time and place, but represents any and all potential and/or actual occurrences of the supposed grammatical forms as types in any given utterances. Asterisked items, such as that given in Figure 4 (Ward et al. 1991:452), of course, are by definition never observed, so what represents DATA in this case is a *supposed* 'native-speaker judgment' of the item's ungrammaticality. I say 'supposed', since TL authors in my corpus are unlikely to identify precisely the observational sources of their DATA, very much less so than LS authors (W=39.5, p<0.02).

To clarify: I consider the observational source to be precisely identified only if an author states explicitly what real-world event has been observed and under what conditions. To say, for example, 'I got this data from Smith (2001)' does not say where *Smith* got it. To say, 'I got this data item from *Time*, July 5, 1998, p. 7; that data item from *The Washington Post*, Jan. 14, 1998, p. A8'; in contrast, is a clear identification, as would be descriptions of interview protocols, experimental methods, questionnaires, and of the persons upon whom these instruments were used.

5. EXAMPLE TOKENS (*EXAMPLE, EXAMPLES, EXEMPLIFY, EXEMPLIFIES,* ETC.). Authors in TL were much more likely to use EXAMPLE than were those in LS (W = 199.0, p < 0.00001). As mentioned above, EXAMPLE and DATA were frequently conflated in the TL corpus, but only once (possibly) in the LS corpus. The difference was significant at a high level of statistical reliability (W = 194.0, p < 0.0001).

6. CLEAR OVERALL DIFFERENCE BETWEEN THE TWO CORPORA. Using just three of the variables measured in this study, it is possible to correctly predict whether a given text is from the TL corpus or the LS corpus at an overall accuracy above 88%. The variables in question are the proportion of data which is explicitly sourced (DSrcProp), the rate of use of EXAMPLE (ExRate), and the rate of usage of EXAMPLE to co-refer to DATA (EeqDRate). Because it is a proportion (the proportion of DATA tokens which indicate items that have been sourced), values for DSrcProp run from 0 to 1. ExRate and EeqDRate represent, respectively, (a) the overall rate of EXAMPLE tokens in a given text as a whole and (b) the rate of EXAMPLE tokens which refer to the same things or the same type of things that the author refers to via a DATA token. ExRate is the number of EXAMPLE tokens in an article divided by the total number of words in the article. EeqDRate is the number of EXAMPLE-as-data tokens in an article divided by the total number of words in the article.

The medians for all three of these variables had showed significant differences between the two corpora: DSrcProp (LS = 0.6809, TL = 0.0000, W = 52.5, p < 0.02), ExRate (LS = 0.0002, TL = 0.0020, W = 199.0, p < 0.00005), and EeqDRate (LS = 0.0000, TL = 0.0012, W = 194, p < 0.00003).

A discriminant analysis based on the same three variables yields Wilks Lambda = 0.467379; χ^2 is used as the test of significance (χ^2 = 19.3957, p = 0.0002). A classification table (Table 2, overleaf) shows that over 86% of the LS articles are correctly assigned to their corpus in this fashion, over 85% of the TL articles.

Based on a suggestion made by Stephan Gries (Southern Denmark University) at the end of the oral version of this paper at LACUS 2001, I tried repeating the analysis with a randomly-selected sub-sample of the data. The first randomization yielded n_{LS}=7, n_{TL}=9. This happened to result in 87.50% of the total cases correctly classified (LS: 85.71%, TL: 88.89%; discriminating function p ≤ 0.0011). The second randomization yielded a different subset also n_{LS}=7, n_{TL}=9. This happened to result in 100.00% of the total cases correctly classified (discriminating function p ≤ 0.0001). A third randomization yielded n_{LS}=8, n_{TL}=5. The smaller number of samples happened to result in only 84.62% of the total cases correctly classified (LS: 100.00%, TL: 60.00%; with a non-significant but 'suggestive' discriminating function p ≤ 0.0867).

I redid the original calculation on the whole data set with a step-wise approach, forward and backward, both of which showed that ExRate was contributing the most to the accuracy of the classification of corpus items. Out of curiosity, I checked for a correlation between ExRate and EeqDRate and found a strong correlation, indeed (r = 0.938574, p < 0.0001). In other words, the higher the rate of EXAMPLE tokens, the more likely the conflation of data and examples.

Actual Corpus	Group Size	Predicted Corpus	
		LS	TL
LS	15	13 (86.67%)	2 (13.33%)
TL	14	2 (14.29%)	12 (85.71%)
percent of all cases correctly classified: 86.21%			

Table 2. *Discriminant analysis classification table.*
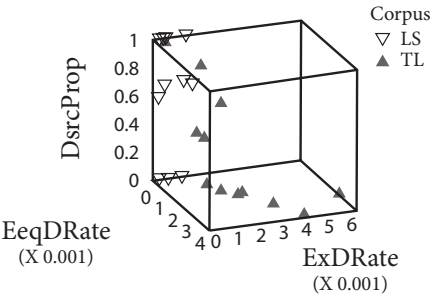
Figure 6. *3-D scatterplot.*

Figure 6 shows a scatterplot of the three variables in question (DSrcProp, ExRate, and EeqDRate) using the original (complete) data set. Each article in the LS corpus is represented by an open downward-pointing triangle ($\triangledown$), each article in the TL corpus, by a solid upward-pointing triangle ( ). Note how the LS ($\triangledown$) items cluster in the left corner, while TL articles ( ) follow a sort of backwards 'J' toward the opposite corner. Two of the TL articles, in particular, are extremely difficult to differentiate from the LS cluster—they are MacLaury (1991) [DSrcProp = 0.79] and Boysson-Bardies & Vihman (1991) [DSrcProp = 1.0]. MacLaury's was a study on color semantics using a variety of interview-based elicitation techniques (p. 37) with a number of human subjects. Boysson-Bardies & Vihman used audio- and video-recorded material from 20 infants; they analyzed their results statistically.

7. CONCLUSIONS. What does all this suggest for linguistics as a field of study? The LS corpus is very consistent in regard to the identification of DATA as something observable in terms of objects and events that exist apart from the observer, not strictly 'objects' created by the viewpoint of the observer (and existing only in that observer's subjective experience)—to paraphrase Saussure (1959:8) on the 'objects of language'. As a whole, the TL corpus is very unlike the LS corpus in this regard.

The LS corpus contains *at most* one case in which references to DATA (concrete observation tokens) and an EXAMPLE (a type, and therefore an abstraction) are conflated. Conflation of DATA and EXAMPLE, of concrete token and abstract type, is more the rule than the exception in the TL corpus.

However, even in the TL corpus this conflation is not universal; obviously, it does not have to be. Nor does linguistic DATA need to consist of things that no author in the LS corpus (or some other scientific corpus) would label as such.

Finally, I must emphasize that what I have been discussing are *not* simply issues of terminology. They reflect underlying practices and approaches. It can be no coincidence that the two articles (MacLaury 1991 and Boysson-Bardies & Vihman 1991) in the TL corpus which appear closest to the LS cluster in Figure 7 are also two articles which exhibit perhaps more clearly noticeable characteristics of 'social science' methodologies and genres than the majority of the other articles in the TL corpus.

REFERENCES

BOTHA, RUDOLF P. 1973. *The justification of linguistic hypotheses: A study of nondemonstrative inference in transformational grammar*. The Hague: Mouton.

DE BOYSSON-BARDIES, BÉNÉDICT & MARILYN MAY VIHMAN. 1991. Adaptation to language: Evidence from babbling and first words. *Language* 67(2):297–319.

COLEMAN, DOUGLAS W. 1997. Science or philosophy? Progress report on a lexical analysis of corpora from two journals in linguistics. PALC '97–*Practical Applications in Language Corpora*, ed. by Barbara Lewandowska-Tomaszczyk & Patrick James Melia. Łódź Poland: University of Łódź.

———. 1999. Assumptions, hypotheses, and theories in 'applied' and 'theoretical' linguistics. LACUS FORUM 25:461–72.

———. 2000. Linguistic data and its uses: The problem of conflating DATA and EXAMPLES. LACUS FORUM 26:71–80.

FITZSIMONS et al. [See Appendix I.]

MACE [See Apendix I.]

MACLAURY, ROBERT A. 1991. Social and cognitive motivations of change: Measuring variability in color semantics. *Language* 67(1):34–62.

DE SAUSSURE, FERDINAND. 1959. *Course in general linguistics,* trans. by Wade Baskin. New York: Philosophical Library.

STUMP, GREGORY T. 1991. A paradigm-based theory of morphosemantic mismatches. *Language* 67(4:675–725.

TOULMIN, STEPHEN EDELSTON. 1964. *The uses of argument*. Cambridge: Cambridge University Press.

WARD, GREGORY, RICHARD SPROAT & GAIL McKOON. 1991. A pragmatic analysis of so-called anaphoric islands. *Language* 67(3):439-74.

APPENDIX I: LIST OF LIFE SCIENCES (LS) CORPUS ITEMS

AB 59-1;1.10. MACE, R. 2000. Evolutionary ecology of human life history. *Animal behaviour* 59(1):1–10.

ASC 2-1;9.19. ELANGOVAN, A. & K. F. SHIM. 1998. Dietary phosphorus requirement of juvenile tiger barb, *Barbus tetrazona* (Bleeker, 1855). *Aquarium sciences and conservation* 2(1):9–19.

BBA 1519-1;1.12. SMITS, V. A. J. & R. H. MEDEMA. 2000. Checking out the G_2/M transition. *Biochimica et biophysica acta / Gene structure and expression* 1519(1/2):1–12.

BM 13-1;37.46. HÖRDT, W., V. RÖMHELD & G. WINKELMANN. 2000. Fusarinines and dimerum acid, mono- and dihydroxamate siderophores from *Penicillium chrysogenu*m, improve iron utilization by strategy I and strategy II plants. *Biometals* 13(1):37–46.

BFJ 103-1;7.22. KUPIEC, B. & B. REVELL. 2001. Measuring consumer quality judgements. *British food journal* 103(1):7–22.

CLAN 13-1;17.35. Educational Affairs Committee, American Association of Clinical Anatomists. 2000. A clinical anatomy curriculum for the medical student of the 21st century: Developmental anatomy. *Clinical anatomy* 13(1):17–35.

DBR 128-1;35.44. DEMÊMES, D., C. J. DECHESNE, S. VENTEO, F. GAVEN & J. RAYMOND. 2001. Development of the rat efferent vestibular system on the ground and in microgravity. *Developmental brain research* 128(1):35–44.

ER 44-1;7.18. PARRINO, L. A. SMERIERI & M. G. TERZANO. 2001. Combined influence of cyclic arousability and EEG synchrony on generalized interictal discharges within the sleep cycle. *Epilepsy research* 44(1):7–18.

EX 2-1;5.23. Coles, S. G. & M. J. Dixon. 2000. Likelihood-based inference for extreme value models. *Extremes* 2(1):5–23.

FGB 31-1;7.20. YUN, S-H., T. ARIE, I. KANEKO, O. C. YODER & B. G. TURGEON. 2000. Molecular organization of mating type loci in heterothallic, homothallic, and asexual *Gibberella / Fusarium* species. *Fungal genetics and biology* 31(1):7–20.

IJIM 28-1;13.26. SCHMITZ, A. & L. T. WASSERTHAL. 1999. Comparative morphology of the spiracles of the Papilionidae, Sphingidae, and Saturniidae (Insecta: Lepidooptera). *Journal of insect morphology and embryology* 28(1/2):13–26.

IMG 51-1;20.29. FITZSIMONS, M. M., H. CHEN & M. H. FOSTER. 2000. Diverse endogenous light chains contribute to basement membrane reactivity in nonautoimmune mice transgenic for an anti-laminin Ig heavy chain. *Immunogenetics* 51(1):20–29.

JCN 436-1;4.16. MACKLER, J. M. & N. E. REIST. 2001. Mutations in the second C_2 domain of synaptotagmin disrupt synaptic transmission at Drosophila neuromuscular junctions. *Journal of comparative neurology* 436(1):4–16.

JMB 179-1;1.12. Reuss, L. 2001. Isolated-polarized epithelial cells as an experimental system for cell physiology studies. *Journal of membrane biology* 179(1):1–12.

JSB 132-1;6.18. YASUNAGA, T., Y. SUZUKI, R. OHKURA, K. SUTOH & T. WAKABAYASHI. 2000. ATP-induced transconformation of myosin revealed by determining three-dimensional positions of fluorophores from fluorescence energy transfer measurements. *Journal of structural biology* 132(1):6-18.

❧

LINGUISTICS AS AN EMPIRICAL SCIENCE: THE STATUS OF GRAMMATICALITY JUDGMENTS IN LINGUISTIC THEORY

Patrick J. Duffley
Laval University

'anyone who has taught an introductory syntax course has had the experience of presenting an "ungrammatical" example only to be told by some smart-aleck about an unsuspected interpretation on which the sentence is quite normal' (McCawley 1982:78). A recent discussion with one of my colleagues, in which I had the pleasure of playing the part of the smart-aleck, led me to some serious reflection about the question of grammaticality judgments and their role in linguistic methodology. The conclusions of these reflections will be presented in this paper. My colleague and I were examining sentences (1)a and (1)b below, the first of which was claimed to be acceptable and the second not:

(1) a. What did John hurt himself fixing?
     b. *What did John hurt Bill fixing?

Although I could sense that there was some sort of difference in ease of interpretation between these two sentences in favour of (1)a, I was not at all happy with the suggestion that (1)b was 'less acceptable' or 'less grammatical' than (1)a. So I asked my colleague in what sort of context someone would say (1)a, and the description ran more or less as follows: 'the speaker knows that John was fixing things around the house yesterday and that he hurt himself while fixing something, but he does not know what that thing was and would like to be informed thereof'. This led to the logical rejoinder that if the speaker knew that John was fixing things around the house with Bill yesterday and that he hurt Bill while fixing some object whose identity the speaker was ignorant of but wished to know, then (1)b is a perfectly acceptable English sentence for obtaining that information.

The sort of discussion referred to above, which is typical in linguistic circles, leads one to wonder just what people (linguists included) are doing when they make so-called grammaticality judgments. It also raises the more basic question of why some linguists use such judgments as their prime source of data. What are they hoping to prove? Can grammaticality judgments provide the sort of evidence that these linguists are looking for?

It is not at all evident why an account of speakers' competence in understanding and producing language should be based on behaviour in a situation where they are doing neither, but rather are being asked to report their intuitions about the

acceptability of certain sequences of words. The motivations for following such a procedure can be boiled down to four main reasons:

> First, by eliciting judgments we can examine reactions to sentence types that might occur only very rarely in spontaneous speech or recorded corpora. This is a standard reason for performing experiments in social science—observational study does not always provide a high enough concentration of the phenomena we are most interested in. A second, related reason for using grammaticality judgments is to obtain a form of information that scarcely exists within normal language use at all—namely, negative information, in the form of strings that are not part of the language. The third reason for using judgments is that when one is merely observing speech it is difficult to distinguish reliably slips, unfinished utterances, and so forth, from grammatical production. A fourth and more controversial reason is to minimize the extent to which the communicative and representational functions of language obscure our insight into its mental nature. (Schütze 1996:2–3)

With respect to the last reason, Schütze admits that it 'presupposes a particular view of grammatical competence as cognitively separate from other facets of language knowledge and use, and hence its validity depends on one's theoretical stance on the issue'. It will be my contention in this paper that all of the other reasons except the first are also theory-dependent and that on top of this grammaticality judgments are practically worthless as scientific evidence, even if one accepts the theoretical presuppositions of generative grammar.

To start with the last point, it must be realized that the term 'grammaticality judgment' itself is in fact a misnomer. What is actually meant would be better expressed by the term 'acceptability judgment' (cf. Schütze 1996:26). Given Chomsky's (1965:10–11) definition of grammaticality as belonging to the sphere of competence, that is, of the ideal speaker's knowledge of his language, it makes no sense to speak of 'grammaticality judgments', since grammaticality is not accessible to people's intuitions: all a native speaker can do is judge a string's acceptability. So what people are actually doing when reacting to a sequence of words presented to them by a linguist is judging whether it seems acceptable to them or not. Is there any relation, then, between acceptability and grammaticality which would allow inferences to be made about the latter based on the former?

According to generative theory there is indeed a relation between these two concepts, whose nature is described in the following well-known passages:

> Acceptability is a concept that belongs to the study of performance, whereas grammaticalness belongs to the study of competence… Grammaticalness is only one of the many factors that interact to determine acceptability. Correspondingly, although one might propose various operational tests for acceptability, it is unlikely that a necessary and sufficient operational criterion might be invented

for the much more abstract and far more important notion of grammaticalness. (Chomsky 1965:10–11)

…linguistics as a discipline is characterized by attention to certain kinds of evidence that are, for the moment, readily accessible and informative: largely, the judgments of native speakers. Each such judgment is, in fact, the result of an experiment, one that is poorly designed but rich in the evidence it provides. In practice, we tend to operate on the assumption, or pretense, that these informant judgments give us 'direct evidence' as to the structure of the I-language, but, of course, this is only a tentative and inexact working hypothesis… In general, informant judgments do not reflect the structure of the language directly; judgments of acceptability, for example, may fail to provide direct evidence as to grammatical status because of the intrusion of numerous other factors. (Chomsky 1986:36)

These observations show that Chomsky himself is aware of the indirectness of the link between acceptability (performance) and grammaticality (competence), as indicated by the reference to the 'numerous other factors' that interact with the grammar to produce acceptability. Just how numerous and uncontrollable these factors are is shown by the studies of Birdsong 1989 and Schütze 1996. They comprise things like:

(a)   the instructions given to the subjects doing the questionnaire
(b)   the order of presentation of the sentences submitted for speaker reactions (the first sentences in a questionnaire tend to be judged much more severely than the others; cf. Greenbaum 1973, 1976)
(c)   the effect of the repetition of an unacceptable structure leading people to accept it
(d)   judgment strategies (one does not know whether the subjects are using the same criterion to decide on acceptability)
(e)   modality and register (a written questionnaire already represents a fairly formal context for most speakers; cf. Greenbaum 1977)
(f)   how much time is given to the informants to react
(g)   context (is it easy to imagine a possible context for the sentences?)
(h)   meaningfulness (can people make sense of the sentences?)
(i)   length and complexity of sentence judged
(j)   frequency of constructions (less frequent structures are often judged unacceptable)
(k)   lexical content of items
(l)   rhetorical structure (structural parallelism renders certain sequences acceptable which are not perceived so otherwise; cf. Langendoen 1972).

I would add to this long list an even more basic and important factor: the very fact of asking a speaker to make an acceptability judgment is asking him to do something completely unnatural. What people rather are accustomed to doing with their

language is not making grammaticality judgments, but simply using it to express themselves. Add to all this the fact that one is not supposed to know what the grammar looks like—it is what the linguist is trying to determine—and you get a bag so mixed that its contents are impossible to sort out.

Why, then, have recourse to data whose connection with the object of one's hypothesis is so tenuous? This question takes us back to the motivations given in defense of this procedure—rarity of significant data in spontaneous speech, the need to obtain negative data, the facilitation of distinguishing performance errors from grammatical production, the separating out of the communicative and representational functions of language from its mental structure. When scrutinized more closely, all of these reasons except the first turn out to be products of the theoretical stance adopted by generative grammarians. I have already alluded to Schütze's admission that this is the case for the last reason. Regarding the facilitation of the distinction between performance errors and grammatical production, it should be fairly obvious that this alleged justification merely begs the question by presupposing some concept of 'grammatical production'. Moreover, as shown by the enumeration given above of the many possible performance errors which can occur in the making of grammaticality judgments, the use of such judgments does not facilitate the identification of potentially extralinguistic factors which have an impact on the data but merely adds further factors to the list. This has led Schütze (1996:179–180) to observe that: 'In fact, it might appear that grammaticality judgments are the *worst* way to get at linguistic competence, as compared to production and comprehension, because they involve the interaction of many more factors'.

Under Schütze's pen, this is a merely rhetorical objection. He goes on to give two reasons why this does not constitute grounds for abandoning grammaticality judgments as a source of data:

1.   while more factors are involved in such judgments, they 'might be less mysterious than those connected to language use' (how could we ever define the 'understanding of a sentence' or 'communicative intentions' and how could we draw conclusions about grammaticality from them?);
2.   grammaticality judgments provide an alternative path to the grammar (they are subject to different influences than language use is and so facilitate the search for the common core that underlies both types of behaviour, i.e., the grammar).

Neither of these reasons stands the test however. Concerning the first motive, one of the crucial factors impacting on grammaticality judgments is necessarily whether the subject can understand the sentence or not (cf. Schütze 1996:162), a fact which would seem to make natural language comprehension no more 'mysterious' than such judgments. Moreover, if one compares comprehension and grammaticality judgments in Schütze's own model (p. 175), one notes that both are determined by the same four factors—input, knowledge (general, contextual, etc), competence and parsing

strategies—with four other factors being added for grammaticality judgments. This only makes sense—in order to judge a sentence's acceptability, one must first comprehend it—but the facts certainly do not support the suggestion that the process of judging a sentence for grammaticality is any less obscure than that of comprehending the sentence in a natural context—quite the contrary. As for the argument that grammaticality judgments provide an alternative path to the grammar, it is much sounder methodology to begin with the cases where it appears probable that the fewest factors are involved before attempting to come to grips with the more complex cases. As Birdsong (1989:72) puts it, 'the hypocrisy of rejecting linguistic performance data as too noisy to study, while embracing metalinguistic performance data as proper input to theory, should be apparent to any thoughtful linguist'.

Schütze's second reason—the need to obtain negative data—brings us even closer to our objective of understanding why linguists of the generative school have such regular recourse to grammaticality judgments. The very fact of needing to discriminate between certain sequences of words that are 'part of the language' and other which are not implies a certain view of both grammar and language which is peculiar to generative theory. The citation below provides a capsule summary of this view:

> A major objective of linguistic research is to construct a grammar capable of generating all the grammatical sentences and no ungrammatical ones. This research involves identifying the rules that allow speakers to determine which sentences of their language are well-formed and which are not. (O'Grady & Dobrovolsky 1987:103)

Particularly revealing in this quotation is the close relation made between the project of constructing a generative grammar and the search for the rules that allow speakers to judge which sentences are well-formed and which are not. This suggests that a transfer has taken place from the role the grammar is claimed to have in the theory to the role of the subject in a grammaticality judgment: just as the grammar determines what is well-formed and what is not, so the speaker confronted with a string of words in a questionnaire decides what is structurally good and what is not. However this is definitely not what people do when they comprehend what others are trying to say in a normal speech situation (nor is it, as we have seen above, what they are doing when they make grammaticality judgments). Such a view of grammar makes it an algorithm for performing structural 'grammaticality' choices rather than an instrument for carrying on communication.

Examples of this procedure abound; to give a typical case, one might refer to Givón's studies on causative verbs in two articles entitled 'Cause and Control: On the Semantics of Interpersonal Manipulation' (1975) and 'The Binding Hierarchy and the Typology of Complements' (1980). In his discussion of the verbs *cause*, *make* and *have*, Givón claims that these English verbs may be scaled according to two semantic properties which are universally attested: (a) intended ('controlled') vs. unintended ('uncontrolled') causation; (b) 'mediated vs. direct causation' (1980:335).

Cause is a 'noncontrol causation verb', *make* a 'direct control causation verb' and *have* a 'mediated control causation verb'. When the data supporting these 'generalizations' is confronted with actual usage however, the suspicion arises that the data was fabricated to support the universal semantic properties rather than the latter being inferred from an observation of usage. For instance, the claim that *make* denotes deliberately intended causation while *cause* evokes accidental causation is based on the purported contrast in acceptability between (2)a and (2)b:

(2)   a. John accidentally/inadvertently caused Mary to drop her books.
      b. *John accidentally/inadvertently made Mary drop her books.

Actual usage shows, however, that the distinction between *cause* and *make* has nothing to do with intentionality. On the one hand, *cause* can denote a deliberate action, as in (3):

(3)   If a person has thoughtlessly or deliberately caused us pain or hardship…
      (Brown U. Corpus B08 0470)

On the other hand, *make* can evoke unintentional causation, as in:

(4)   Other women—they only made me love you more.
      (O'Neill 1955 [vol. 1]:130)

The analysis of *have* as denoting mediated causation, which is intentional but requires the intervention of a third party, suffers from a similar lack of support from the empirical data. Givón adduces the purported contrast in acceptability between (5)a and (5)b/c:

(5)   a. I had her lose her temper by sending John to taunt her.
      b. ? I caused her to lose her temper by sending John to taunt her.
      c. ? I made her lose her temper by sending John to taunt her.

Actual usage in this case would seem, however, to be exactly the opposite of Givón's judgements: (5)a makes no sense at all, while (5)b and (5)c are quite normal. *Have* is used in English to evoke getting someone to do something by exercising one's authority or control over them through a request or command, as in (6) below:

(6)   The teacher had me recite my poem in front of the class.

This does imply intentionality on the part of the causer and compliance on the part of the causee, but there is no idea at all of mediation by a third party suggested by the meaning of the construction illustrated in the sentence above.

The conclusion we have been led to, therefore, is that grammaticality judgments are not a reliable source of empirical data. What speakers are doing when they perform such judgments is appraising the acceptability of the utterances they are being asked to evaluate. Even if one accepts the hypothesis of a separate grammatical module constituting one of the important factors which determine acceptability, the causal link between acceptability and grammaticality does not allow one to make inferences from one to the other. If natural language production (i.e., performance) is viewed as inadequate data for inferring conclusions about grammaticality, the data provided by grammaticality judgments must be considered even less trustworthy. As a type of metalinguistic behaviour, these judgments are themselves just another sort of performance, and as such they are subject to even more confounding factors than natural utterance production.

One might palliate some of the drawbacks of grammaticality judgment data by the design of the questionnaire used to elicit them. As shown by Gries (this volume), the inclusion of experimental controls such as randomized presentation of sentences, inclusion of fillers, and clear exemplification and explanation of the required judgment process, can elicit reactions which correspond fairly closely to corpus data. Moreover, if sentences were presented to informants with a context, that is, a description of the communicative situation, then, in this more natural setting, one should obtain more reliable judgments of acceptability.

The fact remains, nevertheless, that sound methodology would advise one to first study language in its natural setting before placing speakers in an artificial situation and asking them to do something entirely different from everyday language use. The very nature of a questionnaire suggests a testing of the informants' ability to conform to some norm of expected behaviour, and triggers the reaction 'what *should* one say in this situation?'. Even if one were to succeed in eliminating this conditioned reflex—something highly unlikely in the present author's opinion—there still remains a hypothetical element inherent in the nature of a questionnaire: informants are being asked to answer the query 'what *would* one say in this situation?' Neither question corresponds necessarily to what the speaker *actually says* in a given situation. Thus, in any case, one is driven back to actual usage as the final test of the explanatory capacity of any theory. Isn't what people actually say what we linguists are supposed to be explaining in the first place?

REFERENCES

BIRDSONG, DAVID. 1989. *Metalinguistic performance and interlinguistic competence*. Berlin: Springer-Verlag.

CHOMSKY, NOAM. 1965. *Aspects of the theory of syntax*. Cambridge: MIT Press.

——. 1986. *Knowledge of language: Its nature, origin, and use*. New York: Praeger.

GIVÓN, TALMY. 1975. Cause and control: On the semantics of interpersonal manipulation. In *Syntax and semantics*, vol. 4, 59–91. New York: Academic Press.

———. 1980. The binding hierarchy and the typology of complements. *Studies in language* 4:333–77.

GREENBAUM, SIDNEY. 1973. Informant elicitation of data on syntactic variation. *Lingua* 31:201–12.

———. 1976. Contextual influence on acceptability judgments. *Linguistics* 187:5–11.

———. 1977. Judgments of syntactic acceptability and frequency. *Studia linguistica* 31(2):83–105.

LANGENDOEN, D. TERENCE. 1972. The problem of grammaticality. *Peabody journal of education* 50(1):20–23.

MCCAWLEY, JAMES D. 1982. How far can you trust a linguist? In *Language, mind and brain*, ed. by Thomas W. Simon *&* Robert J. Scholes, 75–88. Hillsdale NJ: Erlbaum.

O'GRADY, WILLIAM *&* MICHAEL DOBROVOLSKY. 1987. *Contemporary linguistic analysis: An introduction*. Toronto: Copp Clark Pitman.

O'NEILL, EUGENE. 1955. *The plays of Eugene O'Neill*. New York: Random House

SCHÜTZE, CARSON T. 1996. *The empirical base of linguistics: Grammaticality judgments and linguistic methodology*. Chicago: University of Chicago Press.

❧

MORPHOLOGICAL PATTERNS OF TERMS IN NEUROSCIENCE AND PARTICLE PHYSICS

Carolyn G. Hartnett
College of the Mainland

How helpful and accurate is the lexicon of science? Scientists and teachers of science since Lavoisier two hundred years ago have stated that knowing science means knowing the language for its concepts. Igor Meľčuk (2001) has discussed the disarray of terms in European science in the last century. The British science journal *Nature* recently showed the concern of science with its lexicons by beginning a weekly column entitled 'Words', reporting the first use of the word *scientist* in 1834 (Danielson*)*. Recently, it reported that the Sudbury Neutrino Observatory in Ontario combined its new measurements with some from the Super-Kamiokande detector in Japan to show that neutrino particles have unexpected mass and sometimes switch their identities. When their identities change, should their names change? An editorial called the finding 'not a crisis for existing models, but a route to deeper ones' (*Nature* 2001a). It welcomed the implications for the theory called the 'standard model', GUT ('Grand Unified Theory'), TOE ('Theory of Everything'), and SUSY (supersymmetry), as well as the yet-unseen Higgs boson[1].

If the lay public theorizes that science is difficult only because of its vocabulary, then the reasons for a difficult lexicon ought to be examined. What evidence of difficulty appears in the morphology of terms in science in English, the leading language of science now? How helpful is the morphology of the lexicons of science? How useful are the many glossaries? Since sciences have different vocabularies, I examined separately the lexicons of two different fields that are currently making great strides. I report comparative tabulations of the morphological patterns of terms in the fields of particle physics and neuroscience. I give examples and conclude with reasons for difficulties, based on the ways scientists must work. All the terms I discuss are standard ones used in publications, not vernacular for informal conversation in the lab.

Particle physics deals with extremely rare tiny particles, too small for instruments to detect, usually predicted only by logical gaps in paradigms and identified only by their effects. They can appear to be in two places at once. The Heisenberg Uncertainty Principle holds that not everything, such as location and velocity, can be known simultaneously. 'Spooky' is how Einstein described one of his own thought experiments[2]. Astrophysicist John Gribbin is dissatisfied with the terms *particles* and *waves*, which he calls metaphors. 'We call those objects particles, for want of a better name. What they really are, we do not know' (1998b:51–52). The Nobel Prize-winning physicist Steven Weinberg also expresses concern about names for the materials he studied:

> …quantum mechanics has transformed the very language we use to describe nature: in place of particles with definite positions and velocities, we have learned to speak of waves and probabilities. Out of the fusion of relativity with quantum mechanics there has evolved a new view of the world, one in which matter has lost its central role. This role has been usurped by principles of symmetry, some of them hidden from view in the present state of the universe. On this foundation we have built a successful theory of electromagnetism and the weak and strong nuclear interactions of elementary particles. Often we have felt as did Siegfried after he tasted the dragon's blood, when he found to his surprise that he could understand the language of birds. (Weinberg 1992:3–4).

Notice how easily the physicist moves from science to mythology. This movement contradicts the stereotype, but it is not unusual for creative scientists. They use their imaginations, enjoy their work, and welcome new information that may make them revise their imperfect theories.

Neuroscience did not exist in 1848 when a large metal bar was blasted straight through the brain of Phineas Gage. He lived and provided the basic case study for a new field. Now neuroscientists are exploring electrical impulses between the billions of tiny neurons in our brains and prompting biological discussions of language use, including puns and slips of the tongue (e.g. Lamb 1998).

MATERIALS AND METHODS. To explore the lexicons of these two sciences, I tabulated the frequency of acronyms, eponyms, nominalizations, learned vocabulary, specialized uses of common words, translations, and other types of etymology. I examined all the words that appeared in both the index of *The Brain* (Time-Life Books 1990) and *The Oxford Companion to The Mind* (Gregory 1987). To match those words in number, I selected the terms on every third page of *Q is for Quantum: Particle Physics from A to Z* (Gribbin 1998a). These glossaries have extensive explanations to enable uninitiated lay readers to comprehend writing for the initiated. I also consulted the *American Heritage Dictionary* (*AHD* 2000) and the *Oxford English Dictionary* (*OED* 1999, CD-ROM 2.0). The basic 415 terms were not adequate guides to the vocabulary of articles labeled for these fields in current issues of the journals *Science* and *Nature*; I combine examples of the missing newer terms with the items in the glossary when the newer terms illustrate a problem better.

Comparisons of tabulations show that the two fields are obscure in different ways. Reorganizing the tabulations according to the motivation for the vocabulary choices shows that the underlying motivations are similar in the two fields, although not always proportionate. The following are the resulting seven reasons for difficulty.

1. RESEARCH METHODS UTILIZE GREEK AND LATIN ROOTS. Differing research methods and materials result in disproportionate use of Latin and Greek roots. Particle physics attempts description, despite the invisibility of the content. For example, *hadrons,* from Greek for 'thick,' are so small that more than a hundred may be needed for an

atom. However, they have much more mass or are 'thicker' than *leptons*, elementary subatomic particles named from Gk *leptos*, 'fine, thin,' from *lepein*, 'to peel,' not to be confused with *leptin* and *Lep,* material which affects neuronal activity (Cowley 2001: 411), or with *LEP*, the acronym for the *Large Electron Positron* in Europe.

Neuroscience used nearly twice as many Latin and Greek sources as particle physics (97 v. 55). These roots were almost five times as likely to refer to the location, shape, or appearance of what was named (34 v. 7). Examples include *cortex* (from Latin 'bark' for the textured outer layer of gray matter), *lateral geniculate nucleus* (from Latin 'side, and 'bent like a knee,' and 'little nut' for the visual center), and *amygdala* (from Latin from Greek 'almond shaped' for a certain mass of gray matter at the back of the brain). These names reflect the extensive use of Latin roots in traditional medical terminology.

Descriptive names identify but do not explain purpose. They reflect how neuroscientists identify a physical structure before understanding what it does. To explore what is happening in to the brain of a living patient, neuroscientists may inject a radioactive dye and observe its path. They diagnose epilepsy by inserting electrodes into the skull for *electroencephalagrams (EEG*s) that show patterns of frequencies in these 'brain wave tests' labeled by the Greek letters *alpha, beta, delta,* and *theta*. They apply the descriptive information contained in some of their morphemes to determine the specific locations to place electrodes; they use their knowledge of a physical structure to determine how it works and what its problems are.

On the other hand, particle physics deals with invisible content known through its reasoned effects. These researchers question the causes of effects, reason logically to predict a possible explanation, and then plot a way to show the effect of the proposed explanation, perhaps with large expensive cyclotrons that record flashes of light when rare particles collide. Logical reasoning is a major tool, for their observations are even less direct than brain wave tests.

2. OVERLAPPING FIELDS SHARE OR DIVIDE TERMS. Scientists need research tools and terms from other fields, despite problems. Often words have different meanings in different fields. A British *billion* is a thousand times more than an American *billion*. The American Heritage Dictionary lists two general definitions for *nucleus* and eight others labeled for specific sciences: biology, botany, anatomy, physics, chemistry, astronomy, meteorology, and linguistics. Neuroscience needs biology, chemistry, electricity, physiology, and physics to explain *synapse* (from Greek 'join together'), wherein neurons secrete a neurotransmitter like dopamine to stimulate electrical responses in another neuron or cell.

Both neuroscience and particle physics involve terms from electricity. The affix *-ion* has a minor relevance in discussing the brain because in 1875 Broca wanted to use the end of *inion* as a suffix to name parts of the skull (e.g. *gnathion* and *gonion*). He knew that *inion* was Greek for 'the nape of the neck' and had been used in English since 1811 (OED). His choice lost out to *occiput* from Latin roots for 'against' and 'head,'

and now the *occipital lobe* at the back of the brain is the visual center. Both choices describe locations.

Michael Faraday had already utilized *ion* in 1834 when he was creating the electric motor and generator and needed to refer to a charged group of atoms. His source was Greek *ion* for 'something that goes,' from 'to go,' because an electric charge causes movement as ions travel from one electric pole to another. In 1891 -*ion* became a suffix added to the *electr-* root for the magnitude of charge of each bond in the atom[3], and in 1902 *ion* became the name for the stable elementary particle that is a constituent of all atoms. After -*ion* became a suffix in *electron, proton,* and *neutron*, it was used in naming many other particles such as *meson, muon, pion, lepton*, and *fermion.*

Particle physics formed nouns from adjectives or other nouns more than twice as often as neuroscience (24 v. 10). A conflicting choice of forms offends the physicist who complained that although *gravity waves* have a particular meaning in fluid dynamics, particle physicists had usurped that term rather than use what he called 'the more proper *gravitational waves*' (Visser 2001:834).

3. ADVANCEMENTS MAY IMPLY CHANGES. New developments require new vocabulary. Some fields are changing so fast that anyone who does not read the latest reports is lost. Journal writers quickly assume that their initiated readers understand new terms. Furthermore, while some names of particles change when they become better understood, as when *mu mesons* became *muons,* other names, such as *atomic bomb*, persist despite changes that contradict their roots. High school chemistry teaches that matter is made of molecules, which are made of *atoms*, which meant 'indivisible' in Greek in accordance with the ancient understanding that is now contradicted in what is called 'the Standard Model'. A newer refinement is *superposition*, being in two states at once, a concept which is difficult for some minds to accept.

4. TWO-WORD TERMS REPRESENT COMPLEXITY. Terms of more than one word frequently include the names of scientists, but understanding their significance requires knowledge of the history of the field. Particle physics was more than three times as likely to name a scientist involved (27 v 8): *Cerenkov counter, Higgs boson, Fermi-Dirac statistics, Fermilab, fermion,* and *fermium.* (In eponyms, capitalization is irregular.) Although Fermi himself was honored by having an element named for him, it was not the one that he discovered.

Many other terms consist of more than a single word. Neuroscience terms averaged 1.5 words, and terms in particle physics averaged 1.1. The latter, however, consisted of two or more nouns in sequence more than three times as often as did neuroscience terms (38 v 11, or 28% v 8%). Examples include *Down's syndrome* and *iconic memory* in neuroscience and *angular momentum* and *coupling constant* in particle physics.

Two word terms, which predominate, present special problems in both science and general usage, because there are many ways in which the words relate to one another. Is a *coupling constant* a constant that couples with something or the constant of interaction between couples. Is *iconic memory* the memory of icons or memory that works

by creating icons? It is not deducible that *a paper clip* holds paper, while *a paper plate* does not, and now is probably made of plastic. *Chaos theory* and *fuzzy logic* sound silly, but the terms have become so well known that general usage describes inaccurate or manipulative figures as *fuzzy math*. However, science needs terms to refer to what it deals with, and it must make calculations. *Chaos theory* deals with the movement of a particular particle that is so random that it cannot be predicted although the majority of the motion is clear; deterministic rules can result in disproportionate effects from a small change in starting conditions. Scientists need appropriate vocabulary for reasoning about random features to search for a pattern.

5. ACRONYMS AND NOMINALIZATIONS CONTRIBUTE TO BREVITY. Although acronyms were common in current journals, only occasionally did the glossary sample reduce terms to initials or pronounceable acronyms (6 and 4). A neuroscience example is *dopamine*, a chemical secreted by neurons and involved in memory, emotions, schizophrenia, and Parkinson's disease. *D+O+P+A* is an acronym for **d**ihydr**o**xy**p**he-nyl**a**lanine plus *amine* 'compound derived from ammonia'. It has nothing to do with *dope* from Dutch for 'sauce' (Gregory 1987: 519). Brain activity is studied with *electroencephalagrams (EEG*s), *magnetic resonance imagery (MRI),* and *positron emission tomography (PET)*. The *Stanford Linear Accelerator Center* is a research center known by the acronym SLAC, and the *Sudbury Neutrino Observatory* is referred to with the acronym *SNO,* pronounced as 'snow.' An anticipated theory that gets a great deal of discussion is called *supersymmetry* and is abbreviated to *SUSY*. It involves many dimensions and may reveal something about the relation between force and matter.

The clever brevity of terms in quantum physics is economical but not always clear. It seems to be a current contagious trend, used frequently for neurotransmitters now being discovered. Recent journals report many materials known by acronyms, such as *MuSK (**mu**scle-**s**pecific **k**inase,* Lin 2001), and *SLAM (**s**ignaling **l**ymphocyte **a**ctivation **m**olecule). *SLAM* is incorporated in another acronym, *SAP* for '*SLAM*-**a**ssociated **p**rotein'; it is also used as a verb in a headline, 'SLAMing T-cell differentiation' (*Nature* 2001b). Clever acronyms and shortened forms abound. A material that inhibits neurons in synapse is called *Nogo*. A midline repellant is called *Slit*, a '**s**ecreted **li**gand for the **t**ransmembrane protein *roundabout*', which is known as *Robo*.

These terms are too new for the standard glossaries consulted. Acronyms and other shortened forms of names are taught in technical writing courses as *COIK*, '**c**lear **o**nly **if** **k**nown.'

Another method of shortening the presentation of information utilizes nominalizations, nouns made from verbs, adjectives, or other nouns (examples from the acronyms and initialisms in the previous paragraphs include *emission, resonance, accelerator,* and *observatory* from verbs; *center* and *symmetry* relate to adjectives, and *imagery* comes from another noun related to a more common verb). Nominalizations of verbs condense information but omit the actor, time, and probability of the action. This condensation is efficient for those knowledgeable in the field but decreases readability tremendously for the uninitiated. An arbitrarily selected sample sentence from

a neuroscience article in *Nature* is at the extreme end of difficulty in readability statistics (Fournier 2001: 341).

> *Immunohistological* staining of chick embryonic spinal-cord cultures localizes the protein to *axons*, consistent with mediation of *axon-outgrowth* inhibition induced by *Nogo-66* (Fig.5d)[4].

The syntax seems simple (subject, one-word verb, direct object, no subordinate clauses). It has only one article but seven other modifiers, and five prepositional phrases. The lexical density is very high: 73% of the words carry content, including the 27% of the words that are nominalizations, one of which is a hyphenated two-word modifier. An earlier more extensive analysis compared parallel reports on neuroscience research in *Science* and found the same patterns of heavy use of nominalizations and prepositional phrases (Hartnett 2001). Specialists got shorter sentences in a more nominal style with tremendously more nominalizations and prepositional phrases than uninitiated readers saw in parallel articles reporting the news and significance of the same research. Reports for specialists in the field had simpler syntax but greater lexical density.

6. SPECIALIZED MEANINGS AND SYSTEMS HELP ONLY THE INITIATED. In neuroscience *sympathetic* has a specialized meaning[5], but particle physics narrowed the meaning of common words three times as often (53 v. 16). Examples include *element, field, fixed, spin*, and *string theory*. *Fixed* means set and unchangeable, and *spin*[6] is simpler than *chirality*, the left or right-handed orientation, from Greek for 'hand.' *String theory* is an untestable hypothesis involving single-dimension particles in a world of many more dimensions.

Compared to the vocabulary of neuroscience, particle physics had five times as many ordinary words that could mislead careful readers by narrowing, broadening, or otherwise changing the meanings of (11 v. 2). An *atom bomb* involves a nuclear reaction; a *black body* is a hypothetical perfect absorber of all incident radiation but is not black when heated; a *deuteron* is a positively charged subatomic particle which has only one neutron and one proton, although *deu* adds them together as two; and the *Manhattan project* was deliberately named to mislead for secrecy in developing the nuclear bomb. In German writing by Einstein and Planck in 1904, a *quantum* was the smallest unit of energy, from Latin for 'how great.' It is great only in comparison, but common usage today implies something significantly large.

7. WHIMSY IS CREATIVE. Particle physics had a wide range of types of terms. In the original statistics from the glossaries, it used 50% more learned terms than neuroscience did (49 v. 32), such as *path integrals, perturbation* and *resonance,* often with narrowed meanings. Slightly more of its terms (72 v. 60) were somewhat decipherable, such as *gluon, accelerator,* and *absolute zero*. It used a few metaphors, such as *excited state*[7] in addition to some whimsical terms like *quark. Quarks* are constituents of

hadrons and are composed of elementary particles bound together appropriately by *gluons,* which have neither mass nor charge. Metaphor and whimsy were absent from the original neuroscience glossaries.

Stories abound showing that the imaginations of the researchers are the best source credited for some names. The technical but whimsical name for some elementary particles is *quarks.* Murray Gell-Mann chose this term from a line in Joyce's novel, *Finnegans Wake,* 'Three quarks for Muster Mark!' In a scurrilous poem, a legendary king got complaints or birds' *squawks* (*quarks* as a standard verb, AHD). Gell-Mann altered the pronunciation to resemble *quart* as in a call at a pub or a bar for three quarts. The number three was relevant because only three such subatomic particles were known then; now there are six. These subparticles were called *aces* (by Zweig [Gribben 1998a: 158]) or *partons* (by Feynman), but those names did not stick. The six types of *quarks* are distinguished by a property called *flavor*. The flavors are *down, up, charm, strange, bottom,* and then *top*, which was predicted by theory but not discovered until 1995. The *strange quark* was named because it behaved in an unusual, unanticipated way.

Calling the types of quark *flavors* has no more basis than calling the study of their strong interactions *quantum chromodynamics*. Greek *chromo* means color. Certain types of forces of particles are labeled for colors as a method of classifying them, yet they have no color. The term *quantum chromodynamics* is abbreviated to *QCD* and parallels *quantum electrodynamics*, which is abbreviated to *QED,* which suggests to academic minds the Latin abbreviation, *q.e.d., quod erat demonstrandum,* 'which was to be demonstrated', meaning it is already proven so that further discussion is unnecessary. On another occasion, when Gell-Mann realized how particles could fit together in groups of eight, he called his classification scheme the 'Eightfold Way', irrelevantly and irreverently suggesting the Buddha's eight-step plan for righteous living. Such names persist, despite competing names and recognized problems.

CONCLUSIONS ABOUT THE LANGUAGE OF SCIENCE. Morphology has its reasons, but they are not sufficient to simplify or even explain science, especially particle physics, which seems least conventional. The lexicons of science are difficult not because of their morphology but because of the nature of scientific research. It is always unsettled and incomplete. The great physicist who reformulated quantum mechanics Richard Feynman said that objective evaluation requires not knowing the answer, and the purpose of knowledge is to appreciate wonders more (1999: 102–03). New knowledge can contradict what is already accepted. Just as we say the sun rises and sets without believing that it really does, scientists continue to use terms whose etymology no longer reflects the current understandings. As understanding develops, only some terms and systems of terms change. There is no rule.

Neither are there rules of how to form a name in many fields. As a result, many fields have had naming problems and are troubled by them now. Chemistry has more systematized names than most fields because it recognized the problem early and formed organizations in the nineteenth century to systematize nomenclature. Its

suffixes express very specific identities. Some outmoded terms, however, are too well established to be uprooted. Some biologists now want to scrap the system Linneaus described in 1758 because of the 'current understanding of evolution and biodiversity' (Pennisi 2001). Genetics also is having a serious problem with multiple names for the same gene or process and a single name applied to a dozen different ones (Niklas 2001). A Gene Oncology consortium (with the acronym *GO*) was formed to control the nomenclature (Pearson 2001). However, it must limit its work to names of functions, because it was already too late to eliminate the many problems with established names of genes or to stop the fun that geneticists have with the trend toward whimsical acronyms like *Shh* for the *Sonic **h**edge**h**og,* built on *hedgehog,* a protein mutation that participates in signaling. They named a genetically modified rhesus monkey *ANDi,* for 'inserted DNA' backward (Saltus 2001).

Glossaries and systems of naming are helpful even when they are incomplete or inconsistent. Although glossaries cannot include terms coined too recently to have become established, the lexicon of a field reflects its current working conditions and research methods. Glossaries cannot substitute for a broad, deep education. They are intended to cover a single field, but modern scientists must know more than their single field, and the borders are not clear. Research must be verified by repeated experimentation to overcome the possibility of various causes of a perceived result. The research methods and the interferences may involve outside influences. Neuroscience deals with living patients who respond to electrodes in accordance with the treatment and also with their own cultures and personalities, individual life histories, and interpersonal relationships with the staff. These are not explained in the encephalogram. Particle physicists have an even greater problem of not being able to view their invisible materials directly; they must reason back from perceived effects to create a hypothesis to explain them.

Scientists find brevity advantageous, even necessary in a complicated statement. They economize by using acronyms and nominalizations that may eventually find their way into the mainstream, whether they are sufficiently clear or not. Thus scientists get a reputation for being hard to understand. Only those who know a field and its history recognize the names of the predecessors whose shoulders they stand on; they find eponyms relevant and easy to remember, although they are sometimes only honorary, sometimes very precise, sometimes not.

Physicists love to tell stories of gaining insights during Eureka moments when they shout, 'Aha!' These moments often occur away from the lab, perhaps during a vacation when the scientist's mind is still almost unconsciously playing with the situation. The whimsical names that physicists coin show that they are clever human beings who enjoy puns and other sorts of humor. Physical scientists in all fields enjoy exchanging what they call 'engineer jokes' at family gatherings, other social affairs, and on a website (www.dctech.com/physics/humor.html). To be creative, scientists must use their imagination. Modern geeks are not all lonesome nerds, despite the stereotype. Creative researchers realize that teamwork and language are important. They enjoy their work and welcome new information, just as I enjoyed doing this research.

[1]   The *Higgs boson* is referred to informally as 'the God particle' because this hypothetical subatomic particle is believed responsible for all mass in the universe. It is named after the British physicist Peter Higgs, who theorized about relevant interactions. *Bosons* are particles that behave in accordance with *Bose-Einstein Statistics*. S. N. Bose (in India, 1894–1974) discovered a statistical rule and informed Einstein, who built on it to predict the *Bose-Einstein Condensate (BEC,* which may contain pointed excitations called *skyrmions*). If found naturally, BEC could be used to study supernovae. *Boson* was coined in 1947 when Dirac combined the name of Bose with *ion*.

[2]   The 'spooky' EPR paradox was named with the initials of the originators, Einstein, Podelsky, and Rosen (Gribbin, 1998a, 126–27).

[3]   *Electr-:* L from Gk 'amber,' which attracts bits of chaff when rubbed. In 1600, Gilbert distinguished electricity from magnetism by adapting the Latin word that Roger Bacon had used around 1250 as an adjective for *electric* (Fahnestock 1999).

[4]   *Immunohistological staining:* a neuroanatomical technique for studying synapse.

[5]   S*ympathetic:* Gk 'with' + 'emotion'; inhibiting physiological effects.

[6]   *Spin has* three different physics definitions in AHD (2000). The new field of *spintronics* uses spin to carry information. Cf. *chirality,* from L from Gk for 'hand,' the left or right handed orientation of the *spin* of a particle.

[7]   *Excited state:* ME from L 'set in motion'; physics metaphor, raised to a higher energy level.

REFERENCES

AHD. 2000. *American heritage dictionary of the English language*, 4th ed. Boston: Houghton Mifflin.

Cowley, Michael A., James L. Smart, Marcelo Rubinstein, Marcelo G. Cerdan, Sabrina Diano, Tamas L. Horvath, Roger D. Cone & Malcolm J. Low. 2001. Leptin activates anorexigenic POMC neurons through a neural network in the arcuate nucleus. *Nature* 411:480.

Danielson, Dennis. 2001. Scientist's birthright. *Nature* 410:1031.

Fahnestock, Jeanne. 1999. *Rhetorical figures in science.* New York: Oxford University Press. 178–93.

Feynman, Richard. 1999. What is and what should be the role of scientific culture in modern society. In *The pleasure of finding things out*, ed. by Jeffrey Robbins, 97–115. Cambridge ma: Perseus Publishing.

Fournier, Alyson E., Tadzia Grandpre & Stephen M. Strittmatter. 2001. Identification of a receptor mediating Nogo-66 inhibition of axonal regeneration. *Nature* 409: 341–46

Gregory, Richard L. (ed.) 1987. *The Oxford companion to the mind.* New York: Oxford.

Gribbin, John. 1998a. *Q is for quantum: Particle physics from A to Z.* London: Weidenfeld & Nicolson.

———. 1998b. *The search for superstrings, symmetry, and the theory of everything.* Boston: Little, Brown.

HARTNETT, CAROLYN G. 2001. When do sciences make nouns from verbs, and when don't they? *LACUS forum* 27:103–12.

JACKSON, CAROLYN (ed). *The Brain* (from the series *How things work*). 1990. Alexandria VA: Time-Life Books.

LAMB, SYDNEY. 1998. *Pathways of the brain.* Amsterdam: John Benjamins.

LIN, WEICHUN, ROBERT W. BURGESS, BERTHA DOMINGUEZ, SAMUEL PFAFF, JOSHUA R. SANES & KUO-FEN LEE. 2001. Distinct roles of nerve and muscle in postsynaptic differentiation of the neuromuscular synapse. *Nature* 410:1057.

MEĽČUK, IGOR. 31 July 2001. A Formal Language for Morphology. Inaugural Address, Twenty-Eighth LACUS Forum. Montreal.

NIKLAS, KARL J. 2001. Taxing debate from taxonomists. *Science* 292:2249–50.

Nature. 2001a. Blessings of mixed neutrinos. *Nature* 412:1.

——— . 2001b. SLAMing T-cell differentiation. *Nature* 411:xi.

OED. 2000. *Oxford English dictionary*, 2nd ed. CD-ROM, version 2.0. Oxford: Oxford University Press.

PEARSON, HELEN. 2001. Biology's name game. *Nature* 411:631–32.

PENNISI, ELIZABETH. 2001. Linnaeus's last stand? *Science* 291:2304–07.

SALTUS, RICHARD. 12 January 2001. Bioengineered monkey seen as research boost. *Houston Chronicle* from *Boston Globe.*

Science. 2001. Spin injection goes flat-out. *Science* 202:1445.

VISSER, MATT. 2001. Music of the sphere. *Science* 291:834.

WEINBERG, STEVEN. 1992. *Dreams of a final theory*. New York: Vintage.

WILCZEK, FRANK. 2001. Newton rules (for now) *Nature* 410:811–12.

WU, JANE Y., LILLI FENG, HWAN-TAE PARK, NECAT HAVLIOGLU, LENG WEN, HAO TANG, KEVIN B. BACON, ZHI-HONG JIANG, XIAO-CHUN ZHANG & YI RAO. 2001. The neuronal repellent Slit inhibits leukocyte chemotaxis induced by chemotactic factors. *Nature* 410:948–52.

❧

PLATO AND ARISTOTLE *VERSUS* WRITING

Earl M. Herrick
Texas A&M University, Kingsville

From its beginning, structural linguistics, in most of its many forms, has given short shrift to the study of written language. Saussure (1959:23 [1915]) says that 'language and writing are two distinct systems of signs; the second exists for the sole purpose of representing the first.' Sapir (1949:20 [1921]) says that 'written language is… a point-to-point equivalence… to its spoken counterpart. The written forms are secondary symbols of the spoken ones—symbols of symbols…' Bloomfield (1933:21) says that 'writing is not language, but merely a way of recording language by means of visible marks.' Hockett (1958:4) says that 'the linguist distinguishes between *language* and *writing*' [italics in the original]. Martinet (1964:17) says that 'the study of writing is a discipline distinct from linguistics proper'[1].

A few structural linguists do discuss writing more extensively. Gleason (1955:301) includes two chapters on writing, but says that 'many linguists consider all forms of writing [to be] entirely outside the domain of linguistics […] [although] many of the same methods of study can be used for dealing with both, and the structures revealed are in many respects similar'. Trager (1972:19), devotes half of his book to writing systems, but says that 'language comes first, and writing is at best a secondary symbol system of recent development'[2].

Structural linguists' insistence that they should study spoken material rather than written material[3] may have arisen from a reaction to the practices of the Neogrammarians and to the popular and school-grammar idea that written language is better than spoken language, but it would have been supported by certain statements in the works of Plato and Aristotle that are part of our Western cultural heritage. I want to suggest that these things which these philosophers said have become uninvestigated commonplaces[4] of our linguistic thought; because the theme for this year's forum is 'What constitutes evidence in linguistics?', I want to look at these things that they said and consider whether they are valid evidence for the nature of language.

1. PLATO. Plato's statement that is usually cited in opposition to a distinct linguistic status for writing occurs in his 'Phaedrus' (Plato 1952a:138–39), where he tells the Myth of Thoth. Plato says that he has heard a story about Thoth, the Egyptian god of writing, mathematics, and magic. In this story (as Plato tells it) Thoth tells the king of Egypt that he has invented letters, and tells the king that these letters will be a good thing for both memory and wit. The king tells Thoth that the letters which he has invented are a bad thing, and that letters 'will create forgetfulness in the learners' souls, because they will not use their memories', and that people who use letters

'will have not truth, but only the semblance of truth', and that they 'will have the show of wisdom without the reality'. Plato then goes on to say that writing is like a painting, which always appears the same but cannot answer questions that are asked of it, unlike 'an intelligent word graven in the soul of the learner', which can do so. Plato's interlocutor asks him whether he refers to 'the living word of knowledge which has a soul, and of which the written word is properly no more than an image', and Plato (1952a:139) says that that is indeed what he means. Plato (1952:140) also says that writing can be only a reminiscence of what we know, and that clearness, perfection, and seriousness are to be found only in those principles of justice, goodness, and nobility that are communicated orally and are graven in the soul.

Plato expresses the same attitude toward writing in his autobiographical 'Seventh Letter' (Plato 1952b:810), in which he says that if philosophical truths are to be put into words, the words must be spoken, not written, because 'no man of intelligence will venture to express his philosophical views in language, especially not in language that is unchangeable, [such as language] that is set down in written characters'.

To find out why Plato said these things, we must look both at the man Plato and at his times. When Plato was in his twenties, Athens went through two political revolutions within one year. It had been a democracy ruled by all of its citizens (that is to say, by all of its adult, free, native-born males), although its democracy was often led by members of its old, propertied families. In the year 404 B.C.E., Athens lost a war with Sparta, the democracy was overthrown, and power was seized by an aristocratic oligarchy called the Thirty Tyrants. Then, after a few months, the Thirty Tyrants were overthrown, and another democracy was established.

Plato was an aristocrat descended from some of the oldest and most aristocratic families in Athens. The Thirty Tyrants included both his mother's brother and her cousin, and Plato was asked to join them in ruling Athens, but he declined to do so because he saw how violently they were acting. Then after the Thirty Tyrants were overthrown and a democracy was restored, he found that the restored democracy was acting as violently as his friends and relatives the oligarchs had acted[5].

Plato therefore found himself at loose ends. In his 'Seventh Letter', he laments that he was unable to do what a young man of his background should be doing—namely, helping to run his country. Because he realized that a career in Athenian politics was not then open to an aristocrat like himself (Plato 1952a:801), he decided to travel, and as he did so he found his vocation of philosophy. Eventually he returned to Athens and opened a school (Gomperz 1955, vol.2:250–52, 254–59, 270; Guthrie 1975:10–19). But even as a philosopher, he remained an aristocrat opposed to democracy. In both his 'Laws' and his 'Republic', he envisions ideal governments in which the rulers (who will be few) will be philosophers and philosophers (who will be few) will be the rulers.

What was happening to the use of writing during these times? Simply put, in Hellenic society the general use of writing was a mark of democracy. In earlier times, when all the Hellenic city-states were ruled by aristocracies, writing had been limited to what has been called 'craft literacy'. There were some literate people who could be hired to write when writing was needed and could be hired to read when reading

was needed, but reading and writing were not things that people expected to do for themselves (Illich & Sanders 1988:22–23)[6]. In early aristocratic Greece, information was preserved not by writing, but by memory; and important information was preserved only in the aristocrats' memories. But after Athens became a democracy, beginning with Solon's constitutional revision in the early 6th century B.C.E. (Plutarch 1952:73), public writing flourished, and everything from laws, treaties, and election results to public financial accounts were inscribed on walls for everyone to see[7].

A well-recorded example of this sort of thing happened in the history of Rome. Linguistically, Rome was not Greek, but culturally it was a Hellenic city, with a population that was divided into aristocrats and commoners. At first, information about the laws of Rome could be found only in the memories of the aristocrats. Eventually, the commoners rebelled at this arrangement; in the year 450 B.C.E. they succeeded in having the laws of Rome compiled and inscribed on the Twelve Tables, which were posted up publicly so that everyone could read them or else have them read to him by some literate friend (Boak 1955:78–79; Heurgon 1973:169–70). But even after the substantive law was publicly codified in the Twelve Tables, the procedural law could be found only in the memories of the aristocrats, and although a commoner might know what the law was, he did not know how to go to court and enforce it. It was only in 304 B.C.E. that Gnaeus Flavius, the son of a freedman and a client of the reforming aristocrat Appius Claudius, published, with the connivance of his patron, information that would let the commoners in on the secrets of how to apply the law (Boak 1955: 83; Heurgon 1973:197). In Rome, therefore, as in other Hellenic cities, the greater availability of readable facts about public affairs and the greater ability of people to read them were a part of the democratization of government.

We can see, therefore, that Plato was doing two things when he disparaged writing in his 'Phaedrus' and in his 'Seventh Letter'. First, he was not attacking writing rather than speaking as a means of communication; he was attacking writing rather than memory as a means of keeping records. Second, when he attacked writing, he was not being a neutral philosopher dispassionately seeking the truth; he was being a disgruntled aristocrat, complaining that he and his friends were no longer ruling Athens.

2. ARISTOTLE. Even though Aristotle was Plato's student, they were very different people. Plato throughout his entire life was aware that he was an aristocratic Athenian, and he lived in Athens whenever he could. Aristotle was born into a middle-class family in Stagira, a small Hellenic city at the northern end of the Aegean Sea, and he did not identify himself with that city. Stagira was located near Macedon, a semi-Hellenic kingdom lying north of Greece. Aristotle's father was court physician to the king of Macedon who was the father of Philip II and grandfather of Alexander the Great, and Aristotle grew up around the Macedonian royal court. Eventually Aristotle settled in Athens and opened a school there, but only because it was the cultural center of the Hellenic world (Gomperz 1955, vol.4:19–25).

Aristotle's statement that is usually cited in opposition to a distinct linguistic status for writing occurs in his 'On Interpretation', where he says that 'spoken words are the

symbols of mental experience, and written words are the symbols of spoken words'
(Aristotle 1952:25). Aristotle may well have said this simply because, at the time that
he said it, it was an accurate statement of how writing was used for communicating in
the Greek language. In Greco-Roman times, people normally read aloud, even when
they read to themselves; they did not expect to read silently. There were exceptions.
Some passages in the plays of Euripides and Aristophanes assume that silent read-
ing is being done, even though the practice may have been unfamiliar to the plays'
audiences (Svenbro 1993:163–64). And there are two well-known Roman examples
which show how unusual it was then for people to read silently to themselves. Saint
Augustine says in his 'Confessions' that he was amazed to discover that Saint Ambrose
(the bishop of Milan who died in 397 C.E.) could read to himself without pronouncing
the words aloud (Augustine 1952:35). And Plutarch records twice an incident in the
life of Julius Caesar: At a meeting of the Roman Senate during the Catiline conspiracy,
a letter was brought in and given to Caesar, which he read silently to himself, and
Cato, who prided himself on being the epitome of Roman virtue, demanded that the
letter be read out aloud so that everyone could hear what Caesar was up to. This inci-
dent, which was recorded (Plutarch 1952:628, 804) because the letter was a love letter
to Caesar from Cato's sister, points out how unusual and therefore how suspicious it
was then for anyone to read a document silently, thereby depriving the bystanders of
their chance to hear what was written in it.

It can also be argued that the inscriptions on ancient Greek statues were not
intended to be read silently, because their grammatical forms only make sense if the
statues themselves are assumed to be speaking. Apparently they were designed so
that, if people were looking at a statue and a literate person were present, that person
would read the inscription aloud, thereby making audible the voice of the statue itself
(Svenbro 1993:ch. 2)[8].

There is, however, an important difference between written documents as they
were produced in Greco-Roman times and written documents as they are produced
now. In those earlier times, almost all writing was done without any spaces or mark
between words, in the style called 'scriptio continua'[9]. In order to read such a text, a
reader might have to pronounce strings of letters aloud and use his/her subconscious,
native-speaker knowledge of the language to try out the various ways of grouping the
letters of those strings in order to figure out just what words they represented (Sven-
bro 1993:45, 166–67). But this would be no trouble for those who read such writing if
they were native speakers of the language in which it was written.

The modern practice of regularly writing spaces between all of the words in every
text seems to have started in Ireland in the 6th century C.E.[10] In those years, there were
two things about Ireland that made it an unusual place. One was that the Irish were, in
their time, the only really literate nation in Western Christendom. The rest of Europe
was going through the real Dark Ages; the Western Roman Empire had fallen to the
barbarians, and Charlemagne's efforts to revitalize learning were a couple of centuries
in the future. In the monasteries of Ireland, however, there was a general practice of
scholarship; throughout Ireland manuscripts were regularly studied and copied in

Latin, in Greek, in Irish, and even in Hebrew (Cahill 1995:158–60, 183). 'During several centuries it was said that if any man in Western Europe knew Greek he must be Irish-born or Irish-trained' (Hannah 1925:102).

The other unusual thing about Ireland was that it was the only part of Western Christendom which had never been part of the Roman Empire[11], and the Irish therefore spoke a vernacular language that was not a Romance, Latin-derived dialect. In most of Western Christendom in those times, people who wrote anything at all wrote it in as good Latin as they could manage, and they read by pronouncing the writing aloud in their own Romance dialect, which would be close enough to the written Latin that they could use their subconscious, native-speaker knowledge to figure out what the written Latin words were, even if the text were written without breaks between the words. In Ireland, however, everybody who knew Latin had learned it as a second language, and no one had a subconscious, native-speaker knowledge of it. In order to read written Latin, the Irish therefore had to have texts that were divided up into the words which they had learned, so that they could identify those words (Illich & Sanders 1988:46).

Then, in the next few centuries, it was Irish missionary monks who spread Christianity and literacy across northern Europe (Hannah 1925:104, 177–79; Cahill 1995: 170–71, 183–84). They started on the western coast of Great Britain; the part of that island which had once been within the Roman Empire had been overrun by non-Latin-speaking pagans, and a Romance dialect was no longer the vernacular language there. Beginning in 564 C.E. with Saint Columcillie (known in Latin as Columba), Irish monks came to Great Britain and christianized it, starting with Northumbria, the northernmost English kingdom (Cahill 1995:200), which became the intermediate step in the expansion of the Irish literary and scribal tradition to the rest of England and thence to the European continent. Many of the leading missionaries of northern Europe in the 7th and 8th centuries C.E., including Saint Columbanus in France and Italy, Saint Willibrord in the Netherlands, and Saint Gall in Switzerland, came from Ireland or from Northumbria (Cahill 1995:188–96, 205–09). (Saint Boniface, originally named Wynfrith, was from Wessex, but by his time the Irish influence had spread beyond Northumbria to all of England.) Later, Charlemagne would send to the Northumbrian capital of York in order to get Alcuin (Illich & Sanders 1988: 59–60) to come to his court and lead the revitalized schools he was trying to establish in his empire. And, as these Irish and Irish-influenced missionary monks spread Christianity across northern Europe, they took with them the Irish practice of writing Latin with word breaks, which thereby became established as the usual European scribal practice. This practice of writing with word breaks was what made silent reading practicable.

We can see, therefore, that when Aristotle said that 'written words are the symbols of spoken words', he was simply describing how writing was used in his own day in the Greek language. What he said, however, has since become one of the uninvestigated commonplaces of linguistics, and when we have investigated the circumstances under which he said it, we see that it does not apply to writing systems which have word breaks.

3. CONCLUSION. We see that both Aristotle's and Plato's statements which seem to support the idea that writing is a mere representation of spoken language are invalid or are no longer valid. We therefore see that these statements cannot be used as arguments against the idea that we should regard writing as a distinct part of a human linguistic system for literate human beings.

---

1   Some of these linguists include short discussions of writing, but only because writing is needed for recovering the earlier pronunciations of language or because of spelling pronunciations, which are seen as results of extra-linguistic influence.

2   Two groups of linguists in these years did not give the same sort of primacy to spoken language; possibly for this reason, they were not usually regarded as 'structuralists'. The linguistic theory of Hjelmslev and his colleague Uldall, which was given the name 'glossematics', assumes that languages have highly abstract expression structures which can be represented equally well in various media for the sake of communicating. 'The [expression] elements in a linguistic structure may be represented in any way whatsoever, provided only that the elements required by the structure are kept distinct. The elements may, for example, be represented graphically, with each element having its own letter. So long as the letters are distinct from one another, they may have any shape desired… The elements can also be represented phonetically, each element by its own sound, no matter what, so long as it is sufficiently distinct from the others… The manual alphabet of deaf mutes is another special way of representing the expression elements of a language' (Hjelmslev 1970:40–41). Vachek and his colleagues, who were known as the 'Prague School', assume that any written language has separate written and spoken norms. Vachek (1973:9–39) gives a survey of what various linguists have said about the relationship between writing and speaking.

3   The difference of opinion as to whether writing is part of language or peripheral to it, and therefore whether the term 'grapheme' means something inside language or something outside language that refers to something inside it, lies behind the controversy in Daniels 1992, Herrick 1995a, Daniels 1995, and Herrick 1995b. See also Lockwood 2001 and the terminology that is given by Kohrt 1986, to which Lockwood refers.

4   Every science probably has its uninvestigated commonplaces: things which 'everyone has always known' and which the scientists in that field assume and accept without thinking about them. Whenever scientists express new ideas, there is a chance that some of the uninvestigated commonplaces of their field will accidentally slip into the ways they formulate their new ideas. It is therefore helpful to any scientific field to point out some uninvestigated commonplaces that may slip into its work. An example of a difficulty raised by an unexamined commonplace can be found in the history of astronomy: Copernicus believed that the Earth and the other planets traveled around the Sun; however, because 'everyone knew' that the heavens, which are composed of the quintessence, are ideally pure, and 'everyone knew' that the ideal geometrical form is a circle, Copernicus believed that their orbits around the sun had to be circular or at least describable by combinations of circles. He therefore postulated epicycles for their orbits that were almost as complicated as those of Ptolemy's Earth-centered system (Koyré 1971:26–27, 548–61). It took several decades for Kepler to figure out how the Earth and the other plants really go around the Sun, and that they travel in ellipses, not in circles (Pannekoek 1961:240–41).

5   This became clear when, among many other things that happened, Socrates, who was Plato's teacher, was judicially murdered by the restored democracy. Socrates himself was a

stonemason, a middle-class artisan, but he had been the friend and teacher of some of the Thirty Tyrants, including one of Plato's relatives, and the leaders of the restored democracy did not forgive Socrates for his association with those aristocratic oligarchs.

6   Writing as 'craft literacy' functioned very much like present-day stenotypy. Nowadays, if one needs to have a record made of what is said in court or in a deposition (and if one cannot use a video camera), one hires a stenotypist who has the skill to create that kind of written record. But everyone does not expect to be able to do it.

7   See, for example, the plates in Woodhead 1981.

8   I once had a student who functioned much like everyone did in Greco-Roman times. She had a real problem with studying, because she could not understand her textbooks when she read them; she simply could not understand any words that she had perceived by seeing them. What she did was to read all of her textbooks aloud into a tape recorder (and after doing so she had no idea of what she had read). She then played the tape and listened to what she had read aloud, and then she understood what the textbooks said. We now think of this as a pathological condition, but people of Greco-Roman times read in essentially the same way: they read aloud, and they listened to what they were saying in order to understand.

9   There were forerunners. In Roman imperial times, monumental inscriptions and the works of Virgil were ordinarily inscribed with spaces or special marks between words. Saint Jerome (died 420 c.e.) sometimes inserted marks between words in his translations from Hebrew into Latin, and an early manuscript of a work by Saint Isidore of Seville (died 636 c.e.) put spaces between the words in its headings (Illich & Sanders 1988:46).

10 The Benedictine monks are often regarded as great copiers of manuscripts and preservers of literature; but at first this was not a part of their monastic vocation. Saint Benedict's *Rule* says nothing specific about book-learning, although it assumes that books will be available for the divine service and for edifying reading by those monks who are literate (Leclercq 1961:15–22, 28–30).

11 It has been said that in these centuries the Irish had a Christian culture which was somewhat modified in an Irish direction and that the rest of Western Christendom had a Greco-Roman culture which was somewhat modified in a Christian direction (Cahill 1995:148–49).

REFERENCES

ARISTOTLE. 1952. On interpretation. In *Aristotle I (Great books of the western world 8),* trans. ed. by W.D. Ross, 25–36. Chicago: Encyclopaedia Britannica.

AUGUSTINE. 1952. The confessions. In *Augustine (Great books of the western world 18)*, trans. by E.B. Pusey, 1–125. Chicago: Encyclopaedia Britannica.

BLOOMFIELD, LEONARD. 1933. *Language.* New York: Henry Holt.

BOAK, ARTHUR E.R. 1955. *A history of Rome to 565 A.D.*, 4th ed. New York: Macmillan.

CAHILL, THOMAS. 1995. *How the Irish saved civilization.* New York: Doubleday.

DANIELS, PETER T. 1992. Is a structural graphemics possible? *LACUS forum* 18:528–37.

———. 1995. Reply to Herrick. *LACUS forum* 21:425–31.

GLEASON, H. A., jr. 1955. *An introduction to descriptive linguistics.* New York: Henry Holt.

GOMPERZ, THEODOR. 1955. *Greek thinkers: a history of ancient philosophy*, trans. by G.G. Berry. New York: Humanities Press.

GUTHRIE, W.K.C. 1975. *A history of Greek philosophy.* vol. 4, *Plato.* Cambridge: Cambridge Univ. Press.

HANNAH, IAN C. 1925. *Christian monasticism: a great force in history.* New York: Macmillan.

HERRICK, EARL M. 1995a. Of course a structural graphemics is possible! *LACUS forum* 21:413–24.

———. 1995b. Reply to Daniels's Reply. *LACUS forum* 21:432–40.

HEURGON, JACQUES. 1973. *The rise of Rome to 264 B.C.*, trans. by James Willis. Berkeley: Univ. of California Press.

HOCKETT, CHARLES F. 1958. *A course in modern linguistics.* New York: Macmillan.

HJELMSLEV, LOUIS. 1970. *Language: an introduction*, trans. by Francis J. Whitfield (work first published in Danish in 1963). Madison: Univ. of Wisconsin Press

ILLICH, IVAN & BARRY SANDERS. 1988. *ABC: the alphabetization of the popular mind.* New York: Vintage Books.

KOHRT, MANFRED. 1986. The term 'grapheme' in the history and theory of linguistics. In *New trends in graphemeics and orthography*, ed. by Gerhart August. 80–96. Berlin: DeGruyter.

KOYRÉ, ALEXANDRE. 1973. *The astronomical revolution*. Trans. by R.E.W. Maddison. Ithaca NY: Cornell Univ. Press.

LECLERCQ, JEAN, O.S.B. 1961. *The love of learning and the desire for God: a study of monastic culture*, trans. by Catherine Misrahi. New York: Fordham Univ. Press.

LOCKWOOD, DAVID G. 2001. Phoneme and grapheme: how parallel can they be? *LACUS forum* 27:307–16.

MARTINET, ANDRÉ. 1964. *Elements of general linguistics*, trans. by Elisabeth Palmer. Chicago: Univ. of Chicago Press.

PANNEKOEK, A. 1961. *A history of astronomy*. London: George Allen & Unwin.

PLATO. 1952a. Phaedrus. In *Plato (Great books of the western world 7)*, trans. by B. Jowett, 115–41. Chicago: Encyclopaedia Britannica.

———. 1952b. The seventh letter. In *Plato (Great books of the western world 7)*, trans. by J. Harward, 25–36. Chicago: Encyclopaedia Britannica.

PLUTARCH. 1952. Lives of the noble Grecians and Romans. In *Plutarch (Great books of the western world 14)*, the Dryden translation. Chicago: Encyclopaedia Britannica.

SAPIR, EDWARD. 1949. *Language: an introduction to the study of speech*. (first published in 1921). New York: Harcourt Brace.

de SAUSSURE, FERDINAND. 1959. *Course in general linguistics*, trans. by Wade Baskin (first published in French in 1915). New York: Philosophical Library.

SVENBRO, JESPER. 1993. *Phrasikleia: an anthropology of reading in ancient Greece*, trans. by Janet Lloyd. Ithaca NY: Cornell Univ. Press.

TRAGER, GEORGE L. 1972. *Language and languages*. San Francisco: Chandler.

VACHEK, JOSEF. 1973. *Written language.* The Hague: Mouton.

WOODHEAD, A. G. 1981. *The study of Greek inscriptions*, 2nd ed. Cambridge: Cambridge Univ. Press.

A SOUND-MEANING RELATIONSHIP AS EVIDENCE FOR ERROR-CONTROL CODING IN LANGUAGE

John T. Hogan
University of Alberta

Pursuant to the theme of the nature of evidence in linguistics, the claim is made that the meaning-sound relationship of the linguistic sign shows evidence that the lexicon is guided by principles of error-control coding. In this paper, two complementary and traditional meaning-sound relationships, arbitrariness and iconicity, are reviewed, and it is proposed that a third and independent relationship holds between sound and meaning for basic linguistic signs, and that evidence for this can be found in English thesauri and in a cross-language study of the phenomenon. This third relationship follows from principles found in error-control coding, a major branch of information theory.

The seminal work in the area of error-control coding is Shannon and Weaver's *The Mathematical Theory of Communication* (1949). Cherry 1966 has an extensive survey of the ideas of information and coding theory applied to human communication in the same year as Chomsky's *Syntactic Structures* (which significantly changed the direction of research and theorizing in linguistics away from empirical communication models to more highly formal 'computational' models of language). During the same time frame, psychologists (Miller 1951, Garner 1962) applied the concepts of information theory to psychological processes, and some linguists (Hockett 1953 and Gleason 1961) made early attempts to extend the ideas of redundancy, information measure and coding to language. In subsequent decades, information theory and coding theory have been extensively developed as branches of applied algebra and geometry in mathematics and have been applied to communication systems in electrical and computer engineering (Hamming 1980, Hill 1991). However, with the focus on other sorts of linguistic description after 1960, the application of coding theory principles to language was forgotten.

1. ERROR CONTROL CODES. Error-control codes serve to combat two main sources of message error in a communication network. The first source of message error is caused by noise. Noise in its widest sense is anything that destroys a message encoded into a signal. Thus noise can be: (1) physical noise such as the humming of air conditioners; (2) physiological noise such as hearing loss at the receiving end or a speech impairment or slip of the tongue at the sending end; (3) neurological noise such as loss of attention due to metabolic changes, or cross-talk due to listening and thinking of something else at the same time; (4) sender noise due to misarticulation or hypoarticulation by a speaker; or (5) socio-cultural noise due to differences in

individual backgrounds. The second source of message error is caused by fading of the signal.

To combat noise, a message can be repeated more than once or some feature can be added to it to indicate that some part of it has gone missing. This added feature is called an *information check* (Beckmann 1972). In both cases the redundancy of the message is increased along with the length of the message. Some information checks for nouns are number, case, gender, and articles; and for verbs, tense, mode, and aspect.

Fading of the signal, the second source of message error, occurs physically when the signal is low in intensity. Psychologically, fading can be viewed as the loss of the signal due to limitations of short-term memory. To combat this type of loss, signals should be as short as possible or within the range of seven plus or minus two items. The distribution of word frequencies and length as described by Zipf's distribution (1965:28) follows from this. Long words of high frequency usually become shortened rather rapidly. Processes such as contractions, and grammaticalization also shorten the message.

Clearly, the pressures to combat deterioration of messages due to noise and to prevent fading in memory are opposed to each other: the first leads to message lengthening to increase clarity while the second leads to message shortening to increase economy. Thus, there must be optimization trends in communication systems to balance these pressures. This paper considers evidence for error-control coding as such an optimization feature in the mental lexicon.

2. TRADITIONAL APPROACHES: ARBITRARINESS AND ICONICITY.

2.1. ARBITRARINESS. In de Saussure's *Course in General Linguistics*, the linguistic sign is given as an association between sound (phonological) and meaning (content). His ideas about the sign, the fundamental unit in his language system, were subsequently developed into various semeological theories in Europe. In American linguistics, the basic unit comparable to the sign is the morpheme. De Saussure presented several properties of signs, including linearity, conventionality, arbitrariness, immutability of sound structure in the short term and mutability in the long term. The property of interest here is arbitrariness, i.e., the notion that the meaning of the sign (or morpheme) cannot be deduced from the sound (phonological structure). This idea was first examined in the dialogues of Plato. In his dialogue *Cratylus*, two opposing views are presented. One consists of the thesis that there is a physical connection between sound and meaning—that once the meaning is determined the sound structure must necessarily be determined. The antithesis is that there is no necessary connection between sound and meaning, merely an arbitrary connection, and Plato argues through his dialogue that this is the correct relationship. This position was held for two millennia and was repeated by de Saussure. De Saussure offered examples from European languages. For example 'dog' in French, Spanish, Irish, Latin, German, and Russian is *chien, perro, madra, canis, Hund*, and *sobaka*. The phonological sequences are highly dissimilar for languages even from a single typological group and geographically close to each other.

Arbitrariness allows speakers of a language to innovate new linguistic forms without any constraints, at least from a causal perspective. The number of morphemes

in a language can potentially approach infinity. However, psychological constraints such as limits on the size of verbal long term memory and the counterbalancing role of syntax keep the average mental lexicon to roughly 80,000 morphemes and words. One of the most important features of arbitrariness is thus the maintenance of openness in the mental lexicon (see Aitchison 1994:7). Another is the freedom to innovate new lexical forms, as has been witnessed in conjunction with technological innovations in the past century.

2.2. ICONICITY. More recently, a counterpoint to arbitrariness has been heard under the title of iconicity. Iconicity plays an important role in discourse, for example in the principle that the unmarked order of mention follows the order of actual events. Also, the proximity elements in a construct mirror the closeness of relationship in the world. Iconicity at this level indicates a correspondence of the syntactic order with relationships of mental events. For example, the proximity of *kick* and *wall* in *The horse kicked the wall* suggests that the wall was affected more than it would be in such a sentence as *The horse kicked at the wall*, where *kick* and *wall* are farther apart. So, for English constructions of the above type, affectedness as conceptual distance and linguistic distance appear to be correlated (Haiman 1985).

Moreover, but to a lesser degree, there has been an interest in iconicity as a sound-symbolic process. At a basic level, some sounds have a symptom function, such as a cough or hiccup, or may indicate emotional states, such as *ugh*. Still others may have a vocative function, such as a throat-clearing or an *ahem*. These sounds are used to gain attention and are rarely incorporated in any syntactic construction of a language. At a less basic level, certain words can be sound imitative signs such as the imitation of animal sounds or sounds in the environment such as rapping or whistling. Words such as *bow-wow*, *meow*, *tap*, or *hiss* are somewhat conventionalized and follow the phonological patterns of a language. However, there are also less conventionalized imitative sounds such as howls used by a biologist to evoke responses from wolves or honks to attract geese, etc. The latter are not entered in dictionaries by lexicographers.

Synesthetic sound symbolism is the vocal communication of phenomena from the visual, tactile and proprioception properties of objects. For example, the vowel [i] has been associated with diminutive referents and the vowel [a] with larger objects, as in *slit-slot*. Hinton et al. 1994 claims that the differences in vocal tract length in the production of these vowels is shorter for [i], which is closer to a child's vocal tract length, and longer and more horn-like for [a], which is closer to adult length. Vowel lengthening can also be used iconically to indicate size. When the vowel in *long* is elongated considerably beyond its normal length, the suggestion is that an object is very, very long. Another example, using fundamental frequency, can be demonstrated with the word *high* in the sentence *The moon is high in the sky*. If a speaker pronounces *high* with a high fundamental frequency, the suggestion is that the moon is very high in the sky. These examples use acoustic signals to indicate the visual property of length and height.

Iconicity and arbitrariness can be thought of as forming a scale, and cases may fall somewhere between the two extremes. Phonesthemic words such as *glitter, gleam,*

glisten, glow, and *glimmer* clearly contain phonemic segments that are language specific and carry a common semantic connotation. Rhodes 1994 classifies onomatopoetic words into a scale, from a wild end, with purely imitative sounds, to a tame end, with words such as *quack, hoot, moo.* In the middle he has semi-wild words which include many phonesthemic cases. Sound imitative words in this class can be analyzed into those that are associated with abrupt onsets such as *peep*, *beep, plink, click,* and *creak*, which all begin with stops. Words like *thump, whack*, and *yap* connote sounds with poorly resolved onsets. They begin with fricatives or approximants. The vowels in words such as *peep/pop*, *clink/clunk*, *jingle/jangle* are associated with high and low frequency sounds, which correlate with the average formant frequencies of these vowels, i.e., the average for [i] is higher than for [a]. Last, the final consonants or consonant clusters are associated with the decay of the sound. Words such as *bong, boom, crunch, crash*, have long decays, whereas *pop, tweet, click* have abrupt decays. This category of iconic words has a large number of members and it is unclear whether they should be analyzed as a recurring partial in morphological analysis.

The extent of iconicity in the lexicon is an open question. Apart from polysemous and morphologically derived forms in the lexicon, arbitrary relationships between sound and meaning appear to predominate.

3. THE SOUND/MEANING RELATION AND ERROR CORRECTING CODES

3.1. THE GENERAL PRINCIPLE OF ERROR DETECTION. The main thesis of this discussion is that there is yet a third relation between sound and meaning. The principle is as follows: words that are close in meaning should be phonetically dissimilar, whereas words that have no semantic connection may be phonetically similar or even identical. However, if this principle were a rigidly implemented rule, English would not have the antonyms *cleave* 'adhere' and *cleave* 'split' or the confusion between semantically related *lie, lay, lain* and *lay, laid, laid*. This principle, then, must be considered only a tendency across languages. The idea behind the principle comes from information theory, especially the theory of error-control coding. The goal of such codes is to minimize the risks of error by placing signals that could potentially be confused far from each other in signal space. For example, the word *help* does not have many actual words close to it. This can be seen by using the phoneme commutation test for each segment. The commutation test generates 50 possible words that differ from *help* by one segment. Seven are actual words, namely, *kelp, whelp, yelp, held*, *health*, *hemp*, and *helm* and 43 are nonsense words. It is important that actual words in the mental lexicon should be surrounded by nonsense words in signal space. The reason for this is that if a word is mispronounced or misheard, then the output or input will not result in an actual but unintended word. It will be a nonsense word which will be detected by the speaker or hearer as an error and thus the message can be sent again. Furthermore, the greater the interword distances are, the less likely it is that a speaker's misarticulations, hypoarticulations, dialectal variations or foreign accent will be confused with other words by the hearer. All of these articulations may be interpreted as departures from a local norm, but they would not be heard as another unintended

word production. It should be noted that syntactic context and the non-linguistic context also assist a hearer in error detection plus error correction. If someone yells out *kelp me* in a swimming pool, the hearer may know that *kelp* is a noun and therefore cannot be an imperative. Given the context of the swimming pool, the speaker will rapidly deduce that someone needs help.

Given that actual lexical items should have near neighbors that are nonsense words, severe restrictions are placed on the arbitrariness of the sound-meaning relationships. For example, if an innovated word for a new hue of yellow is required, then one should not select *yollow, yeelaw, rellow*, etc. as the new phonological form, since the new related hue may sound like a mispronunciation of *yellow*, which is not what was intended. The word *saffron* would be a better choice.

3.2. EVIDENCE FROM THESAURI. Where does one see evidence of the above error-detecting principle? First, one may look in the classical sources of semantically similar items, the thesauri. Synonyms, most importantly, but also antonyms and hyponyms, form classes of semantically related words whose phonological closeness can be examined. Synonyms that are highly semantically similar, polar antonyms that are opposites, and hyponyms that are members of a class, should all be phonologically dissimilar. For example,

Synonyms:
    (1)   easy: simple, effortless, straightforward, uncomplicated, facile, light, smooth
    (2)   melt: liquefy, thaw, fuse, dissolve, deliquesce, soften
    (3)   crest: top, apex, summit, pinnacle, peak, ridge, crown, head
Polar antonyms:
    (1)   easy: difficult
    (2)   melt: freeze
    (3)   crest: foot
A hypernym/hyponym set:
    woodwind: *bassoon, clarinet, cor anglais, English horn, flute, oboe, piccolo, recorder*.

By casual inspection, we see that the above semantically related items are all quite phonologically dissimilar. Inspection of the *Penguin Dictionary of English Synonyms and Antonyms*, which has roughly 12,000 entries, and the *Oxford Thesaurus of Current English* which has roughly 9,700 entries with 150,000 synonyms, supports the major idea that most synonyms, antonyms, and hyponym sets are quite phonemically dissimilar to their primary word entry and to each other. These books do not include polysemous words which are direct extensions via metaphor or image schema. However, derivational relatives (paronyms), are entered as synonyms. For example*, dominate* has *domineer* as a synonym. Most derivations will necessarily be phonemically close in distance and provide an exception to the above coding principle.

Homophones are the best cases exemplifying the coding principle that if words are phonemically similar then they should be semantically dissimilar. However, before inspecting some examples, one has to exclude homophones resulting from a derivational relationship. Words such as *entrance* and *entrants*, *adulteress* and *adulterous* would have to be excluded. Their closeness in phonemic distance is due to the shared root morpheme and the homophony of the derivational affixes.

Examples of non-derivational homophones with their meaning are:

(1) *fence*   'barrier'
     fents   'cloth remnants'
(2) *chute*   'narrow passageway'
     shoot   'discharge a weapon'
(3) *roe*   'fish eggs'
     rho   '17th Greek letter'
     row   'propel a boat'
     row   'order series of objects'
(4) *ait*   'little island'
     ate   'consumed food'
     eight   'cardinal number'

It may be noted that, in many cases, these homophones are members of different lexical classes and vary as to frequency of occurrence. Therefore they usually do not contrast with each other in the same syntactic paradigm.

Another class of phonemically similar words is rhymes. In English many rhymes are a result of derivational suffixing, compounding of major lexical classes and phonesthemic groupings. The choice of monosyllabic rhymes provides more instances of the same principle. Inspecting the rhymes *boon*, *croon, dune, goon, hewn, June, loon, moon, noon, prune, rune, soon, spoon, swoon, strewn*, and *tune*, we again note that none belong to any recognizable semantic group. However, some do belong to the same part of speech.

Thus inspection of several large glossaries appears to support the general principle of error correcting coding—that signals that are semantically close maximize their distance across the signal space. They are like lexical magnets of the same polarity.

3.2. CROSS-LANGUAGE EVIDENCE. Last, we may ask whether the above principle holds typologically. Twenty-one dictionaries and one grammar were surveyed in order to obtain words for twenty-eight body parts (meronyms). The twenty-three languages, including English, were sampled from sixteen macro-families. In a few cases, two languages were taken from the same group if they were geographically distant, such as Japanese and Turkish from the Altaic group. The languages used are listed in Table 1. below. The list of body parts was chosen as a demonstration set of semantically closely related words because it was thought that they would be universal. Some body-part words out of the twenty-eight could not be used, since they were morphologically

complex in some languages or periphrastic in others. Eventually a core list of nine words, namely, *eye, finger, foot, hair, hand, head, mouth, nose,* and *teeth* were selected for comparison.

A method for measuring phonemic distances among the thirty-six word pairs in each language yielded by the set of nine was based on a procedure from Vitz and Winkler (1973), who developed an algorithm of simple phonemic template-matching. Their procedure can be beset by some arbitrariness when words of different length are matched. In this study, the canonical forms of the words in each language were adhered to as closely as possible for the pattern match. A convention used by Vitz and Winkler for missing segments in a match was to place an asterisk in the empty position. For example, when a CV word is matched with a CVC word, the alignment would be CV* against CVC. A more involved case of alignment, for example, could be a word of CVCVC canonical form against a word of CCVCCV canonical form. There are four possibilities for inserting asterisks into the first word, namely, C*VC*VC, *CV*CVC, C*V*CVC, and *CVC*VC. The second word would have one asterisk inserted after the final vowel, i.e. CCVCCV*. The choice of one of the four depends on the phonotactics and canonical forms of the language in question. The resulting alignment would have seven positions for each word. Next, after alignment is made between two words, a distance is computed by counting the number of phonemic mismatches in the pair of words. In this example, if there are no identical phonemes for any position, the distance is 7.

Examples from the data are as follows for the words for 'foot' and 'hand': Hausa *ka¹*fa* 'foot' versus *ha¹nnu* 'hand', a distance of 4; Tibetan *kaŋpa* 'foot' versus *la:kpa* 'hand', distance = 4; Indonesian *kaki* 'foot' versus *tangan* 'hand', distance = 5 (note that *kaki* must be rendered as *ka*ki**); and Tamil *kaaladi* 'foot' versus *kai* 'hand', distance = 5 (note that 'hand' must be *kai****).

Table 1 reports the average distance for the thirty-six possible word pairs in each language in the first column and the average word length of the nine words in the second column. It should be noted that the average interword distance is greater than the average word length. This is in part due to the lengthening effect of the Vitz and Winkler template-matching technique. However, and perhaps more interestingly, it is also due to the fact that in tone languages, two words of equal length and identical segmental matches will still have a distance equal to 1 since the tone is different; the addition of a different tone does not change the word length. In information theoretic terms, this increases the noise resistance of the signal without adversely affecting the fading of the signal in memory. It should also be noted that in many languages, the body-part set has a mixture of words of different syllable lengths. The Vitz and Winkler procedure used to measure distance captures this only through segmental mismatches. However, the prosodic difference in syllable count (sonority) produces even more marked distance than the metric suggests. This leads to the conclusion that the actual perceptual distances among the words in the sets may be greater than those shown in Table 1 (overleaf).

Language	Average interword distance	Average word length	Language	Average interword distance	Average word length
Arabic	5.69	4.56	Japanese	3.50	3.11
Basque	4.18	3.89	Mandarin	4.67	4.11
Cree	5.22	5.22	Navaho	3.78	3.11
English	**4.14**	**3.44**	Sara-Ngambay	3.81	3.00
Finnish	4.82	4.33			
German	3.78	3.78	Swahili	3.96	4.33
Georgian	3.36	4.44	Tara-humara	3.89	4.00
Hawaiian	4.56	5.11	Tamil	4.19	4.22
Hausa	4.57	4.56	Tibetan	4.64	3.78
Hungarian	3.04	3.00	Turkish	4.14	3.89
Indonesian	4.81	4.89	Wik-Mungkan	3.58	3.78
Inuit	4.79	5.33	Yoruba	3.54	3.44

Table 1. *Cross-language comparison of average interword distance and average word length for nine body-part terms.*

In summary, Table 1 shows us that the sets of nine body-part words range from 5.69 to 3.04 in average distance, with English falling in the middle of the range, and from 5.33 to 3.00 in average word length, again with English in the mid range. For words of this length, the average interword distances are clearly very large. This indicates once more, this time using a methodology different from simple observation of thesauri, that words in semantically similar sets are widely separated from each other in phonetic space in the mental lexicon.

4. OTHER CONSIDERATIONS. Alternative approaches to the determination of distances among phonetic categories have been made by Shepard (1972). Using the confusion matrices based on masking and filtering experiments by Miller and Nicely (1955), Shepard computed distances among 16 English consonants from the confusion matrices and applied multidimensional scaling techniques to obtain underlying spatial dimensions which appeared to be similar to scalar distinctive features. Analogously, this process could be carried out for the 82 consonants and 25 vowels of the I.P.A. Subjects would have to be phonetically trained practitioners of the I.P.A. From masking experiments with several noise varieties and filtering with various settings, confusion matrices could form a standardized basis for estimating phonetic distance and calculation of dimensions of phonetic space. In another approach, Shepard also applied the techniques of hierarchical clustering analysis to determine groups of similar phonetic types and their interdistances.

5. CONCLUSION. This study invites us to develop finer measurements for sound differences and to initiate extensive speculation about the comparison of lexical items in

a semantic metric space. This latter would, in effect, be a method to develop a typology for semantics, which would then allow us to have a geometry of the lexicon. As a visual image, the lexicon could be viewed as a galaxy with the stars as actual lexical items, their mass as phonetic substance/semantic substance and their differences as the distances between stars.

REFERENCES

Aitchison, Jean. 1994. *Words in the mind*, 2nd ed. Oxford: Blackwell.

Beckmann, Petr. 1972. *The structure of language: A new approach*. Boulder CO: Golem Press.

Cherry, E. Colin. 1966. *On human communication*, 2nd ed. Cambridge MA: MIT Press.

Chomsky, Noam. 1957. *Syntactic structures*. The Hague: Mouton.

Garner, Wendell R. 1962. *Uncertainty and structure as psychological concepts.* New York: John Wiley and Sons.

Haiman, John (ed.). 1985. *Iconicity in syntax*. Amsterdam: John Benjamins.

Hamming, Richard W. 1980. *Coding and information theory*. Englewood Cliffs NJ: Prentice Hall.

Hill, Raymond. 1991. *A first course in coding theory*. Oxford: Clarendon Press

Hockett, Charles. 1953. Review of *The mathematical theory of communication* by C. L. Shannon & W. Weaver. *Language* 29:69–93.

Miller, George A. & Patricia E. Nicely. 1955. An analysis of perceptual confusions among some English consonants. *Journal of the Acoustical Society of America* 27.338-52.

Rhodes, Richard. 1994. Aural Images. In *Sound symbolism*, ed. by Leanne Hinton, Johanna Nichols & John J. Ohala, 276–92. Cambridge: Cambridge University Press.

de Saussure, Ferdinand. 1959. *A course in general linguistics*, trans. by Wade Baskin. New York: Philosophical Library.

Shannon, Claude E. & Warren Weaver. 1949. *The mathematical theory of communication*. Urbana: University of Illinois Press.

Shepard, Roger N. 1972. The psychological representation of speech sounds. *Human communication: A unified view*, ed. by Edward E. David, Jr. & Peter B. Denes, 67–113. New York: McGraw-Hill.

Vitz, P.C. & B.S. Winkler. 1973. Predicting the judges 'similarity of sound' of English words. *Journal of verbal learning & verbal behaviour* 12: 373–88.

Zipf, George Kingsley. 1965. *The psycho-biology of language: An introduction to dynamic philology.* Cambridge MA: MIT Press.

LANGUAGE REFERENCE WORKS CONSULTED

AULESTIA, GORKA & LINDA WHITE. 1990. *English-Basque dictionary*. Las Vegas: University of Nevada Press.

BEAUDET, GÉRARD OMI. 1995. *Cree-English English-Cree dictionary*. Winnipeg: Wuerz Publications.

CARROLL, JOHN B., PETER DAVIES & BARRY RICHMAN. 1971. *The American Heritage word frequency book*. Boston: Houghton Mifflin Company.

CHETTIAR, A. CHIDAMBATANATHA (ed). 1965. *English-Tamil dictionary*. Madras: Rathnam Press, University of Madras

ECHOLDS, JOHN M. & HASSAN SHADILY. 1975. *An English-Indonesian dictionary*. Ithaca: Cornell University Press.

FAHIR, IZ, H.C. HONY & A.D. ALDERSON. 1992. *The Oxford Turkish dictionary*. Oxford: Oxford University Press.

FERGUSON, ROSALIND. 1986. *The Penguin dictionary of synonyms and antonyms*. London: Penguin Books.

GOLDSTEIN, MELVYN C. 1984. *English-Tibetan dictionary of modern Tibetan*. Berkeley: University of California Press.

GVARJALADZE, TAMAR & ISIDOR GVARJALADZE. 1974. *English-Georgian Georgian-English dictionary*. Tbilisi: Ganatleba Publishing House.

HINTON, LEANNE, JOHANNA NICHOLS & JOHN J. OHALA. 1994. *Sound symbolism*. Cambridge: Cambridge University Press

HOBBS, JAMES B. 1993. *Homophones and homographs: An American dictionary*. Jefferson NC: MacFarland & Co.

JOHNSON, FREDERICK. 1969. *Standard Swahili-English dictionary*. Oxford: Oxford University Press.

KILHAM, CHRISTINE, MABEL PAMULKAN, JENNIFER POOTCHEMUNKA & TOPSY WOLMBY. 1986. *Dictionary and source book of Wik-Mungkan language*. Darwin: SIL Australian Aborigines Branch.

LÁSLÓ, ORSZÁGH. 1960. *Anglo-magyar szótár*. Budapest: Akadémiai Kiadó.

LLEWELLYN, W.A. et al. 1987. *Webster's compact rhyming dictionary*. Springfield MA: Merriam-Webster Inc.

MANSER, MARTIN H. ed. 1999. *Concise English-Chinese Chinese-English dictionary*, 2nd ed. Oxford: Oxford University Press.

PUKUI, MARY K. & SAMUEL H. ELBERT. 1986. *Hawaiian dictionary*. Honolulu: University of Hawaii Press.

SCHLEICHER, ANTONIA Y.F. 1998. *Je Ḱá ka Rorùbá*. New Haven: Yale University Press.

SCHOLZE-STUBENRECHT, WERNER & J.B. SYKES. 1997. *The Oxford-Duden German dictionary: German-English/English-German, revised edition*. Oxford: Claredon Press.

SCHÖN, JAMES F. 1968. *Dictionary of the Hausa language*. Farnborough Hants, G.B: Gregg Press.

SPOONER, ALAN. 1998. *The Oxford thesaurus of current English*, revised edition. Oxford: Oxford University Press.

THAYER, LINDA J. & JAMES E. THAYER. 1971. *Fifty lessons in Sara-Ngumbay*, vol. 1. Bloomington: Indiana University Press.

THIBERT, ARTHUR OMI. 1958. *English-Eskimo Eskimo-English dictionary*. Ottawa: University of Ottawa Press.

THORD-GRAY, I. 1955. *Tarahumara-English English-Tarahumara dictionary*. Coral Gables, Florida: University of Miami Press.

WUOLLE, AINO. 1961. *Englantilais-Suomalainen Sanakirija*. Helsinki: Werner Söderström Osakeyhitiö.

YABUKI, KATSUJI & SHOZA KUROKAWA. 1979. *English romanized dictionary for practical conversation*. Tokyo: Japan Travel Bureau Publications.

YOUNG, ROBERT W., WILLIAM MORGAN, Sr. & SALLY MIDGETTE. 1992. *Analytic lexicon of Navaho*. Albuquerque NM: University of New Mexico Press.

❧

TYPES OF EVIDENCE FOR A REALISTIC APPROACH TO LANGUAGE

Sydney M. Lamb
Rice University

THE USE OF THE WORD 'REALISTIC' in the title may suggest that I consider some approaches to language unrealistic. The suggestion is intentional. Linguists have commonly assumed the existence of various entities that can easily be shown to be fictitious or illusory, including especially 'language' itself, as that term is commonly understood—as representing a 'system' shared by members of a community. Such a belief has serious problems when confronted with reality, of which I shall mention just two: first, its assumption that different members of a community share the same system; second, its lack of any physical grounding. To consider the first problem briefly, it can easily be shown that every person's linguistic system is different from that of every other person. On the assumption that this point is fairly clear, I shall not elaborate here. But its consequences must be made explicit: It makes no sense for linguistics to consider as its goal the description of languages as such, since such objects have no existence in the real world. What does exist, then? The linguistic system of the individual human being. Each person has one, different from that of every other person, as already mentioned, and often including information commonly considered to belong to different languages.

Now what about the second problem, that of a physical grounding? The problem becomes altogether different as soon as we accept that our real object of study is the linguistic system of the individual, for such a system has a clear physical grounding, and it is neurological: as 140 years of aphasiology have shown, our linguistic information is in our brains, largely in our cerebral cortices.

We thus have a conception of what linguists ought to be investigating that differs sharply from what many linguists believe themselves to be investigating: *not* some illusory shared disembodied system, and certainly not the system of the 'ideal speaker-hearer', for that, too, is something which does not exist, but the linguistic system of a representative person, a neurocognitive system (Lamb 1999).

Even among the minority of linguists who accept that language has a neurological basis, some also believe in the existence of things such as rules of grammar, and words or morphemes as objects of some kind. Such rules and similar symbolic information would have to be included in that neurological basis; they would therefore have to be represented in the brain. But no one has ever found any evidence at all to support the belief that they are. The belief that there are words or morphemes or the like in the brain comes from an unstated and untenable assumption: that what comes out of a person's mouth must have been inside the person, even inside the person's brain, before it came out—as if the person were some kind of vending machine. The

alternative is to suppose that what a person really does is to produce words 'on the spot'—with the consequence that what is internal is not words or morphemes or the like, but the *means of producing* such forms.

The question we must consider is: what kinds of evidence can we find for the neurocognitive linguistic system of a person? It seems clear that we should include both linguistic evidence, including that relating to language processing, and neurological evidence.

Let us begin with linguistic evidence. There is a great deal of evidence that is routinely ignored by most linguists. Perhaps most obvious is the fact, observed throughout every day, that people are able to talk and to understand one another. Yet linguists almost uniformly neglect this evidence, working with theories of language that have no way of being put into operation for speaking and understanding.

I shall now argue that consideration of the whole range of linguistic evidence, like that mentioned below, makes it clear that a person's linguistic structure is a network, a system in which all the information is in the connectivity. The view that a linguistic system is a network of relationships was put forth already by Saussure early in the last century, and it was given considerable support by Hjelmslev, in work that was unfortunately not widely appreciated (1943/61), partly because it was difficult to understand. In fact, this idea was not fully appreciated even by me, although I became its champion during the sixties, until several years after I first became acquainted with his work in the fifties. In keeping with a corollary of the Whorf principle, that notation influences thinking, it was only when I started using a notation for depicting relationships directly, under the influence of Halliday's notation for his systemic networks, that I was able to appreciate that not only does a linguistic system have a lot of relationships among its units, but that when those relationships are fully plotted, the units as such disappear, as they have no separate existence apart from those relationships (Lamb 1970, Lamb 1999:53–62).

Aside from that argument, which I shall not repeat here, there are several additional kinds of linguistic evidence to support the view that a linguistic structure is a network of relationships. And when you put them all together, in my opinion, the evidence is overwhelming; and the alternative view, that uses rules and other symbolic information, seems quite unable to handle these kinds of linguistic evidence. It is important to appreciate that this view is arrived at and justified purely on the basis of linguistic evidence. It is only during the past ten years that I have seriously investigated whether the relational network theory is supported also by neurological evidence. I now enumerate some pieces of this linguistic evidence, a baker's dozen of items:

1. **Coexistent alternative analyses.** For example, *hamburger* (Lamb 1999:233–36). The network allows both *ham + burger,* and *hamburg + er* to be present.
2. **Multiple parallel interpretation of (many) complex lexemes** (cf. Müller 2000, Lamb 1999:184–97). For example, the Chinese compound *zhong* 'central, middle' plus *guo* 'kingdom' is the name for China; but in its interpretation it also, simultaneously and in parallel, means 'middle kingdom'.

3. **Disambiguation of ambiguous words using linguistic and extralinguistic context**. How connotations operate (Lamb 1999: 187–88).
4. **Phenomena involving association**, such as literary allusions (e.g., to *Hamlet* by quoting) and Freudian slips. For example, the statement *Something is rotten in the state of Florida* conjures up *Hamlet* to people acquainted with the play.
5. **Degrees of entrenchment of idioms and other complex lexemes**—accounted for by variability in the strengths of connections.
6. **Gradualness of learning**—related to degrees of entrenchment. In the learning process, connections get strengthened.
7. **Context-driven lexeme selection (unintentional puns)** (Lamb 1999:190–94). For example, the selection of *zoom* (as opposed to the expected *go*) in *Are you ready to zoom to the camera store?* (Reich 1985).
8. **The interpretation of puns and other cases of ambiguity**. For example, a talking duck goes into a bar, orders a drink, as says '*Put it on my bill*'.
9. **Complex associations in slang lexeme formation**. Eble (2000) gives the following example:

    > Sometimes sound provides the link in a set. With the popularity of African-American comedians came the form *ho*, a dialect pronunciation of *whore*, for 'a promiscuous woman'. The same sequence of sounds, spelled *hoe*, refers to 'an implement for tilling the earth', i.e. a garden tool. Thus *ho* and *garden tool* are current slang synonyms for 'a promiscuous woman' (Eble 2000: 509).

10. **Slips of the tongue** (cf. Dell & Reich 1980).
11. **Prototypicality phenomena.** The conceptual category BIRD, for example, includes some members, like ROBIN, SPARROW that are more prototypical than others, like EMU, PENGUIN. The effects have shown up in numerous psychological experiments using such evidence as reaction time for deciding whether an item is or is not a member of the category. The relational network model provides a simple and direct means of accounting for the phenomena, by means of two devices which are needed anyway to account for other phenomena: variation in the strength of connections (thus the property of FLYING is strongly connected to the category BIRD), and variation in degrees of threshold satisfaction. Strength of activation, strength of connections, and number of activated connections all contribute to the speed and degree to which the threshold of a node is satisfied. It is important to notice that although these phenomena have been discussed in the literature for years, no means of accounting for them other than by means of a network model has ever been proposed.
12. **Realistic means of accounting for speaking and understanding**. This one is of basic importance, and the widespread neglect of the obvious and

widespread evidence—that people are able to speak, and to understand one another, in real time—is actually shocking when you stop to think about it. How can linguists go on, year after year, neglecting this evidence? Surely, the fact that people are able to speak and to comprehend one another (if imperfectly) cries out for explanation. The relational network model, whose origin over thirty-five years ago was motivated partly by this overwhelming evidence, provides a simple and direct means of such accounting: by the 'travelling' of activation through the pathways provided by the network (Lamb 1999). How long will the linguistic community continue to suppose that the ability of people to speak and to understand requires no explanation?

13. **On-line cognitive processing in conversation**. This rich but neglected opportunity for study, again blessed by abundant but neglected evidence, has been explored by Cynthia Ford Meyer in her two prize-winning LACUS papers (1991, 1992), in her dissertation, and in a more recent publication (2000) (see also Lamb in press). Strangely and sadly, her work has not yet encouraged others to undertake similar explorations. Here I will give one example, not from her work but from my own analysis (Lamb 1999:202) of a courtroom exchange reported by Lederer (1987).

> Attorney: *Mrs. Jones, is your appearance here this morning pursuant to a deposition notice which I sent to your attorney?*
> Witness: *No. This is how I dress when I go to work.*

We can observe a number of phenomena that are readily accounted for by the relational network approach. The witness is evidently concerned about her appearance and believes that a woman's clothing contributes to her appearance. Beliefs are registered as conceptual subnetworks, and matters of present or ongoing concern register as weak activation in these networks. Such activation is increased by emotional stimulation. To this factor we add another: unfamiliar lexemes or locutions are likely not to provide much conceptual activation, if any, because the connections that would provide activation are weak or lacking. So the lexeme *pursuant* and the possibly unfamiliar expression *pursuant to a deposition notice,* although they were surely received by her phonological recognition system, probably didn't generate much activity in her lexico-grammatical system, therefore little or none in her conceptual system. In addition, any emotional affect aroused by someone's seeming to draw attention to her appearance would deflect attention that might otherwise be directed toward attempting to understand the passage beginning with *pursuant*. The factor of attention has a global effect on degrees of threshold satisfaction. As a result, that latter part of the sentence, which in an attorney's cognitive system provides strong contextual activation to one interpretation of the lexeme *appearance* (the intended one), fails to have such an effect in the woman's system, and the other interpretation has in any case already been activated by the time the phrase beginning with *pursuant* was received.

There is an opportunity for many more fruitful studies along the lines opened up by Meyer (1991, 1992, 2000).

In any case, this brief survey suggests that considerable linguistic evidence exists for the network model, impressive in its abundance and variety. And we should not overlook that these are all real phenomena that therefore must ultimately be accounted for by linguistic theory. The fact that most linguists have seen fit to neglect such data in the past is irrelevant. And, I might add, unfortunate. These phenomena all strongly support the network model. No one has ever given an indication that any alternative means of accounting for them is available. This fact alone might be seen as rather compelling evidence. Those who find it reasonable to believe that the brain stores rules of grammar, or words or morphemes or other symbols, or that it operates like a computer, need to rethink their position: such a belief must either be supported by some kind of evidence, or else it should be abandoned.

Now we are ready for the next step. As our linguistic systems are represented in our cerebral cortices, it would be nice also to have some neurological evidence. We can look at what is known from neuroscience to either support or cast doubt upon the model. It is also appropriate to consider how the alternative, a symbol-based model, stacks up against the neurological evidence. Never mind that it has not been fashionable to consider such evidence in the past. Enough is known now about the structure and operation of the brain to make neurological evidence part of the arsenal of linguistics.

Fortunately, there is a large amount of usable neurological evidence that bears on these matters. We can divide it into two portions, dealing with 'macro-structure' and 'micro-structure'.

At the level of macro-structure we have considerable evidence from 140 years of aphasiology relating to the presence in the cortex of different linguistic subsystems and their locations. Let me here mention just two of these findings. First, the hypothesis of stratification of linguistic structure is supported. This is the hypothesis that there are different subsystems for different linguistic levels or strata. It is supported by findings from aphasiology that, for example, phonological structure is distinguishable cortically from lexical structure and semantic structure. The second interesting point is that, unexpectedly for most linguistic theories, both of the network and the symbolic variety, there is a clear cortical distinction between two subsystems for phonological structure, one for phonological production, the other for phonological recognition. Thus the hypothesis of stratificational grammar (and other theories) that there is a phonological stratal system is seen to be in need of revision, for there are *two* stratal systems for phonology. As it happens, this revision is quite easy to make in a network theory, and it produces beneficial consequences for the theory's ability to account for phonological phenomena.

More important for the basic question of choosing between a network model and a symbolic model is the evidence at the level of micro-structure. Here we are concerned with the basic question of how linguistic information is represented in the brain. The question can perhaps best be introduced by considering some of the conceivable possibilities.

POSSIBILITY 1. I mention this one because it was current among lay people when I was a child. It was thus the first hypothesis that I considered in my lifelong philosophical exploration. It takes note of the many grooves in the cortex, called *sulci*, and proposes that these grooves store information in the way that the grooves of a phonograph record store information. Needless to say, no one takes this hypothesis seriously nowadays. But it is interesting to take note of one very interesting feature of this proposal: It results from a hypothesis-forming method that operates by metaphoric extension from present-day technology.

POSSIBILITY 2. This one results from the same metaphoric process, but as technology has changed, it is more modern. Its metaphorical basis is the computer, and the hypothesis is that the brain stores information in much the same way as a computer. In my opinion it is nearly as ridiculous as the phonograph record hypothesis, and has no more basis for reliability than that the computer is a more recent technology than the phonograph record. But why should Nature have evolved, over the millions of years, an information-processing device (the brain) that just happens to use the same technology as that which was invented during the latter half of the twentieth century? What a remarkable coincidence! Yet just this hypothesis is explored by some of those regarded as leading thinkers in neuroscience (see for example Churchland and Sejnowski 1992). According to the more naive versions of this hypothesis, information is stored as combinations of binary digits, or perhaps as other kinds of symbols. It is easy to think about and it fits well with our habits of thinking of information as consisting of symbols represented physically in some medium, since that is the way we have long been accustomed to treating information represented externally to our brains. On paper, on blackboards, in computers, information consists of symbols represented in some medium, paper or blackboard or electronically. But that doesn't make the hypothesis correct (Lamb 1999:114–16). In order to win neurological support, such a hypothesis needs to show that neurons or groups of neurons are capable of storing binary digits or other symbols. Moreover, it needs to show how such symbols are operated on in such processes as recognition and production. We know how recognition operates in a computer: it depends upon a process of comparison. Given an item to be recognized, any of various strategies is used to find a candidate among the items stored in the memory, and then this candidate is compared to the input item. If they match, successful recognition has occurred; if they do not, another candidate can be searched for; and so forth. Not to be overlooked is that such a process requires additional equipment not yet mentioned: a buffer in which to store the input item while the process is going on, a workspace in which to perform the comparison, and most important an executive device of some sort, a homunculus, which executes the process; that device thus requires some kind of knowledge of that process and how to carry it out. These features of the hypothesis are easy to overlook since we humans execute recognition and comparison of external symbols all the time, having learned the operations through thousands of instances of practice. We thus take them for granted and have to be reminded that they actually involve a great deal of information processing.

In any case, considerations like these must enter into the testing of any hypothesis of how the brain represents information.

POSSIBILITY 3. The third hypothesis I will mention is the network hypothesis. The second and the third are in competition, as the only two hypotheses that are seriously entertained nowadays. Actually, the network hypothesis comes in several varieties, as several forms of network have been proposed. The best known of them is often called connectionism or PDP (parallel distributed processing) (Rumelhart & McClelland 1986), although those terms appropriately apply to a whole range of alternative network hypotheses and not just to this one. That is unfortunate since this well-known hypothesis is among the most unrealistic. That is, it is among those most lacking in supporting evidence from neuroscience (Lamb 1999: 118–19). Even more unfortunate is that models of this kind are often referred to as neural network models, using a name that suggests a resemblance to real biological neural networks, even though they lack such resemblance. On the plus side of the ledger, however, they do share with relational networks a basic property that sets them apart from computer models: they do not store binary digits or symbols.

It may be instructive, before proceeding, to see how the network model handles recognition. Let us suppose that a word is being received by the system. If it is a spoken word, it will activate the nodes for its auditory features, and these will pass activation on up to higher-level nodes, perhaps representing phonemes. (I say 'perhaps' because we do not yet know what units the phonological recognition system operates with—if not phonemes, some other units, perhaps transitions from one phoneme to another—no matter, the process works the same way no matter what units are utilized.) These higher-level nodes, those activated by this particular word, in turn pass their activation on to a still higher-level node representing the word. It is the activation of that node that constitutes recognition of the word. Notice that no buffer is needed, nor any workspace, and most important, no executive device or homunculus. Each node in the network is its own processor, operating on a simple principle: when it receives enough activation to surpass its threshold, it passes activation on to higher-level nodes to which it is connected.

In order to test the model against the neurological evidence, we need a hypothesis of how the nodes of the network (called 'nections' in Lamb 1999) are represented physically in the cortex. Based on work by Mountcastle (1998), Burnod (1988/90), and Arbib et al. (1998), I have adopted the hypothesis that the node of the network is implemented neurologically as a cortical column (Lamb 1999:323–26). A cortical column, also known as a minicolumn, contains about 80–110 neurons on average (more in the primary visual cortex of primates), and extends through the six layers of the cortex. About 70% of the neurons in a typical column are pyramidal neurons, and the remaining 30% consist mainly of inhibitory neurons of various types and, in layer IV, spiny stellate neurons. The pyramidal neurons provide excitatory connections to other columns,

either neighboring or distant, while the inhibitory neurons provide inhibition to neighboring columns and within the same column.

We are now at a very important point in this journey, and it is helpful to be fully aware of how to use the available evidence as we assess our progress and plan the next steps. In keeping with standard scientific practice, it is important to ask certain questions as a means of testing a theoretical model:

First, is there any data that this way of looking at things handles better than extant models? Does it make better sense of the data than competing models do? From the examination of linguistic evidence surveyed above, we have obtained a resounding 'Yes!' Not only has no other model of language ever even attempted to handle most of the data considered; it is even difficult to imagine how they could be treated in any other way than by means of a network model.

Second, are there any predictions made by the model that can be tested, either by experiment or by observation? Another way of stating this point is to use the concept of falsifiability: What kinds of data would falsify the model? We can ask this question in the context of the columnar hypothesis. The relational network model requires that certain kinds of connections be present among its nodes, and that these connections have certain properties. The relevant properties, all arrived at through consideration of the linguistic evidence, as detailed in Lamb 1999, may be listed as follows:

1. Connections carry varying degrees of activation.
2. Connections can have varying strengths.
3. Connections get strengthened through successful use (the learning process).
4. Nodes have varying thresholds.
5. The threshold of a node can vary over time (part of the learning process).
6. Connections are of two types: excitatory and inhibitory.
7. Excitatory connections are bidirectional (feed-forward and feed-backward) (Lamb 2000).
8. Excitatory connections can be either local or distant.
9. Inhibitory connections are local only.
10. Inhibitory connections can connect either to a node or to a line.
11. In early stages (pre-learning) most connections are very weak (*latent*).
12. A node must contain an internal wait (delay) element (for ordered 'and' node).

It is important to keep in mind that all of these rather specific properties of the network are determined by linguistic considerations, *not* neurological ones. (In fact, the relational network hypothesis has been around now for over thirty-five years, and it is only during the last ten years that I have undertaken the study of neuroscience.) They are properties that are required by the need to account for the linguistic data and linguistic processes, including that of learning. They thus constitute predictions from linguistic theory about properties that must be present in the brain, if the relational

network hypothesis is correct. In terms of the falsifiability doctrine, if any of these properties is not present in the cortical columns of the cortex and their interconnections, then the hypothesis is thereby falsified.

What, then, do we find, upon examining the evidence from neuroscience relating to cortical columns and their interconnections? What we find is that *every one of these properties is present* in the minicolumns and their interconnections (Lamb 1999:321–29). By contrast, if we compare the properties of the Rumelhart-McClelland model with those of cortical columns and neurons, we find that some of them are falsified (Lamb 1999:118–19).

Let me continue by mentioning two additional kinds of supporting evidence.

First, with respect to the receptive side of linguistic structure. The major process involved here is perceptual. Now, to get detailed neuroanatomical studies of the cortex as it engages in the process of speech perception is not possible using any known and permissible methods. But the status of what constitutes 'permissible' is different if we consider other kinds of perception, as they are shared by other mammals. For example, cats and monkeys are also endowed with the capability of visual perception, and it is considered permissible to examine living brain tissues of cats and monkeys (Hubel and Wiesel 1962, 1968, 1977). Although I don't personally approve of such procedures, I will permit myself to mention some of their pertinent results. They find that visual perception in cats and monkeys works in just the way predicted by the network model for the receptive side of language. That is, it uses minicolumns as the basic nodes in a hierarchical network in which successive layers integrate features from the next lower layer. Similar findings have come from the examination of the primary somatosensory cortex and the primary auditory cortex (cf. Mountcastle 1998:165–203). As Mountcastle reports (1998:181), 'Every cellular study of the auditory cortex in cat and monkey has provided direct evidence for its columnar organization'. To be sure, this is indirect evidence, as it concerns auditory perception at lower levels than those involved in speech recognition. They haven't examined the cat's or monkey's linguistic processing since it is lacking. But it is important in this connection to observe that neuroscientists do consider it permissible to extrapolate beyond the cats and monkeys to the supposition that human visual, auditory, and somatosensory perception works in this same way. It is not much of a leap to suppose that speech perception also works this way.

Finally, we may bring quantitative evidence into the examination. Quantitative evidence is commonplace in physics but almost unknown in linguistics. Yet it has appropriate applications in linguistics. In particular, it is very important to apply a quantitative test of capacity. Such testing estimates the capacity provided by the model and compares it with that of actual people, for example vocabulary capacity. What we need to ask is whether it is realistic to assume availability of enough latent nodes, and in the right places, to get a person through a lifetime of learning.

Let us consider the area in which we have our phonological representations. Based on data from aphasiology and from brain imaging studies, it is reasonable to hypothesize that this subsystem is to be equated with Wernicke's area in the narrow definition

of that term; that is, the superior posterior left temporal lobe, including the planum temporale. According to the relational network theory, this area needs enough nodes to represent all the phonological units that might become known by a person, including syllables, phonological words, fixed phonological phrases, in as many different languages as a person is likely to be able to learn to speak with a high degree of fluency. A liberal estimate would be fifty thousand per language. If we multiply by twenty for a phenomenal polyglot with twenty fluent languages, we get a requirement of one million nodes.

For our falsifiability test, we need to estimate the number of cortical columns available in this area of the cortex, using neurological evidence. We can make a rough estimate by measuring the cortical surface of the area and multiplying by a reasonable estimate of the number of (mini)columns per square centimeter of cortical surface. In a typical person, the area in question includes the posterior portion of the superior temporal gyrus, extending also into the Sylvian fissure (the temporal plane) and the superior temporal sulcus, perhaps also into the middle temporal gyrus. The horizontal extent, as might be measured along the top of the superior temporal gyrus, might be three cm or more in the typical individual, and the extent in the orthogonal direction might include at least two cm of the temporal plane (in the Sylvian fissure), one cm or more for the superior temporal gyrus, and two cm for the superior temporal sulcus. For these rough measures we get a surface area of three or four cm by at least five cm, or fifteen to twenty or more square cm. The density of neurons is around eighty to one hundred thousand per square mm of cortical surface. To get the number per square cm we multiply by 100. But to get the number of (mini)columns, at 100 neurons per column, we divide by 100. So, as these two factors cancel each other, the figure of neurons per square mm is approximately the same as the number of columns per square cm. Using the figures at the ends of both ranges, we get

from 15 cm^2 × 80,000 columns/cm^2 = 1.2 million columns
to   20 cm^2 × 100,000 columns/cm^2 = 2.0 million columns

Thus we get from 1.2 million to 2 million nodes in this area, in a typical individual, let's say somewhere in the neighborhood of one-and-a-half million. There is an alternative estimate of six hundred columns per square millimeter—60,000/sq cm—of cortical surface, that would give between 0.9 million and 1.2 million. Either way the figures are rough but close enough for our purposes. We are evidently in the range of one to one-and-a-half million.

And so the quantities match up well—we have a requirement for one million nodes, and we have a capacity of around one to one-and-a-half million nodes—and this requirement is for a very liberal estimate of a person with phenomenal linguistic abilities. On the other hand, such a polyglot probably has a larger phonological recognition area than the average person, perhaps extending into the middle temporal gyrus. And actually, I have *over*estimated the requirement, since I have added together all of the requirements for phonological forms in all of the languages, disregarding

that many of them overlap from one language to another and can therefore share the same nodes—forms like those for *taxi*, *coffee*, and numerous technical terms.

The hypothesis seems to be supported. We have given it a test in which it could easily have been falsified, and it has passed.

Since there are those in the field of neurolinguistics who think it possible that the linguistic system of the brain is a symbol-based system, it is fair to ask whether they have conducted such a test for such models. They have not. Yet their faith persists. Why? I can ask this question, but I have no answer. But if we estimate on the basis of how lexical information is usually conceived of in such models, we have our figure of capacity for one million (from above) to be multiplied by the number of minicolumns or neurons needed to store each item. If the items are represented as combinations of distinctive features and if there are on average 40 features per lexeme and if it takes 100 neurons (one minicolumn) to store each feature, then we have a need of:

1 million lexemes × 40 columns per lexeme = 40 million columns

To emphasize how outrageously excessive this number is, we need only remind ourselves that 40 million columns amounts to about 4 billion neurons. Thus the symbol-based approach is decisively falsified. This is quite apart from the fact that no one has ever shown how a minicolumn could be used to store information. On the other hand, the proponents of such a theory might argue that it is the neuron rather than the column which stores the phonological features. But that theory is also fraught with problems. Presumably they would have to assume that it is the pyramidal neurons that have this function; the area in question has about 70 to 80 million pyramidal neurons. But there would be much to explain, for some of these pyramidal neurons are in the upper layers of the cortex, some in the lower layers, and the two groups have different connectivity. Also, with no redundancy, how does the system continue to operate in cases of occasional neuronal malfunction and death? Not to mention other problems, not least of which is that no one has ever come up with a reasonable theory of how a neuron can be used to store information.

Another quantitative test concerns the arcuate fasciculus, a fiber bundle which connects the phonological recognition area to the phonological production area. The relational network model predicts that this bundle has to have many thousands of fibers, perhaps hundreds of thousands, since it requires unique connections from all low-level nodes in the one phonological system to nodes in the other (Lamb 1999:366–67). Symbol-based and computational models, on the other hand, require only a few hundred fibers in the bundle, even with extensive redundancy. Here we have a test that sharply distinguishes the competing models, one which will clearly falsify the one or the other. And this prediction of the network model concerning the size of this fiber bundle really is a prediction, since up to now the number of fibers in the bundle has not been counted nor even reliably estimated in print. We should soon have results of this test—we await publication of a neuroanatomical study of this fiber bundle.

There is also quantitative evidence relating to processing. We know that people are not only able to speak and to understand, they are able to do so in real time. Any model has to meet the test of being compatible with this fact. With the network approach, such processing is straightforward, consisting of the spread of activation along pathways of the network, governed by the thresholds of nodes – and the model agrees with how recognition works in the primary visual, somatosensory, and auditory cortices. Contrast how recognition has to work in symbol-based systems, even if a plausible hypothesis were forthcoming about how neurons could store symbolic information. Again, no supporter of symbol-based systems has ever proposed, to my knowledge, any explanation of how such a system could be used in real time to recognize speech. In the absence of such a proposal, we may view the formidable difficulties of devising such a model as highly suggestive.

To sum up, I have mentioned several kinds of linguistic evidence, usually overlooked, which suggest that a person's linguistic system is a network. I have summarized, with references to the literature, the results of work examining these kinds of data, that has led to the determination of a set of specific testable properties of such networks. In the context of the hypothesis that the nodes of a relational network are implemented as cortical minicolumns, all of these propeties of relational networks are found to be present in the cortex. Additional confirmation is provided by a test of the capacity needed for the vocabulary of a polyglot. A further test based on the number of fibers in the arcuate fascilulus awaits neuroanatomical confirmation.

REFERENCES

ARBIB, MICHAEL A., PÉTER ÉRDI & JÁNOS SZENTÁGOTHAI. 1998. *Neural organization*. Cambridge: MIT Press.

BURNOD, YVES. 1988/90. *An adaptive neural network: the cerebral cortex*. Paris: Masson & London: Prentice Hall.

CHURCHLAND, PATRICIA & TERRENCE SEJNOWSKI. 1992. *The computational brain*. Cambridge: MIT Press.

DELL, GARY S. & PETER A. REICH. 1980. Slips of the tongue: the facts and a stratificational model. *Papers in cognitive-stratificational linguistics (Rice University studies*, vol. 66, no. 2), ed. by James Copeland & Philip Davis, 19–34. Houston: Rice University.

EBLE, CONNIE. 2000. Slang and lexicography. In Lockwood, Fries & Copeland 2000, 499−511.

HJELMSLEV, LOUIS. 1943/61. *Prolegomena to a theory of language*. (Originally published in Danish in 1943.) English translation by Francis J. Whitfield, Madison: University of Wisconsin Press.

HUBEL, DAVID & TORSTEN N. WIESEL. 1962. Receptive fields, binocular interaction and functional architecture in the cat's visual cortex. *Journal of physiology (London)* 160:106–54.

——— & ———. 1968. Receptive fields and functional architecture of monkey striate cortex. *Journal of physiology (London)* 195:215–43.

——— & ———. 1977. Functional architecture of macaque monkey cortex. *Proceedings of the Royal Society of London* B198:1–559.

Lamb, Sydney M. 1970. Linguistic and cognitive networks. In *Cognition: a multiple view,* ed. by Paul L. Garvin, 195–222. New York: Spartan Books.

———. 1999. *Pathways of the brain: the neurocognitive basis of language.* Amsterdam: Benjamins.

———. 2000. Bidirectional processing in language and related cognitive systems. In *Usage-based models of language*, ed. by Michael Barlowe & Suzanne Kemmer, 87–119. Stanford: CSLI Publications.

———. In press. Interpreting discourse. In *Relations and functions within and around language*, ed. by Michael Cummings, Peter Fries, David Lockwood & William Spruiell. London: Continuum.

Lederer, Richard. 1987. *Anguished English*. New York: Dell Publishing.

Lockwood, David G., Peter H. Fries & James E. Copeland (eds.). 2000. *Functional approaches to language, culture, and cognition*. Amsterdam: Benjamins.

Meyer, Cynthia Ford. 1991. "What shall we talk about next?": cognitive topic in the production and interpretation of conversation. *lacus forum* 18:85–98.

———. 1992. Twice-told tales: aspects of the storage and expression of personal experience. *lacus forum* 19:63–74.

———. 2000. Cognitive networks in conversation. In Lockwood, Fries & Copeland 2000, 253–66.

Mountcastle, Vernon B. 1998. *Perceptual neuroscience: the cerebral cortex*. Cambridge: Harvard University Press.

Müller, Ernst-August. 2000. Valence and phraseology in stratificational linguistics. In Lockwood, Fries & Copeland 2000, 3–21.

Reich, Peter A. 1985. Unintentional puns. *lacus forum* 11:314–22.

Rumelhart, David E. & James L. McClelland. 1986. *Parallel distributed processing: explorations in the microstructure of cognition*. Cambridge: MIT Press.

See also: http://www.rice.edu/langbrain

THE SCOPE OF CORPUS EVIDENCE

Joybrato Mukherjee
University of Bonn

IT IS CERTAINLY TRUE that corpus-based methods have become mainstream over the last years, particularly in English linguistics. Both the compilation of large and representative corpora and the computer-based analysis of such corpora in quantitative and qualitative terms have led to insights into actual language use which could not be obtained from invented and decontextualised examples or intuition-based judgements alone[1]. In the present paper, particular attention will be paid to the corpus-based description of patterns of usage and their relevance to a truly usage-based and cognitive grammar of English. In general, I will suggest that quantitative data and their careful qualitative interpretation be included in future attempts to model speakers' linguistic knowledge. This theoretical issue, referring to the widening scope of corpus evidence, will be illustrated with a sample analysis of corpus data on the frequencies and distributions of related lexicogrammatical patterns of the verb *provide*.

1. PATTERNS AND ROUTINES IN LANGUAGE USE. To begin with, I would like to summarize what corpus-based research has brought to light with regard to authentic language use. Generally speaking, the in-depth analysis of large amounts of data reveals that language use is largely based on routines and patterns. Some twenty years ago, Pawley and Syder (1983:193) pointed to the fact that native-like language use is not only characterised by creativity, but at least to the same extent by routine: 'The problem we are addressing is that native speakers do *not* exercise the creative potential of syntactic rules to anything like their full extent, and that, indeed, if they did so they would not be accepted as exhibiting nativelike control of the language'.

After several decades of research into ever larger corpora, this hypothesis has been confirmed. This not only holds for what Pawley and Syder (1983:192) call 'lexicalized sentence stems', but for many other aspects of language use as well. Collocations such as *vested interest(s)* and *stinking rich* (cf. Partington 1998:26) and collocational frameworks such as *all* + DETERMINER + *time* (cf. Lenk 2000:189–91) show that in actual usage the combination of words is not at all free. Not only do we encounter lexical co-occurrences, but also 'the co-occurrence of grammatical choices' (Sinclair 1996:85), i.e., colligational patterns. For example, *naked eye* tends to be preceded by the definite article, a preposition and a verb or an adjective as in *visible to the naked eye* (cf. Sinclair 1996:85–86). On this basis, Hunston and Francis (2000) suggest a 'pattern grammar' approach to the description of frequent lexicogrammatical co-occurrences of all kinds. For example, *difficult* is shown to occur significantly often in the pattern BE + *difficult* + *for* + NOUN GROUP + *TO*-INFINITIVE (cf. Hunston & Francis 2000:131).

Finally, corpora have shed light on semantic patterns in language use. Stubbs (cf. 1995:26,45), for example, finds out that *provide* tends to be used in positive contexts, whereas *affect* is shown to have a so-called negative 'semantic prosody'. Also, there are semantic restrictions on specified word-forms of a given lexeme. For instance, Esser (2000:97) points out that in the 100-million-word British National Corpus (BNC), the singular form *tree* is attested with both the meaning 'plant' and the meaning 'drawing'. On the other hand, the plural form *trees* is semantically restricted to the meaning 'plant'[2].

In general, such routines and patterns in language use are not found by intuition and introspection, but only by the examination of large amounts of data. Note that all linguistic routines mentioned are derived from frequencies and distributions in text. These linguistic routines are typical of language use, i.e., language in performance. The basic question I would like to address is the following one: what may corpus data on language use tell us about speakers' internalised knowledge of the underlying language system?

2. A CORPUS-BASED MODEL OF SPEAKERS' LINGUISTIC KNOWLEDGE. At first sight—and in generative terminology—the aforementioned research question may seem to be an attempt to relate language in performance to linguistic competence. However, I am deliberately abstaining from using the generative concepts here. A model of the language system that is based on corpus evidence has not much in common with a generative model of competence. Thus, it is not very useful to take over and extend or redefine the term competence, which would automatically lead to terminological confusion (cf. Taylor 1988 passim). Rather, I prefer to speak of a corpus-based model of speakers' linguistic knowledge.

There are, at least, three fundamental differences between the generative approach to competence and a corpus-based model of speakers' linguistic knowledge. First, generative grammar focuses on the knowledge of what is possible in language. The range of what is possible is mainly identified on the basis of intuition-based grammaticality judgements. Second, the focus is on an ideal speaker-hearer. Third, as Chomsky himself has repeatedly pointed out, frequencies in text are considered irrelevant to competence, that is the internalised knowledge of grammar (see the interview B. Aarts (2000:5–7) conducted with Chomsky). On the other hand, a corpus-based model would be based on language used by real speakers in authentic contexts. It takes into account frequencies in text because the model is also intended to mirror speakers' internalised knowledge of what is probable. The knowledge of linguistic routines and patterns includes the ability for use, i.e., the knowledge of principles and factors which are responsible for those routines and patterns. This ability for use corresponds to what Chomsky (1980:224) himself describes as 'pragmatic competence', which he, however, clearly separates from competence proper, namely 'grammatical competence'.

A corpus-based model of speakers' linguistic knowledge would not separate grammatical and pragmatic competence. More importantly, it is an attempt to bridge

the gap between what speakers know and what speakers use. I follow in this regard Halliday's (cf. 1991:31) view that system and use are inseparable, that in fact language use is an instantiation of the language system. A model of speakers' knowledge of that system should therefore account for observable language use as attested in corpora. From a complementary perspective, by carefully observing language use we may catch a glimpse of the language system and speakers' knowledge of that system.

Considering the general outcome of more than thirty years of corpus-linguistic research, it appears to me that a corpus-based model of speakers' linguistic knowledge should be able to account for the following characteristic features of language use as attested in corpora. First, some linguistic forms are more frequent than others in language use, and some formally possible forms are unlikely to occur at all. It is reasonable to assume that speakers know, as it were, about such probabilities of linguistic forms and their combinations. Second, we find linguistic routines and patterns of different kinds (see section 1) so that speakers' linguistic knowledge not only allows for infinite use but is based on routine as well. Third, quantitative data on the frequencies and patterns in text can usually be explained by functional and context-dependent principles and factors. These principles and factors then seem to be part of speakers' linguistic knowledge: language encoders are obviously guided by such principles and factors to make appropriate use of their linguistic means and to adhere to regular expectations in their linguistic behaviour. In my view, this observation should translate into a model which ascribes to whatever is frequent in language use a status that is different from whatever is rarely used. Fourth, lexical and grammatical choices are interdependent in language use. The all-pervading nature of colligations and lexicogrammatical patterns calls into question the autonomy of syntax. A model of speakers' linguistic knowledge that is supposed to account for actual usage should take—as Halliday (1991:31) puts it—'lexicogrammar as a unified phenomenon'.

To summarise, a corpus-based model of speakers' linguistic knowledge would be data-oriented and frequency-based, functionalist and lexicogrammatical in nature. Basing a description of linguistic knowledge on quantitative data obtained from corpora would be a good example of what Kemmer and Barlow (2000:x) call a 'usage-based model': 'This idea of the fundamental importance of frequency… sharply distinguishes usage-based models from other approaches in which frequency is an insignificant artifact, unconnected with speakers' linguistic knowledge'. The foundations of a corpus-based (and, thus, usage-based) model of speakers' linguistic knowledge can also be easily reconciled with Langacker's (1987, 1999, 2000) cognitive grammar approach. In fact, the term 'usage-based model' was first used by Langacker (1987:494) and defined as follows: 'Substantial importance is given to the actual use of the linguistic system and a speaker's knowledge of this use; the grammar is held responsible for a speaker's knowledge of the full range of linguistic conventions, regardless of whether these conventions can be subsumed under more general statements. [It is a] nonreductive approach to linguistic structure that employs fully articulated schematic networks and emphasizes the importance of low-level schemas'. In a similar vein to Halliday (see above), Langacker regards system and use as inseparable

and speakers' linguistic knowledge as a knowledge based on—and derived from—the actual use of the system. It is obvious that corpus evidence can play a major role in such a usage-based cognitive grammar: corpora are samples of 'actual use of the linguistic system'; the 'schematic networks', 'low-level schemas' and 'linguistic conventions' correspond largely to the lexicogrammatical patterns and routines that can be identified by drawing on corpus data.

In the following section, I would like to delve more closely into some of the lexicogrammatical patterns of the verb *provide* in order to illustrate the way in which a corpus-based model of speakers' linguistic knowledge may help bridge the gap between the analysis of actual corpus data and the modelling of language cognition. In particular, I will try to show that the cognitive grammar approach may profit considerably from the consideration of corpus data, thus putting into operation Schmid's (2000:39) 'From-Corpus-to-Cognition Principle: Frequency in text instantiates entrenchment in the cognitive system'.

3. FROM CORPUS TO COGNITION: A SAMPLE ANALYSIS. The verb *provide* is associated with an argument structure the semantics of which, according to Goldberg (1995:49), can be described as 'X CAUSE Y to RECEIVE Z'. I would like to refer to the three corresponding semantic roles as the acting entity (X), the affected entity (Y) and the provided entity (Z) respectively[3]. In this argument structure, the verb *provide* has four, formally different patterns[4]:

- the ditransitive pattern: $V - n_1 - n_2$
- the *with*-pattern: $V - n_1 - with - n_2$
- the *for*-pattern: $V - n_2 - for - n_1$
- the *to*-pattern: $V - n_2 - to - n_1$

It should not go unmentioned that some colleagues (especially American native speakers) have raised objections to my treatment of the *to*-pattern as a lexicogrammatical pattern in its own right. However, as I have argued elsewhere (cf. Mukherjee: in press), the *to*-pattern is a valid pattern which is structurally and semantically analogous to the *for*-pattern. Consider the following two examples, displaying the use of the two patterns in very similar contexts[5]:

(1)   …shall *provide* technical assistance and funds *to* States for training for public safety officials … (FROWN H15 22–23)
(2)   In carrying out the requirements to *provide* technical assistance and funds *for* training, … (FROWN H15 151–152)

In both cases, a computerised parsing on the basis of the TOSCA (*Tools for Syntactic Corpus Analysis*) scheme (cf. van Halteren and Oostdijk 1993:145–62) would result in a direct object realised as a noun phrase and a subsequent adverbial realised as a prepositional phrase (introduced by *to* or *for*). Of course, there are some instances

pattern of *provide*	V-n_1-n_2	V-n_1-*with*-n_2	V-n_2-*for*-n_1	V-n_2-*to*-n_1	Σ tokens of *provide*
LOB	0.0 %	6.0 %	15.1 %	3.0 %	398
FLOB	0.0 %	5.9 %	15.0 %	5.7 %	540
BROWN	0.6 %	6.9 %	16.9 %	4.7 %	508
FROWN	0.7 %	5.9 %	15.9 %	7.5 %	577
BNC	0.0 %	6.1 %	14.8 %	3.8 %	22,312

Table 1. *Relative frequencies of four patterns of* provide *in five corpora.*

of *provide* which superficially resemble the *to*-pattern but in which the prepositional phrase is a postmodification within the noun phrase which, in its entirety, functions as a direct object:

(3)   It provides an excellent *guide to inter-agency co-operation…* (FLOB H09 110)

Such instances of *provide*, which the TOSCA parser would analyse differently, have not been taken into account. Interestingly enough, some British native speakers have objected to the inclusion of the ditransitive pattern (but not of the *to*-pattern). This does not come as too much of a surprise since it has often been suggested that the ditransitive use of *provide* is restricted to American English (cf. e.g., Quirk et al. 1985:1210). In a wider setting, both objections reveal the need for as little human inter-vention as possible in the collection and analysis of data (e.g., by means of automatic parsing) due to the subjectivity and unreliability of intuition.

Four 1-million-word corpora have been searched for the four patterns under discussion: the *Lancaster-Oslo/Bergen Corpus* (LOB) of written British English with texts from 1961, the *Freiburg LOB Corpus* (FLOB) with texts from 1991/92, the *Brown Corpus* (BROWN) of written American English from 1961 and the *Freiburg Brown Cor-pus* (FROWN) with texts from 1991/92. Also, the 100-million-word *British National Corpus* (BNC) of spoken and written British English from the 1990s has been taken into consideration. Table 1 gives the relative frequencies of the four patterns in rela-tion to the total number of occurrences of *provide* in each corpus.

Applying the chi-square test, the relative frequencies of the patterns in the five corpora turn out to be very stable, the differences being statistically insignificant. In particular, there is no significant diachronic change (nor, in fact, a regional variation), which is at odds with the following hypothesis put forward by Hunston and Francis (2000:97): 'Although *provide* is typically used with the pattern V n *with* n ("provide someone with something"), there are a handful of occurrences in the Bank of English of "provide something to someone" (the pattern V n *to* n), presumably by analogy with *give*'. However, it is possible to account for the stable distribution of the patterns at hand from an entirely synchronic and functional point of view. As the ditransitive use
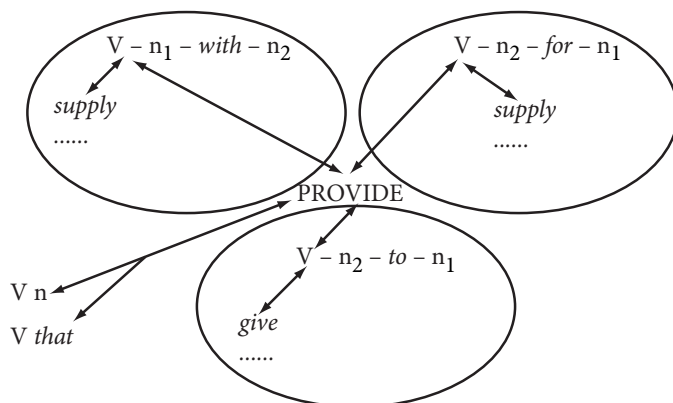
Figure 1. *A constructional and lexical network of* provide.

of *provide* (i.e., without preposition) occurs sporadically in the two American English corpora only, I would like to concentrate on the remaining patterns of *provide*, namely the *with*-pattern, the *for*-pattern and the *to*-pattern.

Drawing on Langacker's (1999) constructional and lexical networks, the lexico-grammar of *provide* can be visualised somewhat simplistically as in Figure 1, which shows that the three constructions under discussion are associated with specific verbs. Also, the arrows indicate that not only do specific verbs choose specific constructions, but that specific constructions select specific verbs in return. Needless to say, *provide* occurs in other patterns as well, as in the pattern 'V n' (where *provide* is followed by a noun group only) and the pattern 'V *that*' (where *provide* is followed by a *that*-clause). Those patterns, however, are left out of consideration for the purpose of this paper. With regard to the occurrence of one and the same lexeme in different constructions, Langacker (2000:35) notes that 'elements are always shaped by the contexts in which they occur'. However, I will not get into details about the interesting fact that all the patterns of *provide* choose a restricted lexis, that is that the constructions may be regarded as carrying an abstract meaning themselves. This important aspect has spawned a vast literature over the last years, particularly in construction grammar with its focus on argument structure (cf. Goldberg 1995 passim).

The point I am making is that the plausibility and scope of any cognitive framework could be increased considerably if information obtained from corpora were included. The following examples are intended to illustrate the different kinds of information that could and should, in my view, be included. Specifically, this corpus-based information makes it possible to describe not only which patterns are available, but also which principles and factors are at work in the procedure of pattern selection.

Consider examples (4) to (6):

(4)   …your study will provide *you* with the knowledge that is generally
        accepted… (BNC EEB 179)

(5)   …providing *teachers* with scientific resources and project materials…
(FLOB H12 216)

(6)   …which is to provide *the nation* with food of the highest quality…
(LOB H10 65)

In more than 90% of all occurrences of the *with*-pattern, the affected entity is animate, as is the case in examples (4) and (5). In example (6), *the nation* can also be regarded as animate since collective nouns are usually subsumed into animate gender classes as well (cf. Quirk et al. 1985:316). This strong association between the *with*-pattern and animate affected entities holds true for all five corpora at hand. No such association can be observed in the *for*-pattern.

What is more, the *with*-pattern and the *for*-pattern differ with regard to the order of elements. In the *with*-pattern, the affected entity precedes the provided entity. In the *for*-pattern this order is reversed. Speakers thus choose one of the two patterns according to the principles of end-weight and/or end-focus. That is to say, if the affected entity represents the heavier constituent, the *for*-pattern is used, as in examples (7) to (9):

(7)   Should the government directly provide education for *the children who want public education*? (BROWN J48 1950)

(8)   The special Hospitals Broadmoor, Rampton and Ashworth Hospitals provide care for *1,700 mentally abnormal people who are judged to…* (BNC FYW 1096)

(9)   The lute also provided the music for *the game of musical chairs they played…* (BNC G3M 963)

If, on the other hand, the provided entity is the heavier constituent, it is the *with*-pattern that tends to be used. Examples (10) to (12) illustrate this correlation:

(10)   It also conveniently provided me with *straight edged divisions of the remaining space*. (BNC CN4 417)

(11)   …the New Age provides seekers with *a spiritual core around which they can orbit…* (FLOB D15 20)

(12)   …and provide the Americans with *bases from which nuclear weapons can be used*. (LOB B23 19)

In a similar vein, speakers may choose between the two patterns in order to place the rhematic information in end-focus position[6]. In examples (13) and (14), the *for*-pattern is used because the affected entity is in focus.

(13)   A white cow used to provide milk for *everyone in the locality…*
(BNC BMT 420)

(14)   To solve the elder-care problem, he would provide "choices" for *old people who still have a lot of money*. (FROWN A14 158–59)

In examples (15) and (16), the *with*-pattern is used because it is the provided entity that is to be focused.

(15)   …the EC must be strengthened to provide the world with *a counterweight to the USA*. (FLOB F17 168)
(16)   …to compensate drivers for any apparent risks in trucking. In addition, it is quite possible that firms provided the drivers with *greater safety resources*… (FROWN J41 104–106)

The third pattern of *provide* to be mentioned is the *to*-pattern. This pattern displays the same order of elements as the *for*-pattern. However, as can be seen in Table 1, the *to*-pattern is used significantly less frequently than the *for*-pattern. This is the case because the *to*-pattern tends to co-occur with a restricted range of provided entities. Consider examples (17) to (19):

(17)   …thus providing a more effective *challenge to* independent services. (FLOB G76 196)
(18)   …it provides the only realistic *solution to* the problems of race relations… (LOB D17 84)
(19)   …governments are able to provide local *subsidy to* local firms or individuals… (FROWN H05 153)

The corpus analysis shows that the *to*-pattern occurs with nouns (as provided entities) which are usually followed by the preposition *to*. That is to say, these nouns—such as *challenge*, *solution* and *subsidy*—have a pattern themselves which could be described as 'N *to* n'. The list of some nouns with this pattern given in Table 2 is based on the pattern information indicated in the *Collins COBUILD English Dictionary*.

To sum up, the analysis of large amounts of data makes it possible to explain in functional terms the choice of one of the three patterns of *provide* under discussion. In the light of the corpus data, the following principles and factors turn out to be relevant: the principles of end-weight and end-focus in general and lexicosemantic restrictions on the *with*-pattern and the *to*-pattern in particular. Note also that corpus evidence may help explain why the *for*-pattern is more frequent than the other patterns in all corpora: it is neither restricted in terms of the animacy of the affected entity nor in terms of the noun in n_2-position and the preposition that usually follows it. The *for*-pattern is therefore very flexible and can be used with virtually all affected and provided entities. It is, as it were, the default case of pattern selection. Generally speaking, the aforementioned principles and factors derived from corpus evidence appear to be part of speakers' linguistic knowledge since they lead language users to prefer a specific pattern to others in given contexts.

pattern of *provide*: V - n_2 - *to* - n_1	examples of nouns occurring in n_2-position
nouns in n_2-position generally occur in the pattern 'N *to* n'	aid, assistance, answer, boost, care, challenge, contribution, grant, help, impetus, incentive, information, input, protection, sanctuary, service, solace, solution, stimulus, subsidy, support, treatment, value

Table 2. *The restricted lexis in the n_2-position of the to-pattern of* provide.
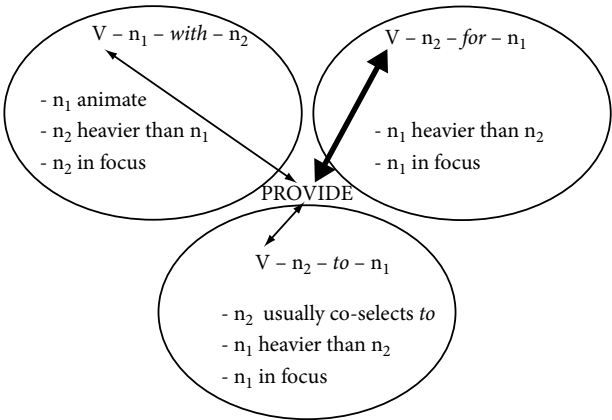
Figure 2. *Including corpus evidence in the constructional and lexical network of* provide.

Thus, corpus evidence should be included in the constructional and lexical network given in Figure 1. Figure 2 offers a refined version of Figure 1 in that it includes the preference for the *for*-pattern in general and the principles and factors which are relevant to the actual use and choice of each pattern in particular. The graphic visualisation approximates to a usage-based model in which the framework of cognitive grammar is complemented with corpus data.

4. SOME CONCLUDING REMARKS. In the present paper, I hope to have shown that a cognitive grammar of speakers' linguistic knowledge can be fruitfully combined with corpus evidence, resulting in a model that is much more usage-based and, thus, much more plausible. In the from-corpus-to-cognition approach as described and exemplified above, the scope of corpus evidence is about to widen. In my view, corpus data not only tell us important things about language use, but also about the underlying language system as represented in speakers' linguistic knowledge.

Secondly, it goes without saying that no corpus—however large it may be—will ever cover all the structural possibilities in lexicogrammar. It is here that intuitive data such as grammaticality and acceptability judgements of native speakers will continue to play a major role in any usage-based model of speakers' linguistic knowledge. As J. Aarts

(1991:46–52) and others have noted, the observation of corpus data on the one hand and intuition-based methods on the other are not mutually exclusive: corpora show us probabilities in a language whereas intuition may tell us more about what is possible in a language. The most comprehensive picture of language, covering linguistic routine as well as creativity, might indeed be achieved by using both corpora and intutitive data. As things stand, any plausible model of speakers' linguistic knowledge should address both questions involved: (1) what is structurally possible; (2) what is likely to occur and why? Future research into the linguistic system and its cognitive basis should thus take into account corpus data to a much larger extent than in the past.

---

[1]   In the present paper, the term 'corpus' is exclusively used for 'a collection of texts assumed to be representative of a given language, dialect, or other subset of a language, to be used for linguistic analysis' (Francis 1982:7). In my view, one should abstain from regarding any collection of texts (let alone decontextualised examples) as a corpus (as has recently come into vogue). The linguistic corpus is both large and, as a statistically reliable sample, representative of more than it actually is. In fact, it is this representativeness in corpus design which makes it possible to extrapolate general trends in language use from corpus findings.

[2]   It goes without saying that other kinds of patterns of usage have been described as well. In particular, Biber's (1988) corpus-based description of different linguistic preferences in different genres and his concept of linguistically defined text-types, cutting across traditionally established and intuitively defined genres, should not go unmentioned.

[3]   In the present paper, I will discuss the corpus data only summarily since my focus is on the scope of corpus evidence from a theoretical point of view. Both a detailed statistical analysis and an elaborated functional interpretation of the data are available elsewhere (cf. Mukherjee: in press).

[4]   In the patterns, 'V' stands for the verb, 'n_1' for the affected entity and 'n_2' for the provided entity. Note that I am not concerned with the passive equivalents of the patterns because the optionality of the *by*-agent (and, in fact, its frequent omission) requires a much more detailed discussion of the relevant principles and genre-specific preferences, which is beyond the scope of this paper.

[5]   As a matter of fact, example (2) is a good candidate for multiple analysis. Specifically, one could argue that *training* in example (2) is not n_1, but a modification within n_2 (*assistance and funds for training*). The same objection would hold true for *solution to the problems* in example (18). This view, however, is clearly based on a particular (but, I think, not the only possible) analysis of the hierachical relations between the constituents at hand and could thus be visualised by way of, say, bracketing (e.g. by representing the structure of example (2) as V - [n_2 - [*for* - n_1]]). Conversely, my analysis in terms of patterns has to be seen within the framework of the pattern grammar approach, which is intended to make do without any hierarchical information, i.e. without a genuinely structural analysis (cf. Hunston and Francis 2000:152). The chief rationale behind this solely pattern-based analysis, mainly inspired by Brazil's (1995) *Grammar of Speech*, is the view that a structural analysis is only possible once the sentence as a product is finished, whereas the pattern grammar is an account of speakers' online production of sentences as a process of 'prospections'. Thus, it does not come as too much a surprise that clauses which might be analysed differently when it comes to hierarchical constituent structure, as in examples (1)

and (2), are subsumed into the same pattern by advocates of pattern grammar, to which I also subscribe. I would like to thank Ruth Brend for a discussion of this theoretical issue in general and for pointing out to me some potential problems involved in a purely pattern-based analysis in particular.

6   While the heaviness of a constituent is a clause-internal (and, thus, easily accessible) feature, it is quite clear that the givenness and newness of constituents can only be described by referring to the context. It is here, that is in exploring 'language as function in context' (Tognini-Bonelli 2001:4), that the analysis of corpora has a great advantage over the use of decontextualised (and/or invented) sentences and artificial laboratory data.

REFERENCES

AARTS, BAS. 2000. Corpus linguistics, Chomsky and Fuzzy Tree Fragments. In Mair & Hundt, 5–13.

AARTS, JAN. 1991. Intuition-based and observation-based grammars. In Aijmer & Altenberg, 44–62.

AIJMER, KARIN & BENGT ALTENBERG (eds). 1991. *English corpus linguistics: studies in honour of Jan Svartvik*. Harlow: Longman.

BARLOW, MICHAEL & SUZANNE KEMMER (eds). 2000. *Usage-based models of language*. Stanford: CSLI Publications.

BIBER, DOUGLAS. 1988. *Variation across speech and writing*. Cambridge: Cambridge University Press.

BRAZIL, DAVID. 1995. *A grammar of speech*. Oxford: Oxford University Press.

CHOMSKY, NOAM. 1980. *Rules and representations*. Oxford: Blackwell.

ESSER, JÜRGEN. 2000. Corpus linguistics and the linguistic sign. In Mair & Hundt, 91–101.

FRANCIS, W. NELSON. 1982. Problems of assembling and computerizing large corpora. In *Computer corpora in English language research*, ed. by Stig Johansson, 7–24. Bergen: Norwegian Computing Centre for the Humanities.

GOLDBERG, ADELE E. 1995. *Constructions: a construction grammar approach to argument structure* (*Cognitive theory of language and culture*). Chicago: The University of Chicago Press.

HALLIDAY, M.A.K. 1991. Corpus studies and probabilistic grammar. In Aijmer & Altenberg, 30–43.

HUNSTON, SUSAN & GILL FRANCIS. 2000. *Pattern grammar: a corpus-driven approach to the lexical grammar of English* (*Studies in corpus linguistics 4*). Amsterdam: John Benjamins.

KEMMER, SUZANNE & MICHAEL BARLOW. 2000. Introduction: A usage-based conception of language. In Barlow & Kemmer, vii–xxviii.

LANGACKER, RONALD W. 1987. *Foundations of cognitive grammar*, vol. 1, *Theoretical prerequisites*. Stanford: Stanford University Press.

——. 1999. *Grammar and conceptualization* (*Cognitive linguistics research 4*). Berlin: Mouton de Gruyter.

————. 2000. A dynamic usage-based model. In Barlow & Kemmer, 1–63.

LENK, UTA. 2000. Stabilized expressions in spoken discourse: worth our time? In Mair & Hundt, 187-200.

CHRISTIAN MAIR & MARIANNE HUNDT (eds). 1999. *Corpus linguistics and linguistic theory: papers from the twentieth international conference on English language research on computerized corpora (*ICAME 20*) – Freiburg im Breisgau 1999 (Language and computers: studies in practical linguistics 33)*. Amsterdam: Rodopi.

MUKHERJEE, JOYBRATO. In press. Principles of pattern selection: a corpus-based case study. *Journal of English linguistics* 29.

PARTINGTON, ALAN. 1998. *Patterns and meanings: using corpora for English language research and teaching (Studies in corpus linguistics 2)*. Amsterdam: John Benjamins.

PAWLEY, ANDREW & FRANCES HODGETTS SYDER. 1983. Two puzzles for linguistic theory: native-like selection and native-like fluency. In *Language and communication (Applied linguistics and language teaching)*, ed. by Jack C. Richards & Richard W. Schmidt, 191–226. London: Longman.

QUIRK, RANDOLPH, SIDNEY GREENBAUM, GEOFFREY LEECH & JAN SVARTVIK. 1985. *A comprehensive grammar of the English language*. London: Longman.

SCHMID, HANS-JÖRG. 2000. *English abstract nouns as conceptual shells: from corpus to cognition (Topics in English linguistics 34)*. Berlin: Mouton de Gruyter.

SINCLAIR, JOHN. 1996. The search for units of meaning. *Textus* 9:75–106.

STUBBS, MICHAEL. 1995. Collocations and semantic profile: on the cause of the trouble with quantitative studies. *Functions of language* 2:23–55.

TAYLOR, DAVID S. 1988. The meaning and use of the term 'competence' in linguistics and applied linguistics. *Applied linguistics* 9:148–68.

TOGNINI-BONELLI, ELENA. 2001. *Corpus linguistics at work (Studies in corpus linguistics 6)*. Amsterdam: John Benjamins.

VAN HALTEREN, HANS & NELLEKE OOSTDIJK. 1993. Towards a syntactic database: the TOSCA analysis system. In *English language corpora: design, analysis and exploitation (Language and computers: studies in practical linguistics 10)*, ed. by Jan Aarts, Pieter de Haan & Nelleke Oostdijk, 145–62. Amsterdam: Rodopi.

∽

MEASURING UP TO EXPECTATIONS: WHAT CONSTITUTES EVIDENCE IN CHILD LANGUAGE RESEARCH

Suzanne Quay
International Christian University, Tokyo

Child language research has come a long way since the early twentieth century, when researchers interested in child language development conducted longitudinal case studies using diary records (for example, the classic studies by Ronjat 1913 and Leopold 1939). When Bennett-Kastor (1985/86) examined 154 studies published between 1970 and 1985 in the area of child language development, she discovered a division in methodological practices between linguists and those trained in the humanities, and psychologists and those trained in the social sciences. In the studies she examined, linguists had a greater preference for naturalistic data based on a small number of subjects and moreover, tended to investigate production. Psychologists had a greater preference for experimental data involving large numbers of subjects and were more likely to investigate comprehension or response. Linguists were found to use audiotaping more often than did psychologists with videotaping, diary records and other stimulus materials used less often than expected. Both groups favored studying children aged between three and five with linguists showing a slight shift towards using subjects aged between birth and two years, and psychologists showing a slight shift towards using subjects over the age of ten.

Child language research was thus described by Bennett-Kastor (1985/86) as a disunified field, more so at the level of data and method than in theory, because of the different backgrounds of its practitioners. Nevertheless, the field has changed with joint efforts from linguists and psychologists. Work on the Child Language Data Exchange System or CHILDES (MacWhinney 1995), particularly in the last decade, has provided methodological guidance and tools for child language research with a database of transcripts for researchers worldwide, and programs for the computer analysis of transcripts. Advances in technology have also played a role in changing child language research. Digital recordings and wireless transmissions can now be used—a far cry from note-taking done by linguists and/or parents while listening to children. The question of interest here is how effective past and present methods are in measuring early child language development.

1. A STUDY WITH SEVERAL METHODS OF DATA COLLECTION. This paper presents separate analyses of data collected using different methods from a longitudinal case study[1] that, considered separately, would create a misleading picture of the child's actual linguistic behavior but which are more informative when considered together. The results indicate, as we will see, that the use of composite, rather than individual measurement

Method	Frequency of collection	Information gathered	Summary of results in percentages
Question-naires	• completed by parent(s) at ages 1;0, 1;5 and 1;9	• family background • amount of exposure to each language	Amount of language input from 0;11 to 1;9—heard average % of time: *English* **46%** *Japanese* **36%** *German* **18%**
Interviews	• conducted at the home and in the daycare at ages 1;0, 1;5 and 1;9	• child's general behavior • understanding and use of particular words and gestures	
Diaries	• mother: each month for words understood *&* every 2 weeks for words produced • daycare staff: every day Freddy was in atten-dance	• new words under-stood and produced by child in the home and at the daycare	**Comprehension** up to 1;4: *English* **63%** *German* **35%** *Japanese* **2%**
MCDI	• Infant *&* Toddler Short Forms in English *&* German: every 3–4 weeks • full Japanese MCDI at ages 1;0, 1;5, and 1;9	• common/typical words understood and produced by Eng-lish-, German- and Japanese-speaking children	**Production** up to 1;10: *English* **57%** *Japanese* **34%** *German* **9%**
Video recordings	• each week in the home: once in an English context and once in a German one • each week in the Japa-nese daycare	• actual linguistic pro-duction in three sepa-rate language contexts	Language choice in lexical production—Conflated across the two language contexts: *Japanese* **78%** *German* **14%** *English* **8%**

Table 1. *Freddy's language development according to each method used.*

formats is a desideratum in any child language acquisition study. The child in question, a male infant aged between 0;11 and 1;10 (year;month.day), was too young to be studied using testing and experimental methods favored by psychologists. Thus only *naturalistic* observation methods were used and their effectiveness discussed as we seek to answer whether each method and the results measure up to our expectations.

2. METHOD.

2.1. THE TRILINGUAL FAMILY. The subject of this study, Freddy, was born on April 24th, 1997 in Tokyo, Japan to a German father and an American mother. From birth, Freddy heard German from his father, a landscape architect with a Masters in Engineering, and English from his mother, a university professor with a doctorate in Sociology. Freddy's parents spoke primarily German to each other and to the child when all three were alone together. Both parents were also competent in speaking Japanese, the main language of the local community. As appropriate, they would speak Japanese with Japanese interlocutors and English with English ones.

2.2. THE DAYCARE ENVIRONMENT. Freddy was the only non-Japanese child at the daycare that he attended from age 0;11 onwards for six hours each weekday. The eight other children and all the daycare staff were monolingual Japanese speakers. Thus Freddy was exposed to Japanese mainly at the daycare and to English and German mainly in the home.

2.3. DATA COLLECTION. Data were collected in the home and at the daycare through questionnaires, interviews, diary records, MacArthur Development Inventories (hereafter, MCDI), and video recordings as listed in the first column of Table 1. The frequency of data collection and the information gathered from each method are also summarized in the second and third columns respectively of Table 1.

The parental *questionnaire* was administered when the child was aged 1;0, 1;5 and 1;9 to gather information about the social background of the family as already described and the changing language exposure patterns for the child. Additional questions about the child's general behavior, understanding and use of particular words and gestures, knowledge of specific games, preferences for certain toys, and whether imaginative play occurred were asked in the *interviews* conducted with the parent(s) and daycare staff also at ages 1;0, 1;5 and 1;9.

The mother and daycare staff were asked to keep a *diary* of words Freddy understood as well as words produced. The mother's diary noted her son's production of lexical items in English, German and Japanese from ages 0;10.25 to 1;10.1. For the words her child understood, she listed them in her diary roughly once a month. The words he produced were listed on average every two weeks. Members of the daycare staff noted Freddy's linguistic progress in a daily diary along with their usual entries about the child's behavior and activities, which they provided daily for every child to inform parents about their child's day.

The *MCDI*, a type of parental report, was also used to assess Freddy's understanding or understanding with production of common vocabulary items. Every three to four weeks, the mother completed the English version of the MCDI—the infant short form from ages 1;0 to 1;4 and the toddler short form from ages 1;4 to 1;9 (details about the short form versions of the MCDI are in Fenson, Pethick and Cox 1994). The mother, without being asked, also indicated on the form when her son understood English words but produced the words understood in English in either German or Japanese

(the MCDI was of course designed to assess monolingual children and does not take into account the language variations that may occur in terms of understanding with production in multilingual children). Because a German version of the MCDI was not available, the German-speaking father was asked to consider, also every three to four weeks, whether the items on the English version of the parental report were applicable in a German language context (basically, translate the English items into German and report on whether the child understood or produced these words in German). The Japanese version was a full MCDI form adapted into Japanese by Ogura and her colleagues at Kobe University (Ogura 1998; Ogura, Yamashita and Murase 1998). The Japanese MCDI was used to interview various members of the daycare staff (more than one staff member and different volunteer caregivers were present each day) when Freddy was aged 1;0, 1;5 and 1;9 to obtain an idea of Freddy's receptive and expressive vocabulary in that language environment. It was not possible to ask any one daycare staff member to complete the checklist on a more regular basis than during the interviews.

Video recordings were made every week (when possible) in the home, once with the mother addressing the child in English (considered to be an English language context situation) and once with the father addressing the child in German (considered to be a German language context situation). Video recordings were also made each week in the daycare, a Japanese language environment. A Sony Handycam Hi8 Video Camera Recorder (CCD-TRV85 NTSC) was used in all three separate language contexts. One video camera was left with the parents to use on a tripod, as this created an environment whereby the child could in fact draw specifically on his knowledge from one or the other of his two home languages in interactions with either his English-speaking mother or with his German-speaking father without interference from a camera operator who could possibly affect the child's language choice. A second video camera was used by the Japanese-speaking research assistant who was sent into the daycare each week to videotape Freddy playing at the daycare or at different parks near the daycare. Altogether, thirty sessions were recorded in the Japanese daycare, twenty-nine sessions with the mother speaking English and eighteen sessions with the father speaking German.

2.4. TRANSCRIPTION AND CODING. Full transcripts in the CHAT format of CHILDES (MacWhinney 1995; see also Sokolov and Snow 1993) are still being made for thirty minutes of video-recorded sessions in the German, English, and Japanese language *contexts* (please note that all reference to contexts is based only on the main language used by the child's interlocutors and does not imply that the infant is aware of having the three languages). Due to the on-going nature of this time-consuming task, this paper reports on a small part of the data, specifically on six pairs of video recordings made in the home. His mother addressed him in English in six English context sessions and his father addressed him in German in six German context sessions, spaced at approximately six- to eight-week intervals between ages 1;1 and 1;10. Further details about these recordings can be found in Quay (2001)[2].

In order to investigate the child's language choice patterns, it was necessary to code his utterances as English, German, or Japanese. The identification of Freddy's early utterances was not an easy task. The phonetic transcriptions of the child's utterances in PHONASCII (MacWhinney 1995: 82–85) along with information on the child's actions and the adult's responses in the transcripts aided in the coding of utterances. In most cases, coding relied on the phonetic resemblance of the utterance to an adult source word in English, German or Japanese, but phonetically simple versions of adult source words were also accepted. For utterances that clearly resembled adult source words, the child's reference or use also had to be similar or match the adult's or the child's own use in other situations. Coding was made easier when previous occurrences of the particular utterance had already been noted in other sources (diaries, MCDIs, interviews), thus providing further evidence of the child's consistent use of a word for a particular meaning. Any utterances that were ambiguous between any two or all three languages were excluded from further analyses.

3. RESULTS. The results reported here have been simplified into percentages, as shown in the last column of Table 1, for the main purpose of comparing the results with the methods used. More detailed results are presented in figures and charts in Quay (2001).

3.1. LANGUAGE EXPOSURE AS EXTRAPOLATED FROM THE QUESTIONNAIRE. The questionnaire filled out by Freddy's mother provides the information about his language exposure. Freddy spent his first twenty-two months of life in Tokyo, with a two-week visit to his relatives in Germany when he was only two and a half months old. During a six-week period in the summer of his second year (between ages 1;2.24 and 1;4.4), the family spent four weeks in the United States and two weeks in Germany.

The 'questionnaire' row of the last column of Table 1 shows that on the average between ages 0;11 to 1;9, Freddy heard English 46% of the time, Japanese 36% of the time and German only 18% of the time. Freddy's exposure to German is the lowest due to the fact that his father had a busy work schedule and was absent for part of the time when Freddy was aged between 1;5 and 1;9.

3.2. RESULTS FROM PARENTAL AND DAYCARE REPORTS IN INTERVIEWS, DIARIES AND MCDI. The data from the MCDI, the diaries and the interviews were combined (as indicated by the shading for these three data sources on Table 1) to determine the composition of Freddy's early lexicon in terms of comprehension up to age 1;4 and production up to age 1;9 in three languages.

In terms of vocabulary comprehension, Freddy appears to understand more English words than German or Japanese ones from ages 1;0 to 1;4. By age 1;4, 63% (N=52) of the words Freddy could understand were English, 35% (N=29) were German words and only 2% (N=2) were Japanese ones (cf. the 'diaries' row of the last column of Table 1).

In terms of words Freddy could produce up to age 1;9 (based again on the MCDI, diaries and interviews), 57% (N=50) of them were English words, 34% (N=30) were

Japanese words and 9% (N=8) were German words (cf. the 'MCDI' row of the last column of Table 1). At age 1;9, he had a total of 88 different vocabulary types in his lexicon from three language sources.

3.3 RESULTS FROM VIDEO RECORDINGS. The most unexpected results occurred in the video recordings when Freddy produced mainly Japanese utterances in both the English- and the German-language contexts. Based on the data obtained from the MCDI, diaries and interviews, we would have expected Freddy to produce more English words than German or Japanese ones. Contrary to our expectations, in both language contexts, Freddy consistently produces more Japanese tokens.

The data are conflated across the two language contexts, as shown in the 'video recordings' row of the last column of Table 1. Of all the data that could be identified as having a language-specific source, 78% (N=209) are Japanese utterances (in terms of tokens). German was produced slightly more often than English as such utterances made up 14% (N=37) of his total language-identifiable production. Only 8% (N=21) are English utterances, in spite of the fact that the MCDI and diaries report that Freddy produced the most English utterances by age 1;9.

4. DISCUSSION. The results from each individual method tell a different story about Freddy's language development when viewed separately. According to parental and daycare reports in the form of interviews, diaries and MCDI, Freddy is the most proficient in English for both comprehension and production. As for his other two languages, the comprehension data suggest he understands more German than Japanese at least up to age 1;4 but the production data up to age 1;9 show that he demonstrates more Japanese spoken ability than German. The most surprising results come from the video recordings with evidence indicating that Freddy speaks mainly Japanese even when his mother speaks to him in English and his father speaks to him in German. Elsewhere in Quay (2001), Freddy's overwhelming use of Japanese in the video recordings has been explained as being due in part to having accommodating trilingual parents, in part to strong peer and community influence, and in part to personality and sociopsychological factors. Given these results, what can we say are the limitations and strengths of the methods used.

The questionnaire, interviews, and diaries are different forms of parental reports that have been criticized for their reliability and objectivity. It is often felt that parental reports are selective, may not provide enough details to reflect changes in development or may highlight idiosyncratic forms (cf. Bennett-Kastor 1988: 60–61; a discussion of disadvantages and limitations of parental reports can also be found in Berglund 1999). Some of the problems with such parental reports have been dealt with by the development of the MCDI, which, while also being a parental report, uses standardized vocabulary checklists so that parents are relying on their *recognition memory* rather than their *recall memory* when they report on children's present rather than past behavior. Since the instruments are checklists that focus on children's

current communicative skills (Bates, Bretherton, and Snyder 1988; Fenson, Pethick, and Cox 1994), they are felt to increase the validity of parental reports.

Besides the added advantage of instant playback, video recordings are considered to be more neutral and give information that the MCDI cannot provide on pragmatic skills, parental communication, and the nature of interaction and non-verbal communication beyond simple gestures. Video recordings are considered to leave less room than parental reports for over-generalization, error and bias. Genesee (1989) has warned that parental information cannot be totally reliable when it concerns parents' own speech. Research by Goodz (1989) and Kasuya (1998) on bilingual families have found that parents claiming to use only the 'one parent-one language' approach or to use mainly one language did not model such speech, thus showing a discrepancy between reported language use and actual production. This, of course, does not preclude parents as the best sources for estimates about their children's early exposure patterns and for descriptions of sociolinguistic background (De Houwer 1995:224–25 and Kasuya 1998: 331 also defend the usefulness of certain types of parental information).

Video recordings, however, also have limitations, as they provide a particular sampling that may not be typical of language production during the rest of a day when no recording is made. The activities recorded in the home where useful language samples could be obtained were of the child playing with his toys or looking at books. Recordings have not been made, for instance, during the daily diaper-changing event, but his mother reported in the diary that the English word, *down*, was produced at age 1;5.15 when Freddy wanted to be lifted down from his changing table. Similarly, *up-down* was used at age 1;8.13 in the morning when he wanted his mother to get up and take him downstairs. Such utterances never appear in the video recordings because they are not appropriate to the situations or activities being recorded. From the English version of the MCDI infant short form completed for Freddy at age 1;3 by the mother, we find that he understands the item *night night*, but actually says 'ne ne' (*nenne* is the Japanese baby word for 'sleep'). He understands the items *finish* and *all gone* on the MCDI but actually produces 'all done' for both concepts. None of these items appears on the video recordings analyzed because they are not needed during the play activities recorded. However, these examples from the diary and MCDI indicate that parental reports can complement the particular sampling limitations of video recordings, especially for constructing a lexicon of the child's vocabulary in three languages.

5. CONCLUSIONS. Different methods in combination contribute to the strengths of this case study. Evidence from parental and daycare reports in interviews, diaries and MCDI serve as an important back-up system to video recordings for three overlapping reasons:

(1) to balance subjective and objective elements,
(2) to supply more comprehensive detail, and
(3) to identify multiple contexts.

In terms of (1), while videotaping is considered to be more objective than parental reports, a full understanding of the child's abilities cannot be captured on video, as video recordings tend to be limited in frequency, duration and longevity. In other words, taping does not usually occur seven days a week, during the child's every waking moment and for indefinite periods. In this study, the mother and father observed the child at home while daycare staff observed the child in the daycare. The researcher visited both the home and the daycare settings and in the latter, research assistants were also present, making notes about the child's linguistic behavior. Although observers are felt to be less objective than mechanical recording equipment, having many observers, as in this study, increases *consensual validity*. In terms of (2), the researcher can be hindered by production data, which are limited only to what the subject happens to say during the sampling period and its situation. It would be difficult to examine the child's full production capabilities, for example in terms of his lexicon, using only video data that record one type of event, such as play sessions between the child and his parents. In terms of (3), contexts are deemed to be the elements that either directly or indirectly affect the development of the child's language as in different physical settings, behavioral and linguistic environments, and interactional variables. Using video recordings alone tends to show only the particular context or situation filmed and not other segments of the child's daily life.

In spite of the fact that the results from the video recordings differ from those obtained from the MCDI, diaries, and interviews, the systematic analyses of data collected through a combination of methods can allow reliable inferences about the child's language knowledge to be made. Ideally, converging evidence across multiple methods would be the most powerful approach to hypothesis testing corroboration regarding what does and does not occur in the linguistic competence of a child. When converging evidence is not available across multiple methods, at least new issues can be raised and researchers have a more comprehensive database from which to draw inferences and explain anomalous results. Caution is thus advised in the interpretation of results in studies that depend on just one method to measure children's communicative abilities.

---

[2]   This paper refers to further details in Quay (2001) but differs from that article as the summary and presentation of results in terms of percentages as shown in Table 1 have not appeared elsewhere before. This paper, unlike Quay (2001) which describes the role of input in early trilingual development, focuses on the overall results from each method for the express purpose of showing that individual measures of child language use and comprehension are misleading unless put in relation to one another.

REFERENCES

BATES, ELIZABETH, INGE BRETHERTON & LYNN SNYDER. 1988. *From first words to grammar*. New York: Cambridge University Press.

BENNETT-KASTOR, TINA. 1985/86. The two fields of child language research. *First language* 6(3):161–74.

———. 1988. *Analyzing children's language: methods and theories.* Oxford: Basil Blackwell Ltd.

BERGLUND, EVA. 1999. *Early communicative and language development in Swedish children: methods, results, clinical implications, and prospects for the future.* Edsbruk: Akademitryck AB.

DE HOUWER, ANNICK. 1995. Bilingual language acquisition. In *The handbook of child language*, ed. by Paul Fletcher & Brian MacWhinney, 219–50. Oxford: Blackwell.

FENSON, LARRY, STEVE PETHICK & JEFFREY L. COX. 1994. *The MacArthur Communicative Development Inventories: short form versions*. Unpublished manuscript. San Diego CA: Department of Psychology, San Diego State University.

GENESEE, FRED. 1989. Early bilingual development: One language or two? *Journal of child language* 16:161–79.

GOODZ, NAOMI S. 1989. Parental language mixing in bilingual families. *Infant mental health journal* 10(1):25–44.

KASUYA, HIROKO. 1998. Determinants of language choice in bilingual children: The role of input. *International journal of bilingualism* 2(3):327–46.

LEOPOLD, WERNER F. 1939. *Speech development of a bilingual child: a linguist's record. Vocabulary growth in the first two years.* (vol. 1). Evanston, IL: Northwestern University Press.

MACWHINNEY, BRIAN. 1995. *The CHILDES project: tools for analyzing talk* (2nd ed.). Hillsdale, NJ: Lawrence Erlbaum Associates, Publishers.

OGURA, TAMIKO. 1998. *Makkaasaa nyuuyouji gengo hattatsu shitsumonshi no kaihatsu to kenkyuu (The development and study of the MacArthur Communicative Development Inventories)*. Kobe: Kobe University Press.

———, YUKIE YAMASHITA & TOSHIKI MURASE. 1998. Shokigengo hattatsu inbentori no datousei oyobi goi chekkurisuto no kentoo (The validity and the analysis of vocabulary checklist in the first version of the Japanese early communicative development inventory). *Kobe daigaku hattatsu kagaku kenkyukiyou (Kobe university developmental science bulletin)* 5(2):261–76.

QUAY, SUZANNE. 2001. Managing linguistic boundaries in early trilingual development. In *Trends in bilingual acquisition*, ed. by Jasone Cenoz & Fred Genesee, 149–99. Amsterdam: John Benjamins.

RONJAT, JULES. 1913. *Le développement du langage observé chez un enfant bilingue*. Paris: Librairie Ancienne H. Champion.

SOKOLOV, JEFFREY L. & CATHERINE E. SNOW (eds.). 1993. *Handbook of research in language using CHILDES*. Hillsdale NJ: Lawrence Erlbaum Associates.

A PLAUSIBLE CONTRADICTION

WILLIAM J. SULLIVAN
Uniwersytet Wrocławski and University of Florida

OUR CONFERENCE THEME, what constitutes linguistic data, has an implicit codicil: (data) from which a description may be built. Phonetic data do not suffice for a sociolinguistic study, and detailed social parameters are irrelevant to the phonetic phenomenon of, say, voicing. Yet under the proper circumstances, both types of data fit Yngve's (1996 and elsewhere) repeated insistence that a scientific linguistics focus on things given in advance. Such objectively measurable properties of communicating human beings, unlike unsupported native speaker intuition, are the only things we can depend on. Still, recorded observations must be interpreted and arranged with logical coherence. Kopernik and Ptolemy depended on essentially the same observations. The difference in their descriptions, however, results from a difference in a (then non-empirical) postulate, namely in whether the solar system is helio- or geocentric. So while Yngve's rejection of grammaticality judgements as data is well-founded, I am equally convinced that reliance on hard data does not guarantee the same description[1].

A perfect example of two divergent descriptions of hard phonetic data exists in the case of voice assimilation in obstruent clusters in contemporary standard Russian (CSR). Two phonetic facts have long been recognized: CSR obstruent clusters are entirely voiced or entirely unvoiced and the voice nature of the final obstruent determines the voice nature of the entire cluster. These two observations are accepted in all descriptions of CSR obstruent phonology in syllable onset position[2]. The problem is that two radically different descriptions have emerged. One, represented by Jakobson, Cherry and Halle 1953 (hereafter JCH) treats voiced obstruents as marked[3]. The other, represented by Lamb 1966 and 1977, treats unvoiced obstruents as marked. The postulates are apparently contradictory. Yet both descriptions seem to account for the phonetic facts of onset obstruent clusters in CSR. It would be hard to criticize Yngve if he identified this as the result of introducing a philosophical concept (markedness) into a scientific enterprise[4].

Yet I am convinced that something else is going on. In fact, no complete description of the system that underlies both phonetic and neurological facts exists. Therefore, I give a representative sample of the phonetic facts and a comparison of the two descriptions. I tell why each approach is plausible and show the locus of the contradiction. By providing a description of the logic underlying the phonetic facts of onset clusters, I show that there is no logical contradiction and that both Jakobson and Lamb are correct.

/t/ vs. /d/[5]		[d] only		[t] only	
o*t*obrať	'select, prf'	o*t*birať	'___, impf'		
po*d*obrať	'choose, prf'	po*d*birať	'___, impf'		
o*t*ojti	'withdraw, prf'			o*t*xodiť	'___, impf'
po*d*ojti	'approach, prf'			po*d*xodiť	'___, impf'
ko*t*a	'cat, AGsg'			ko*t*	'___, Nsg'
ko*d*a	'code, Gsg'			ko*d*	'___, Nsg'

Table 1. Voice with obstruents in CSR.

1. PHONETIC FACTS AND PHONOLOGICAL DESCRIPTIONS. Scientific generalizations (and hence, descriptions) are inductive, Bloomfield said, and he was right. The foundation is observed fact, interpreted as general statements on which formalized descriptions are based[6]. I begin with the forms in Table 1, focusing on the elements represented by *t* and *d* in bold italics.

Now the facts concerning these forms are undisputed: column 1 has [t] and [d], which realize phonemes /t/ and /d/, respectively. Column 2 has [db] clusters. Column 3 shows [tx] and word-final [t]. I henceforth ignore word-final [t].

Contemporary phonological descriptions all owe a debt to Trubetzkoy 1939. Trubetzkoy describes the system underlying Table 1 as follows: [t] and [d] in column 1 exhibit a (characteristic) privative contrast for voice; columns 2 and 3 show archiphonemes of neutralization, with voiced D as the unmarked variant in column 2 and unvoiced T as the unmarked variant in column 3.

JCH basically agrees with Trubetzkoy's analysis, but they use the feature ± voice and treat [-vce] as the unmarked variant everywhere. In their description the marked [+vce] is supplied in column 2, the unmarked [-vce] appears in all column 3 environments. Halle 1959 follows JCH but eliminates contrast and supplies the [+vce] or [-vce] in columns 2 and 3 by an alpha-switching rule. Chomskyan phonologists have followed his approach since then. Sullivan 1974 is a relational network (RN) description. It parallels JCH 1953 without the [-vce] feature. The symbol 'Y' represents phonemic voice (cf. Table 2A).

Contrary to all of these are Lamb 1966 and 1977. Lamb's approach is also a RN one. As such, it parallels Sullivan 1974, except that unvoiced obstruents are treated as marked. The symbol 'h' is used to represent phonemic unvoicing (cf. Table 2B). I use Sullivan 1974 as the representative of the Jakobson approach herein. Because both Sullivan and Lamb use RNs they are directly comparable. The two descriptions are summarized in Table 2.

Line 1 represents the input from the morphology to the phonology. In both descriptions, there are classes of morphemes with sounds potentially realized as [t], [d], [b], and [x]. It is up to the phonology to determine when each realization occurs. In the line 2, Table 2A relates d and b to TY and PY (i.e., voiced T and P), respectively. The t and x are related simply to T and x. Table 2B is exactly parallel, except for one reversal. The t and x are related to Dh and γH (i.e., UNvoiced D and γ), respectively,

t	d	tb	db	tx	dx	1	t	d	tb	db	tx	dx
T	TY	TPY	TYPY	Tx	TYx	2	Dh	D	DhB	DB	Dhγh	Dγh
		\ /		\ /					\ /		\ /	
T	TY	TPY		Tx		3	Dh	D	DB		Dγh	
Cl	Cl Y	Cl Cl Y		Cl Sp		4	Cl h	Cl	Cl Cl		Cl Sp h	
Ap	Ap	Ap Lb		Ap Do			Ap	Ap	Ap Lb		Ap Do	
	Y_	Y___				5	h_				h___	
Cl	Cl	Cl Cl		Cl Sp			Cl	Cl	Cl Cl		Cl Sp	
Ap	Ap	ApLb		ApDo			Ap	Ap	ApLb		ApDo	

A. Sullivan 1974   B. Lamb 1966/1977

Table 2. Two descriptions of Russian obstruent clusters.

and d and b are related simply to D and B. Line three represents what the phonotactics (PT) accepts. In both Table 2A and 2B the obstruents are accepted in their morphemic order, as this fits the general cluster structure of Russian (cf. Figure 2). Any cluster-final occurrence of Y (in Table 2A) or of h (in Table 2B) is realized in that position, but any non-final Y or h is unacceptable and is not realized. In both descriptions, the PT outputs phonemic features in PT order. Note that from line 3 on, there is no distinction between tb and db columns or between tx and dx columns in either Table 2A or Table 2B. This is the biunique level of phonemic contrast rejected in Halle 1959. The important thing in both RN descriptions is that phonemic (UN)voice is located physically at the end of the obstruent sequence. In both descriptions, phonemic (UN)voice is shifted to a position that dominates the entire obstruent sequence, whether there is one obstruent or more, in the hypophonotactics (HPT), given in line 5. That is, the relation of Y/h to obstruent phonemes is linear in the PT but hierarchical HPT.

The descriptions in Table 2A and 2B look almost like mirror images. They both account for the phonetic facts. This makes the choice beween them almost arbitrary. But the difference derives from whether voice (Y, [+vce]) or UNvoice (h, [-vce]) is treated as marked[7]. But before looking at the consequences of this difference, I consider the logical nature and plausibility of the two descriptions.

2. PLAUSIBILITY, CONTRADICTION, AND THE LOGIC OF MARKEDNESS. Consider first the logic of markedness. The question of markedness only arises in the context of an asymmetric choice. A free choice is logically a simple OR relation: [*this* OR *that*]. The alternates are commutative and equivalent, i.e., [*this* OR *that*] ≡ [*that* OR *this*]. In essence, the nature of equivalence here means that a description with the one does not differ in effect or relative simplicity from a description with the other.

If *this* is marked a different situation arises. Under certain circumstances (c) we must take *this* and exclude *that*. Otherwise we cannot get *this* and *that* appears.
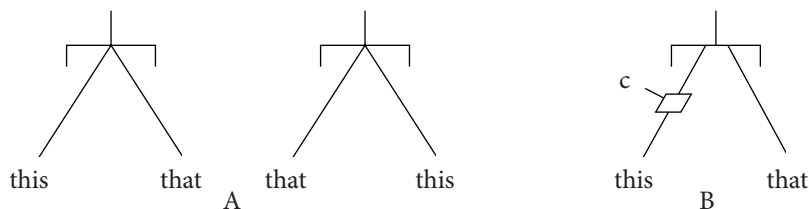
Figure 1. A graphic representation of unmarked (A) and marked (B) choices.

Logically, a marked choice is given by [[c IMPLIES [*this* OR NOT-*that*]] AND *that*]. Clearly no commutative equivalence is possible here. A graphic representation of unmarked choices is given in Figure 1A, of a marked choice in Figure 1B.

We can only reverse a marked choice by turning the descriptive universe upside down, which is at least arbitrary and certainly sounds like a contradiction. That is, in the example of Table 1, by shifting from marked voice to marked UNvoice. Nothing in the phonetic facts requires such a shift. One possible source of justification for this shift could be in the relative plausibility of the two descriptions, what Bloomfield referred to as structural justification or compatibility with established portions of the description[8]. In fact, both descriptions are plausible, though for different reasons.

There are two major reasons for the plausibility of treating voiced obstruents as marked. First, every voiced obstruent phoneme has an unvoiced counterpart. But three unvoiced phonemes, /c/, /č/, and /x/, have no voiced counterparts. Thus, stating the relations of morphemes to phonemic features is marginally simpler if only voice (and not its lack) must be specified. Second, in the position of absolute neutralization (phrase-final) only unvoiced obstruents are found[9].

There are also two major reasons for the plausibility of a description with marked unvoiced obstruents. First, the normal mode of speech requires voicing. Thus the unvoiced sounds are in a distinct minority over all. Second, the motor cortex must send consecutive, overlapping signals to the lungs and vocal folds to produce the voicing. The 'normal mode' mentioned above means that during speech, these signals continue automatically unless they are cut off. This suggests a relationship from the linguistic system that signals the end of voicing at the appropriate time. Equally well, it could be a relationship that prevents an automatic signal from being sent. Either description fits the logic of the situation. Both favor marked unvoiced obstruents.

Thus both descriptions are clearly plausible, but the plausibility arguments are just as clearly skew. I see no way to choose the better set on an empirical basis. Moreover, the second argument favoring marked unvoiced obstruents is not even phonetic. It is an inference drawn from neurological evidence[10]. But this is the key to the whole situation.

3. THE COMPLETE DESCRIPTION. The complete description is given in Figure 2. The relations from morphology are on the upper left, the PT next to them, and the hypophonotactics (HPT) on the right. Beginning at the upper left, note the lines labeled
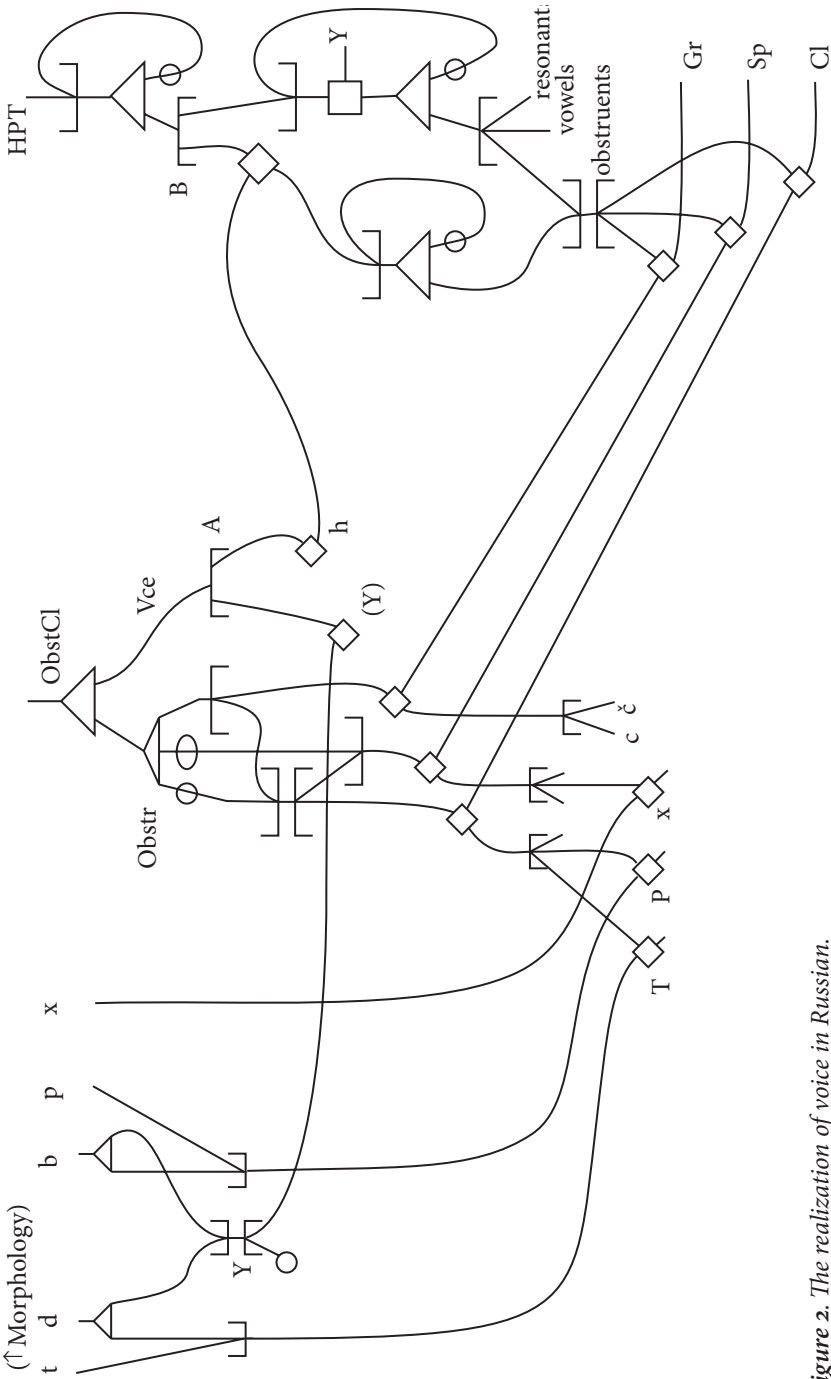
Figure 2. The realization of voice in Russian.

t, d, b, p, and x. They relate to classes of morphemes and are part of a RN description of spell-out rules for morphemes. Thus, for example, t and d are related 'upward' to the classes of morphemes that are related 'downward' to apical stops. The d is related downward to two points. One is a neutralization with t as the archiphoneme T. The other is phonemic voice, labeled Y. Conversely, t is related only to the archiphoneme T. Parallel relationships hold for the other obstruents. Individual obstruents are related to obstruent classes: T and P to stops, characterized phonetically by a relation to oral closure (Cl); x to fricatives, characterized phonetically by a relation to spirant friction (Sp); and c and č to affricates, characterized phonetically in CSR by groove release (Gr). These phonological classes are related to the obstruent cluster (ObstCl: details unimportant for the present study but providing 1-3 obstruents in a row). If phonemic voice (Y) is present in final position of ObstCl, it takes precedence over lack of voice at A. This is so far almost identical to the description presented in Sullivan 1974. At this point there is a divergence: Y has no direct phonetic realization. If voice is not present, the right-hand branch of A is active and a signal is sent to encode the series of obstruents via the left-hand branch of B. Anything else that passes the HPT, vowels, sonants, or voiced stops, is encoded through the right-hand branch of B. Each time the loop on the right hand branch of B is active, another signal for voice is sent from the linguistic system, presumably to the motor cortex in a fully neurological model. Obstruents can be encoded via either branch of B, depending on the conditions in the PT. Everything else can only be encoded via the right-hand branch of B. This represents the full logic underlying the phonetic facts concerning voice in CSR.

Now note the critical nodes regarding voice markedness: A in the PT and B in the HPT. In A the left-hand (marked) branch is related to voice and the right-hand (unmarked) branch is related to a lack of voice. In B the situation is reversed. The left-hand (marked) branch of B is related to a lack of voice and the right-hand (unmarked) branch is related to voice. Thus voiced obstruents are in the marked relation in the phonotactics, as in Sullivan 1974, which parallels JCH. The advantages of simplified specification of phonemic features are retained. Similarly, unvoiced obstruents are in the marked relation in the HPT, as in Lamb 1966 and 1977. The advantages of automatic voicing for everything else and of repeated signals for voice are retained. The voice relation is linear in the PT and hierarchical in the HPT, as in both Sullivan and Lamb.

In short, with all the logic underlying all observed facts concerning 'voice assimilation' in Russian, the description is complete and the advantages of both approaches are preserved. No actual contradiction exists.

4. CONCLUSIONS. The conclusions are clear. I list them without discussion.

1.   You must start from the observed facts—**all of them**—neurological as well as phonetic. (Inference: Lamb and Yngve are both correct.)
2.   Your description must be logically consistent throughout and must account for the logic of the system that underlies all of the observed facts. (Inference:

Lamb and Jakobson/Sullivan were both correct, as far as they went, but none of them went all the way.)

3. Actual contradictions are fatal but can only be exposed by explicit logic.

4. Apparent contradictions can teach us something, as here, where both choices are plausible.

5. AFTERWORD. Trubetzkoy 1939 had an interesting view of privative relations and markedness. In his view column 2 in Table 1 shows unmarked voiced obstruents and column 3 shows unmarked unvoiced obstruents. Jakobson wanted voicing as a phoneme, rather than as just a component of a segmental phoneme, and he was correct in this. But Trubetzkoy was correct to insist that the choice of the marked member of the opposition may differ in different contexts, as nodes A and B in Figure 2 show. We need to remember the work of these men, because it's too much trouble to keep reinventing the wheel.

---

[1] I do not attribute such a belief to Yngve.

[2] Halle 1959, surely the best-known phonology of Russian to date, has some significant phonetic gaps in his description of word-final obstruent clusters. Therefore I restrict myself to that portion of obstruent cluster phonology wherein the facts are complete and comparable in all descriptions.

[3] Markedness, applied to linguistics by Jakobson and Trubetzkoy in the 30's, is a categorization tool with a long and respected history in scientific classification. Before DNA mapping, it was the major (or only) tool in biological classifications.

[4] I do not mean to make Yngve into a straw man here. Most of his criticisms of linguistic research are well-taken, and there is surely a contradiction of sorts here.

[5] I stipulate the need for a level of biunique contrast in phonological descriptions; archiphonemic neutralization follows necessarily.

[6] The Coleman paper in the present volume discusses the varying usages of *data* vs. *examples* in the linguistic literature. Without disagreeing with either the facts or the observations in that paper, I would like to add an observation of my own: we exist in a society and have to speak in a way that lets us be understood, if we are heard. Sometimes this means using inapt or inappropriate metaphors, as I did in answering a question on the Polish linguist list not long ago. But even that doesn't always work. Almost the only place I'm understood accurately is LACUS. Thus I refer to forms (phonetic or graphic) and observations or facts.

[7] A Chomskyan phonologist once told me that 'playing games' like that with markedness, which he assumed to be a universal, is '…almost heresy'.

[8] Again, Bloomfield was right about the general descriptive practice in science. Of course, the practice does not guarantee freedom from all possible error.

[9] Halle 1959 refers to the ease of stating rules for assimilation as well. But rules of his sort are an artifact of the descriptive model, so I ignore this argument. Moreover, the way these 'rules' are described in the two RN descriptions (Table 1C and 1B) can be shown to be equivalent. The proof is easy but requires space, so I leave it as an exercise for the reader.

[10] Bringing in neurological evidence used to be the surest way to start an argument in most linguistic gatherings. In fact, the denials of the relevance of neurological evidence were so heated and so categorical that it was generally much safer to avoid any appeals to neurology. Reference to neurological evidence (or its lack) still starts arguments in certain venues.

REFERENCES

HALLE, MORRIS. 1959. *The sound pattern of Russian.* The Hague: Mouton

JAKOBSON, ROMAN, E. COLIN CHERRY & MORRIS HALLE. 1953. Toward the logical description of languages in their phonemic aspect. *Language* 29:34–46 [Reprinted in JAKOBSON, ROMAN. 1962. *Roman Jakobson: selected writings, vol. 1,* 449–63. 's-Gravenhage: Mouton.]

LAMB, SYDNEY M. 1966. Prolegomena to a theory of phonology. *Language* 42:536–73.
———. 1977. Thrashing classical phonemics. *LACUS forum* 2:154–63.

SULLIVAN, WILLIAM J. 1974. The archiphoneme in stratificational description. In *Proceedings of the XIth international congress of linguists*, ed. by Luigi Heilmann, 287–99. Bologna: Casa Editrici il Mulino.

TRUBETZKOY, N. S. 1939. *Grundzüge der Phonologie. Principles of phonology*, trans. by Christiane A. M. Baltaxe. Berkeley: University of California Press

YNGVE, VICTOR H. 1996. *From grammar to science.* Amsterdam: John Benjamins

❧

EVIDENTIAL RELIABILITY IN PRAGMATICS: A COMPARISON OF ETHNOGRAPHIC AND ELICITATION APPROACHES

Donald Weasenforth
George Washington University

Sigrun Biesenbach-Lucas
American University

RESEARCH IN PRAGMATICS has informed the fields of cross-cultural communication, interlanguage pragmatics and second language acquisition, and applied linguistics and second language teaching. Of particular interest has long been the study of speech acts, the way in which speakers of a language use that language to accomplish communicative purposes, such as requesting goods and services, apologizing for infractions, extending compliments and expressing gratitude. Linguistic data for speech act research can be gathered in a variety of ways and can take various forms. Researchers can observe people use language in natural situations and tape-record what people say; in this approach unprompted language constitutes data. Alternatively, researchers may construct discourse completion tasks (DCTs)—written scenarios designed to elicit from subjects in writing what they would say in given situations; such an approach yields language which has been elicited by deliberate prompts in a different medium. In contrast, researchers may engage subjects in role play situations in which they act out the targeted speech acts; researchers may also use introspective verbal report interviews which ask subjects to verbalize the mental processes they engage in when performing speech acts. Respectively for these last two approaches, data consist of prompted language and introspection about language use. This variety of data collection methods and data types raises questions about reliability: reliability of the data's reflection of natural language use and reliability of results across research studies.

Most speech act studies have relied on DCTs (Beebe & Cummings 1996; Kasper & Dahl 1991; Robinson 1991) as these are an efficient means for gathering a large amount of data quickly; however, there is evidence that speech act realizations elicited through DCTs do not accurately reflect what people would say if the data had been gathered naturally through observation. In other words, the reliability of DCTs has been called into question (Beebe & Cummings 1996; Hartford & Bardovi-Harlig 1992; Kasper & Dahl 1991; Rintell & Mitchell 1989) as they are likely to elicit what subjects *think* they would say. Studies comparing naturally occurring speech acts with speech act realizations elicited through DCTs have until now focused on spoken language and written representations of spoken language in DCTs. In contrast, the present study focuses on a comparison of natural speech act production that occurs in a written medium and DCT elicitation: the purpose is to compare requests students make of professors via electronic mail with requests elicited through a DCT quantitatively, in terms of relative occurrence of levels of directness (Blum-Kulka, House & Kasper 1989), and

qualitatively, in terms of relative occurrence of syntactic forms. Findings can provide further evidence for the reliability or unreliability of elicited data collection in the investigation of speech act realization.

1. BACKGROUND

1.1. NATURALISTIC DATA COLLECTION AND DCT ELICITED DATA COLLECTION.

Two main methods of data collection have been used in research on speech act realization: ethnographic, naturalistic methods and elicitation methods using DCTs. Naturalistic methods yield spontaneous language data from speakers in natural, rather than contrived, situations and thus provide data reflective of speech act production as it occurs in the real world. However, data collection may be very time-consuming, as the speech act under investigation may not occur naturally very often. Also, speakers' backgrounds are difficult to control, and relevant information such as age, educational background, and ethnicity may not be obtained at all (Beebe 1992). Also, if recording equipment is used, it may be intrusive and adversely affect speech act production—Labov's (1972) Observer's Paradox; if researchers rely on note-taking techniques, the accuracy of the data may be affected by the researcher's selective memory (Beebe 1994).

DCT elicitation methods collect data in artificial, contrived situations, prompting respondents to produce a targeted speech act in an imaginary situation that does not entail any real world consequences. Respondents read written scenarios of an oral communication situation for which they provide a written response. This method allows researchers to control respondents' background and situational factors and to gather a large amount of data quickly; also, it provides insights into the norms perceived by native speakers and the structure of the targeted speech act (Beebe & Cummings 1996; Hartford & Bardovi-Harlig 1992). However, the linguistic realization of the speech act and the relative occurrence of forms may differ from actual use; due to the artificiality of the situation, respondents may give expected responses, including a response where no response would naturally occur or responses in forms which would not occur in an actual situation. Moreover, respondents can opt out of the task altogether, not providing a response at all (Bonikowska 1988). Further, DCT responses are highly dependent on stimuli/prompts (Roever 2000) that can easily be misinterpreted, thus producing an unintended speech act (Cohen 2001; Kasper & Dahl 1991). Finally, DCTs have been found to encourage fronting of information (Beebe & Cummings 1996; Hartford & Bardovi-Harlig 1992); the non-interactive nature of the elicitation method tends to decrease the number of turns necessary to fulfill a language function, leading to a collapsing of information.

1.2. COMPARISON OF DATA COLLECTION METHODS.

In studies comparing natural speech act data with DCT elicited data, most researchers have found that results differ. DCTs yield shorter and less complex data than their naturalistic counterparts (Beebe & Cummings 1996; Bodman & Eisenstein 1988; Hartford & Bardovi-Harlig 1992; Rintell & Mitchell 1989). Hartford and Bardovi-Harlig (1992:45) found that

DCTs restricted the range of semantic formulas used to realize the speech act (in that case, rejections) as well as the multiple turn negotiations typical of natural data, and they promoted the use of more direct requests with concomitantly fewer status-preserving strategies. They observed that 'the more difficult the situation is to negotiate in real-life the greater the difference between natural and elicited data,' pointing to the influence of the artificiality of speech elicitation tasks.

The data in these studies typically come from a relatively small sample of respondents, involve different speech acts, and represent different relationships between speakers; thus it is difficult to generalize from their results. In a comprehensive review of numerous speech act studies, Kasper and Dahl (1991) discuss the validity of various data collection methods with respect to how well these approximate natural speech act production. They confirm Wolfson, Marmor and Jones' (1989) call for the collection and analysis of naturally occurring data and urge more comparative studies of different elicitation techniques that take into account the entire speech event in which the speech act occurs.

1.3. E-MAIL AS COLLECTION METHOD. Electronic mail data appear to overcome a number of the shortcomings of naturalistic data pointed out above: it is naturally occurring data that does not need to be recorded and transcribed; turns tend to be collapsed into one message, as writers need to address multiple aspects in one message in order to avoid lengthy, day-long exchanges; writers write in natural, rather than contrived, situations; observed language structures constitute natural language use; and data can be tailored to meet ethnographic standards as writers' background and situational factors can be identified by the researcher. Until now, e-mail data has been used in few linguistic studies (Danet 1999; Murray 1986) and in only one pragmatics study (Hartford & Bardovi-Harlig 1996), which examines a small sample of student requests to faculty. Due to the limited research using e-mail data in pragmatics studies, generalization of findings may be limited. Electronic discourse may differ from spoken language, but e-mail offers new frontiers to the research of naturalistic speech act production.

2. METHODS. Two sets of data were collected and compared in order to identify the possible differences that might result from data collection methods. In both cases, the point of comparison was the requests from students to professors for permission to submit a course assignment later than the assigned due date, a highly face-threatening act.

2.1. ETHNOGRAPHIC DATA. A total of 83 e-mail messages from 64 students—28 native speakers (NSs) and 36 non-native speakers (NNSs)—to one of two American professors provided the ethnographic data for the study. From these messages, 87 requests (44 from NSs and 43 from NNSs) were identified. All messages were unprompted and were the initial messages of an exchange of messages in the cases where a series of messages were involved. The naturally occurring e-mail messages used in this study are ethnographic (cf. Beebe & Cummings 1996) in the sense that the context of the

messages was well defined. The identities of the students and professors were known and aspects of the situation (e.g., time of request, due dates of assignments, types of assignments) were likewise identifiable. All messages were submitted via university-based webmail.

2.2. ELICITED DATA. Responses to a discourse completion task (DCT) served as comparable elicited data. Fifty-seven students (30 NSs and 27 NNSs) were asked to respond to six DCT prompts (see Appendix) by handwriting requests for permission to submit an assignment late under various situations, providing 342 responses. The situations had been identified as important contextual factors determining the acceptability of such requests (Weasenforth & Biesenbach-Lucas 2001) and included: 1) requesting *after* the due date and *without* any attached work, 2) requesting *before* the due date, and 3) requesting *after* the due date *with* attached work. In all three situations, students were asked to write their requests under two conditions: having a good reason for the request, and not having a good reason for the request. Students were asked to complete the DCT, and an accompanying questionnaire, at their leisure outside of class and to return both forms to the researchers. A total of 325 requests (166 from NSs and 159 from NNSs) for late submission were included in the data for the present study.

2.3. PARTICIPANTS. The present study included 53 NSs and 62 NNSs. All NSs were students in a TESOL program at an American university; NNSs were either TESOL students (16) or advanced-level ESL students (46). All students were graduate students and all were highly proficient in English. One NNS student whose e-mail was analyzed also completed the DCT; five NS students were represented in the DCT and e-mail data.

2.4. ANALYSIS. Student requests were analyzed for grammaticolexical indexicals of directness and categorized according to three levels of directness following the widely used framework for analysis of requests by Blum-Kulka, House and Kasper (1989). Only the head act—the most explicit form of the request—of each request was analyzed. Supportive moves (e.g., apologies, justifications, commissives) were identified in the process but are not discussed in this paper. The three levels of directness used in this study are defined as follows:

    Level 1: Direct Requests are requests for which the illocutionary force is most transparent. They raise no doubt on the part of the reader about the import of the message; no interpretation is required. The grammaticolexical forms linked to directness in this study include the following: Performatives (e.g., *I'm writing to request an extension of the due date*); Questions (e.g., *Is there a chance of getting an extension?); Need* statements (e.g., *I need an extension*); and Imperatives (e.g., *Please consider an extension*).

    Level 2: Conventionally Indirect Requests are requests which are modified syntactically or lexically so that the directness is somewhat veiled. These requests are realized through the use of: Preparatory condition using *Could/Can/Would*

you…, Could/May/Can I/we… (e.g., *Could you give me three more days? Can I have an extension?);* Hedged performatives (e.g., *I would like to ask you to extend the due date*); Preference statements (e.g., *I'd appreciate an extension. I'd like you to grant me an extension*); Embedded constructions with complementizer/infinitive (e.g., *I hope that you'll give me more time. I hope to get the assignment done by tomorrow*); and Embedded constructions (including conditionals), including *I am/was wondering if it's possible to get an extension*; Logical conditionals (e.g., *If you could give me more time, I would have a better paper*); Acceptability conditionals (e.g., *If it is OK, could I get an extension?)*, and other conditionals (e.g., *I would be thankful if you could give me an extension*).

Level 3: Non-Conventionally Indirect Requests are requests that require interpretation, requests for which the illocutionary force is not transparent and could simply be understood as a statement: Hints (e.g., *I have difficulties to submit my assignment on time).*

3. RESULTS

3.1. VARIATIONS IN DATA ACCESS. The type of data collection method selected determined the type and amount of data which was accessed. While the DCT was a much more efficient method of collecting a large set of data (cf. Beebe & Cummings 1996), it also constrained the data. Some respondents (11%) chose not to respond to all DCT prompts at all (cf. Bonikowska 1988) or provided responses other than the expected request type (cf. Cohen 2001). The DCT also forced a response when it would not otherwise be used. A number of students noted that they would not ask for an extension under any circumstances, or not via e-mail; however, they did nevertheless in the DCT. On the other hand, the DCTs elicited useful information that would not have been available through ethnographic methods (Hartford & Bardovi-Harlig 1992). Some respondents, for instance, indicated that they would make requests for late submission in person but would not do so by sending an e-mail message to a professor. All students also provided insight into what they considered good and bad reasons for asking for an extension. These types of information are useful in identifying aspects of the communication context which determine pragmatic variations and provide insight into different cultural expectations (Beebe & Cummings 1996).

E-mail will also restrict the amount of data a researcher obtains. Some students reported that they would not ask for extensions of due dates in e-mail, although they would do so in person, because they felt that e-mail was an inappropriate medium. These requests would not be available to a researcher looking only at e-mail messages. On the other hand, e-mail may provide the distance needed for some students to make such face threatening requests (Daft, Lengel & Trevino 1987; Drake, Yuthas & Dillard 2000).

Differences in the number of requests due to collection method are reflected in Figure 1, which provides the percentages of all requests according to the pragmatic situation.
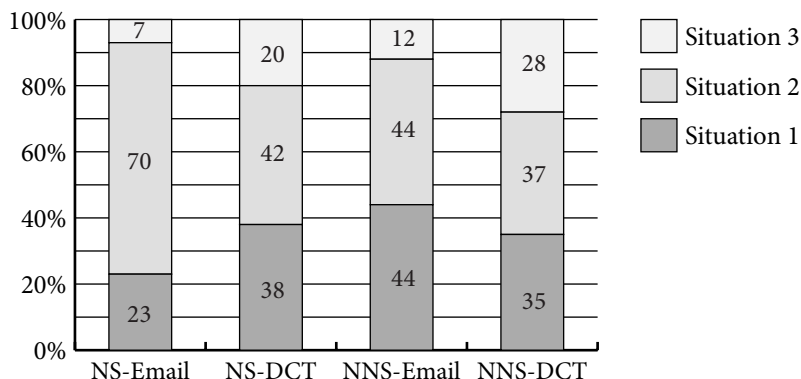
Figure 1. *Requests by situation. (**Situation 1** = requesting* AFTER *the due date and* WITHOUT *attached work,* **Situation 2** = *requesting* BEFORE *the due date;* **Situation 3** = *requesting* AFTER *the due date and* WITH *attached work.)*

One of the most obvious differences across collection methods is the greater use of requests by NSs in e-mail (70%) as compared to the DCTs (41%) in Situation 2. For both NSs and NNSs, the proportion of Situation 3 requests increases in DCTs. Both differences may be due to the fact that the threat of asking for an extension, especially after the due date, is neutralized in the DCTs, since the students are not actually in that situation and face no real-life consequences (Beebe & Cummings 1996; Bodman & Eisenstein 1988; Robinson 1991). With the exception of NNSs in Situation 1, it appears that the DCTs prompted requests which do not occur in actual e-mails, probably due to the face threatening and awkward nature of asking for an extension after the due date. From a cross-cultural perspective, it is interesting to note that the distributions of requests for NSs and NNSs are similar for the elicited data but very different for the e-mail data. The different profiles raise questions about the reliability of research results based on either set of data.

3.2. VARIATIONS IN DISTRIBUTION OF REQUESTS.

3.2.1. REQUESTS VERSUS SUPPORTIVE MOVES. The same questions are raised in light of the distribution of messages with head act requests (in addition possibly to supportive moves) versus messages with supportive moves only. Figure 2 provides the percentages of all messages and DCT responses that included only supportive moves; that is, they did not include any form of request. E-mail messages and DCT responses for NSs and NNSs for each pragmatic situation are represented (e.g., NS1 represents messages/responses for the NS participants for Situation 1). With only one exception, there are more supportive move-only e-mail messages than DCT responses for both NSs and NNSs. The variations in deployment of semantic formulae and the differences across collection methods are most apparent in NNSs' messages/responses for Situation 3 (cf. Hartford & Bardovi-Harlig 1992). The smaller occurrence of supportive-move only DCT responses may be due to the test-like and artificial nature of
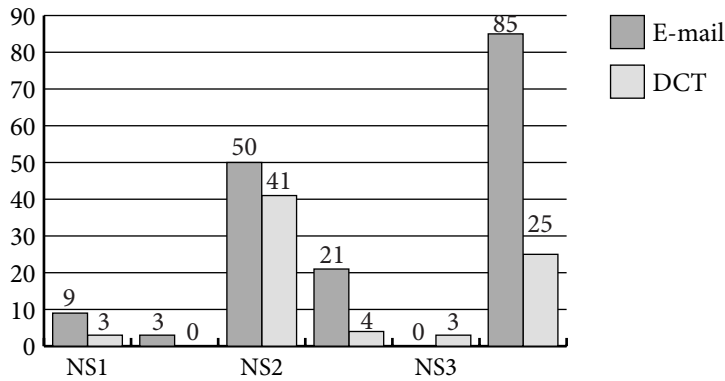
Figure 2. *Supporting move-only messages/response.*
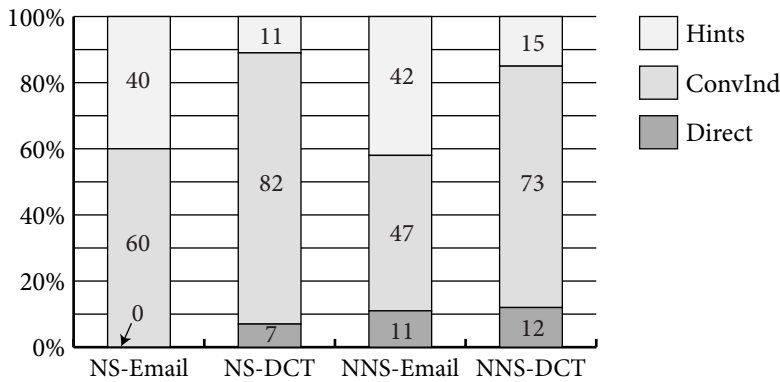
Figure 3. *E-mail vs. DCT for Situation 1.*

DCTs, which neutralizes the threat of asking for an extension. However, students may feel less threatened in sending an e-mail in which they apologize, justify their lateness, and/or commit themselves to submitting work by a certain time rather than actually requesting an extension.

3.2.2. COMPARISON OF COLLECTION METHODS BY SITUATION. Analyses of the level of directness of requests for each pragmatic situation also reveal differences in data associated with data collection method. Figure 3 provides percentages of requests by level of directness for Situation 1 (requesting after the due date without attaching work) and shows that differences between NSs and NNSs are more apparent in e-mail than in the DCTs. Further, for both groups of participants, there is greater use of direct and conventionally indirect requests and a decrease in unconventionally indirect requests in DCT responses. This general tendency toward more directness in the DCT responses is consistent with Hartford and Bardovi-Harlig's (1992) findings that NNSs used more assertive forms of requests in DCTs. The testing nature
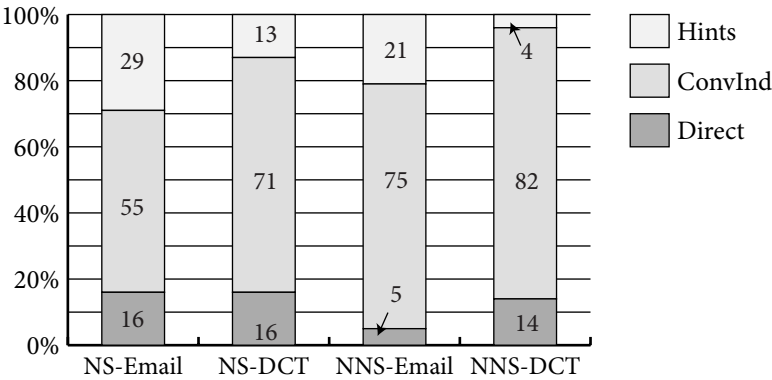
Figure 4. *E-mail vs. DCT for Situation 2.*
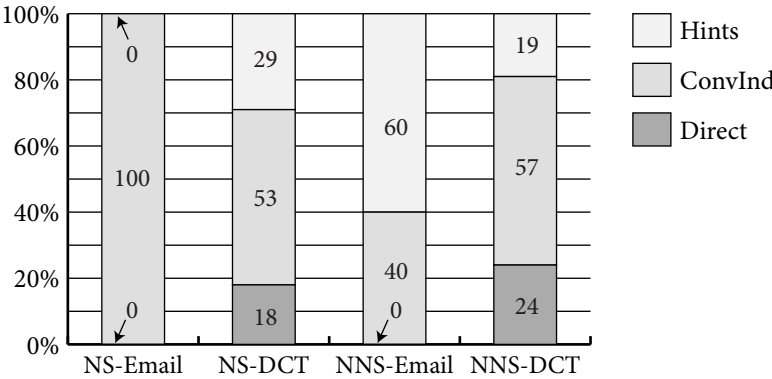
Figure 5. *E-mail vs. DCT for Situation 3.*

of the DCT may have prompted students to make an explicit request. The reality of social implications may also account for less directness in e-mail. In e-mail requests to real professors—especially in the situation of asking for an extension after the due date—hints may be seen as safer.

Figure 4 represents the distribution of requests by directness level for situation 2, requesting an extension before the due date—the least face-threatening of the three situations. The results reveal tendencies very similar to those for Situation 1. In general, students are more direct in their responses to the DCT. Both groups use fewer hints in the DCTs, and NNSs use more direct requests than they do in e-mail.

As Figure 5 shows, the results for Situation 3 (requesting an extension after the due date and attaching work with the request) differ somewhat from those of the previous two situations. Similar to the other two situations, there is a greater use of direct requests in DCTs, and fewer hints in DCT responses, at least from NNSs. Interestingly, there are no occurrences of direct forms in the e-mail messages of either group.

This is possibly due to the bold, face threatening nature of asking for an extension after the due date while submitting the assignment at the same time. Since the threat does not really exist in responding to DCTs, students are more likely to provide such requests, neglecting the social protocol. The differences across groups are also interesting. As pointed out above, while the profiles for NSs and NNSs in the DCT data are again similar, the two groups appear quite different in the e-mail data.

3.3. VARIATIONS IN LINGUISTIC FORMS. The two data collection methods yielded notable differences in how students linguistically realized their requests. Contrary to previous research (Beebe & Cummings 1996; Hartford & Bardovi-Harlig 1992), the DCTs in this study yielded a greater variety of linguistic forms than did the e-mail data. Four linguistic forms–performatives (*I am writing to request …*, *Can you…*, *Would you…*, and *Do you think…*)–occurred in DCT responses but not in e-mail requests. This might be due to the smaller sample of e-mail messages, but differences across collection methods occur for each group also. NSs used *Need* statements and *I wanted to know…* in e-mail but not in DCT responses. Conversely, they used imperatives, *Would you…*, and Preference statements in DCT responses but not in e-mail. They also rarely used Acceptability conditionals in DCT responses but relatively often in e-mail. NNSs, on the other hand, did not use, or rarely use *Would you…* and *Could you…* in e-mail but often used both forms in DCT responses.

4. CONCLUSIONS AND IMPLICATIONS. The comparison of naturalistic and elicited data collection methods has shown that both collection methods limit the amount and type of data collected. While the DCT offered insights into reasons for variations in language use due to the design of the DCT and comments made by the respondents, this collection method also promoted a greater use of direct (all but NSs in situation 2) and conventionally indirect request forms (all but NSs in situation 3) and a lesser use of hints (all but NSs in situation 3) than was gathered through the e-mail data. Also, the occurrence of semantic formulae in the realization of the entire speech act varied across collection method for both groups. It is possible that the artificiality and test-like nature of the DCT accounts for these findings; the risks entailed in use of direct forms are neutralized in a contrived situation with no real-life consequences. In addition, the profiles of NSs and NNSs looked rather similar when considering the DCT data, but differences between NSs and NNSs were more obvious in the naturalistic data. Thus, researchers studying cross-cultural pragmatic differences in request forms would get a very different picture of the differences and similarities of the two groups depending on the data collected.

Variations in linguistic form across the two methods yielded a comparatively small range of forms in the naturalistic data, which is not consistent with previous studies (Beebe & Cummings 1996; Hartford & Bardovi-Harlig 1992; Rintell & Mitchell 1989) but might be the result of the strictly controlled context and stable relationship between requester and request granter. The two collection methods also produced interesting differences between NSs and NNSs: *Could you…*, *Can you…*

constructions were used by NNSs, but not by NSs, pointing to NNSs' lack of pragmatic awareness.

As each method of data collection has advantages and shortcomings, a triangulation of methods is ideal (Cohen 2001; Hartford & Bardovi-Harlig 1992; Kasper & Dahl 1991). Natural data allow researchers to identify salient characteristics of pragmatic context which can be tested in DCTs; in contrast, DCTs can reveal explanations for variations in naturally occurring data, e.g., some DCT respondents indicated that they would not make certain types of requests via e-mail. However, an important conclusion from the present study remains that some pragmatic differences may not surface in DCTs, but do surface in natural data; thus, speech act research cannot rely on elicited data alone if it aims at drawing reliable conclusions about NSs' and NNSs' pragmatic performances.

APPENDIX: DISCOURSE COMPLETION TASK

Directions: Read the following situations and then write the request which you would e-mail to your professor.

1. You did not submit a major assignment on time and have not asked for permission to submit it late. You still have not completed the assignment and you now want to ask the professor for permission to submit the assignment late.

    Assuming you have a good reason for your request, in your e-mail message you write:

    _____

    _____

    Assuming you do **not** have a good reason for your request, in your e-mail message you write:

    _____

    _____

2. Approximately 2 weeks before a major assignment is due, you conclude that it will not be possible for you to complete the assignment and submit it on time. You want to ask the professor for permission to submit the assignment late.

    Assuming you have a good reason for your request, in your e-mail message you write:

    _____

    _____

    Assuming you do **not** have a good reason for your request, in your e-mail message you write:

    _____

    _____

3. You did not submit a major assignment on time and have not asked for permission to submit it late. You decide to e-mail the assignment and attach the assignment 2–3 days late.

   Assuming you have a good reason for your request, in your e-mail message you write:

   _____

   _____

   Assuming you do **not** have a good reason for your request, in your e-mail message you write:

   _____

   _____

REFERENCES

Beebe, Leslie M. 1992. Questionable questions. Paper presented at the 24th Annual tesol Convention, Vancouver bc.

————. 1994. Notebook data on power and the power of notebook data. Paper presented at the 26th Annual tesol Convention, Baltimore md.

———— & Martha C. Cummings. 1996. Natural speech act data versus written questionnaire data: How data collection method affects speech act performance. In *Speech acts across cultures*, ed. by Susan M. Gass & Joyce Neu, 65–86. New York: Mouton de Gruyter.

Blum-Kulka, Shoshana, Juliane House & Gabriele Kasper (eds.). 1989. *Cross-cultural pragmatics: Requests and apologies*. Norwood nj: Ablex.

Bodman, Jean & Miriam Eisenstein. 1988. May God increase our bounty: The expression of gratitude in English by native and non-native speakers. *Cross currents* 15:1–21.

Bonikowska, M.P. 1988. The choice of opting out. *Applied linguistics* 9(2):169–81.

Cohen, Andrew. 2001. Research methods for the study of interlanguage pragmatics. Paper presented at the 35th Annual tesol Convention, St. Louis mo.

Daft, R.L., R. H. Lengel & L. K. Trevino. 1987. Message equivocality, media selection, and manager performance: Implications for information systems. *MIS quarterly*, 355–66.

Drake, B., K. Yuthas & J. F. Dillard. 2000. It's only words—impacts of information technology on moral dialogue. *Journal of business ethics* 23(1):41–59.

Danet, Brenda. 1999. "Feeling spiffy": The changing language of e-mail. Annual Hugo Mueller Lecture presented at the American University, Washington d.c., October 7, 1999.

HARTFORD, BEVERLY & KATHLEEN BARDOVI-HARLIG. 1992. Experimental and observational data in the study of interlanguage pragmatics. In *Pragmatics and language learning*, vol. 3, ed. by Lawrence F. Bouton, 33–52. Urbana IL: DEIL.

——— & ———. 1996. "At your earliest convenience": A study of written student requests to faculty. *Pragmatics and language learning*, vol. 7, ed. by Lawrence F. Bouton, 55–96. Urbana IL: DEIL.

KASPER, GABRIELE & MERETE DAHL. 1991. Research methods in interlanguage pragmatics. *Studies in Second Language Acquisition* 13(2):215–47.

LABOV, WILLIAM. 1972. *Sociolinguistic patterns*. Philadelphia: University of Pennsylvania Press.

MURRAY, DENISE. 1986. Composition as conversation: The computer terminal as medium of communication. *Writing in nonacademic settings*, ed. by Lee Odell & Dixie Goswami, 203–28. New York: Guildford Press.

RINTELL, ELLEN M. & CANDACE J. MITCHELL. 1989. Studying requests and apologies: An inquiry into method. In Blum-Kulka et al. 1989, 248–72.

ROBINSON, M. 1991. Introspective methodology in interlanguage pragmatics research. In *Pragmatics of Japanese as native and target language* (Technical report #3), ed. by Gabriele Kasper, 27–82. Honolulu HI: University of Hawaii, Second Language Teaching and Curriculum Center.

ROEVER, C. 2000. Web-based elicitation of speech acts. Paper presented at the Annual Convention of the American Association of Applied Linguistics 2000. Vancouver B.C. March 2000.

WEASENFORTH, DONALD & SIGRUN BIESENBACH-LUCAS. 2001. Just a little bit longer: A contrastive pragmatic analysis of requests for late submission. Paper presented at the Annual Convention of the American Association of Applied Linguistics, St. Louis MO, February 2001.

WOLFSON, N., T. MARMOR & S. JONES. 1989. Problems in the comparison of speech acts across cultures. In Blum-Kulka et al. 1989, 174–96.

❧

ISSUES IN HARD-SCIENCE LINGUISTICS

VICTOR H. YNGVE
University of Chicago

WHAT IS HUMAN OR HARD-SCIENCE LINGUISTICS? I take linguistics in the usual sense as a discipline with a research community, a cohesive literature, faculty and students in academic departments, and organized professional societies. The term *linguistics* does not rule out interest in systems of communicating used by the deaf, or interest in facial expressions and body motion. Linguistics in this broad sense embraces a freely expandable scope of interrelated phenomena that have long been recognized as being both individual and social in nature.

Essentially all leading linguists for the past two centuries have accepted the goal that linguistics is or should be a science. No particular body of theory, however, grammatical or other, has emerged as acceptable to all. Indeed, linguistic theory has suffered frequent changes as one school after another has achieved a measure of dominance. And the proper shape of linguistic theory has throughout history been the subject of heated but inconclusive debate.

1. LINGUISTICS AND SCIENCE. So what, then, is hard-science linguistics? It is called hard-science linguistics to distinguish it from most current brands of linguistics which are properly characterized as soft science. There are a number of crucial differences that can be a source of confusions if they are not kept well in mind:

(1) Hard-science linguistics is a natural science, like physics, chemistry and biology. This is its major difference from current approaches, which are philosophically based.

(2) Hard-science linguistics takes science seriously. As a natural science it studies parts of the real physical world. Current linguistics, on the other hand, studies non-physical constructs.

(3) Thus hard-science linguistics focuses primarily on people from the point of view of how they communicate, and on the sound waves of speech, the light waves of gestures, and other physical means of communicative energy flow. It also includes consideration of other relevant parts of the real physical world. Soft-science linguistics, on the other hand, focuses on nonphysical constructs such as language and signs.

(4) In the hard sciences it is standard practice to test the predictions of theory against the real world through careful observation and experiment. But in the soft sciences theories are not testable against the real world.

(5)   In the hard sciences theories are testable because the hard sciences study observable real-world objects, whereas the soft sciences study unobservable nonphysical objects, for which there is no objective evidence.

(6)   The hard sciences have developed standard objective criteria for deciding what to believe about the natural world.

    (a)   The criterion for assessing theories is that their predictions must agree with the results of tests through real-world observations and experiments.

    (b)   The criterion for assessing observational and experimental results is their reproducibility.

These criteria cannot be applied in the soft sciences, which do not study the real world. They are forced to fall back on various a priori philosophical criteria such as simplicity, symmetry, 'naturalness', etc. or on sheer intuition about what is plausible. Investigators can and do differ on these matters, which are subjective and arbitrary. The result is that the discipline then drifts from one fashionable body of theory to another and from the dictates of one charismatic linguist to another.

(7)   The hard sciences have paid careful attention to what assumptions they are willing to accept. They have pared them down to only four standard assumptions. The soft sciences, on the other hand, freely admit as many untestable assumptions as they wish. The four standard assumptions of the hard sciences are:

    (a)   that there is a real world out there to be studied;

    (b)   that it is coherent, so we have a chance of finding out something about it;

    (c)   that we can reach valid conclusions by reasoning from valid premises;

    (d)   that observed effects flow from immediate real-world causes.

All other assumptions have been converted into hypotheses to be tested. Those that do not pass tests against real-world evidence or that are untestable have been eliminated. Soft-science linguistics, however, typically accepts a number of scientifically unjustified special assumptions that take it outside of the natural sciences. There are assumptions about utterances, language, meanings, signs, and typically dozens of others, either explicit or implicit and hidden.

2. TWO INCOMPATIBLE GOALS AND A DILEMMA. It has not been generally realized that the current difficulties in linguistics stem in large part from the incompatibility of the modern goal of making linguistics a science and the traditional goal of studying language.

Accepting language as an object of study leads to accepting the scientifically unjustifiable special assumptions of a philosophically-based program of grammatical and semiotic research that can be traced back to the ancients. In hard-science linguistics we must continually be on guard against traditional soft-science assumptions that

threaten to lead us astray. It's a question of priority of goals. If we give priority to studying language, we cannot have a true science. If we give priority to science, we must give up the goal of studying language.

Giving up language in favor of science would be a victory for linguistics, not a defeat. If we study the people who speak and understand rather than studying language, we can actually build a genuine hard-science linguistics that can stand among the other natural sciences and take the place of the present autonomous soft-science linguistics.

The other natural sciences are built on hard-science foundations and each has a conceptual structure that is specific to its subject matter. Physics studies selected parts of the real world from the physical point of view and has concepts of mass, energy, momentum, force, and so on. Chemistry studies parts of the real world from a chemical point of view and has concepts of atoms, molecules, valence, reaction rate, and so on. Hard-science linguistics also rests on hard-science foundations rather than on the traditional semiotic-grammatical foundations. It studies selected parts of the real world, people, from the point of view of how they communicate. To support such studies a new subject-matter specific conceptual structure on which to build a new hard-science linguistics is now available (Yngve 1996). This replaces the conceptual structure of grammar.

But if we start over and build a new linguistics on hard-science foundations and the new conceptual structure, we are faced with a dilemma. There is a vast literature accumulated over centuries containing a wealth of linguistic knowledge, almost all from a soft-science point of view. Thus it is based on or incorporates many scientifically unjustified assumptions. The dilemma is whether to try to make use of this vast treasure and possibly be misled by it, or to ignore it and risk losing the many valid insights it may contain.

For two centuries we have been trying to make linguistics a science, not by moving it onto proper hard-science foundations but by continuing to give priority to the study of language rather than to science. In this we have been encouraged by those philosophers trying to redefine science and legitimize the soft sciences. The result has been a soft-science linguistics dedicated to studying an object, language, introduced only by scientifically unjustified and untestable assumptions. After two centuries it is now clear that this course has led only to confusion and chaos in the discipline.

What we must do instead to resolve the dilemma is to make use of what is already known where possible, but leave the old world of the soft sciences once and for all and become pioneers in the new world of the hard-sciences. We must mount a program of research to *reconstitute* linguistics on hard-science foundations and the new conceptual structure that is now available.

3. RECONSTITUTING LINGUISTICS ON HARD-SCIENCE FOUNDATIONS. Let us see how one might begin. Suppose two people are in conversation. See Figure 1. There are three distinct physical things here (shown below the double line), person A, person B, and the sound waves that pass between them. In hard-science linguistics all the linguistic structure lies in these two people and in the pair of them communicating

Hard Science			Soft
linkage **The couple**			
role part **husband**	**2**	role part **wife**	
participant **the man**	**1**	participant **the woman**	*L? - SH?*
individual		individual	*utterance?*
Person A	**sound**	**person B**	**sound**
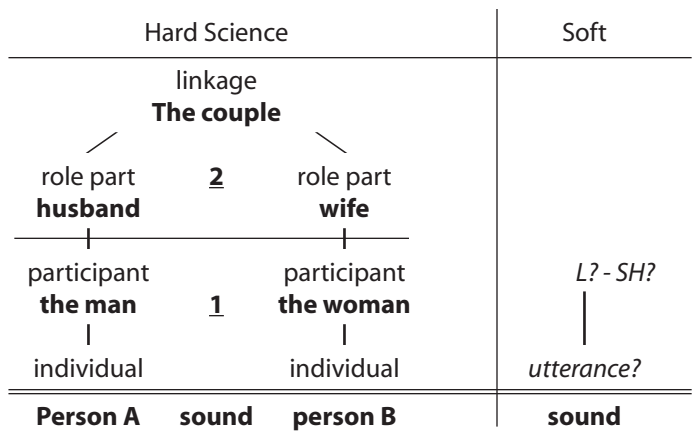
Figure 1. Two persons communicating by means of sound waves. (Real-world objects are in **bold face**, the corresponding systems are in *light face*.)

by sound waves and other energy flow. The linguistic structure is represented in the theory in terms of the properties of systems, where 'property' and 'system' are taken in their normal sense in the natural sciences. The communicating individuals, participants, role parts, and the linkage are all systems in this sense. The sound waves are seen simply for what they physically are, pure energy flow: They carry no linguistic structure at all.

In the tradition, on the other hand, the sound waves are seen as somehow supporting assumed utterances that are analyzed in terms of some theory of language, L, and that in some mysterious way carry a meaning or message or information from one person to the other, who are then seen as 'using' language.

Note that the linguistic structure postulated for utterances is not at all inherent in the physical sound waves. It cannot be recorded by instruments. The similarities and differences, on which phonemes were postulated for example, require a person to perceive them, and different persons perceive them differently. Traditional theory does not take any persons into account. A proper scientific linguistics must be a linguistics focused not on language but on people, for that's where the linguistic structure really is.

I have chosen this example of husband and wife for clarity because we happen to have a name for the two of them, a couple, The couple is part of the real physical world. You could invite them to dinner. The couple, together with the sound waves and other relevant parts of the real world, are represented in the theory as a linkage, which is characterized by properties in the same way as the other systems.

Besides couples there are a variety of other assemblages of multiple persons and objects that can likewise be analyzed as linkages. Linkages can participate in higher-level linkages in a hierarchy of individuals and linkages. In this way we can represent the linguistic structure of the many systems in a complex community or society.

4. TWO NEW DOMAINS OF THEORY. So we see that hard-science linguistics provides two domains of theory (shown above he double line in Figure 1): domain 1 at the individual and participant levels (below the single line), and domain 2 at the role part and linkage levels (above the single line). This represents the fact that communicating is both individual and social. These two domains of theory differ in the real-world evidence, individual or social, on which the properties of the systems are postulated. Properties of individuals and properties of participants, **1**, are set up on the basis of observed similarities and differences of individual persons. Properties of role parts and properties of linkages, **2**, are set up on the basis of observed similarities and differences of assemblages of people and objects at the social level. Comparing two objects separately with a physical measuring scale is, of course, equivalent to comparing them directly with each other.

The tradition, on the other hand, provides only one domain of theory, language, and it has never been clear whether it is individual or social or some abstraction which is neither.

Note that Chomsky's ideal speaker-hearer, SH in figure 1, does not answer to any real-world evidence from real persons. There is nobody underlying it that you could invite to dinner. Incredibly, it is defined entirely in terms of language, which does not exist in the real world, and is completely subservient to the assumptions and definitions of a particular linguist. In fact, even the structured utterances here, or the text in other theories, has to be introduced by a special subject-matter-specific assumption, as Bloomfield already pointed out.

5. PROPERTIES OF SYSTEMS. We observe that everyone is different communicatively from everyone else. The uniqueness of the individual is expressed in terms of postulated properties of the systems that model them. Properties are set up on the basis of observed real-world communicative similarities and differences of persons and assemblages. They may be taken as binary variables without loss of generality. These properties can be represented here as:

> A B <u>C</u> D E F <u>G</u> H I J K L M <u>N</u> O P <u>Q</u> R S T U ... ,
> where the underlined properties figure in the discussion
> below concerning how properties change.

Hard-science linguistics is often called *human linguistics* to distinguish it from the traditional linguistics of language. The name human linguistics is particularly apt since its theory is founded on the very uniqueness of individuals and groups celebrated in the humanities rather than on the normative basis of grammar which would make everyone the same.

Properties change dynamically as people learn and speak and understand. Some properties change quite rapidly, others may stay fixed for longer periods of time. The knowledge of how to communicate is represented in terms of properties.

We also observe that communicative behavior is heavily context dependent. Properties reflect both inputs to the system and its current state, which represents the current context in which the ongoing communicative behavior is understood.

It has been shown that the structures of properties in the various systems such as individuals, participants, role parts, and linkages can all be organized according to the same conceptual structure and formal theory.

6. PROCEDURES. Properties are structured in part in terms of procedures. Procedures are dynamic causal laws of communicative behavior postulated on the basis of observational and experimental evidence from the real-world objects modeled. A procedure specifies how some property changes value in dependence on the current values of other properties, some of which may represent the situation or context, others the results of inputs.

Procedures are triggered when specific properties (inputs and context) take the values specified in the logic expression on the left. They then change the value of a property as specified on the right after a specified time delay Δt, as for example:

> **C x -G v N :: Q, Δt**,
> where **x** is 'and', **v** is 'or', - is 'not' and **::** is read as 'sets' (the indicated
> property on the right to the indicated value).

Thus communicative behavior results in change, not accretion, so hard-science linguistics is basically pragmatic in its foundations, in contrast to the tradition, which treats pragmatics as an afterthought if at all.

The current situation or context that affects ongoing communicative behavior is represented in what is called the domain of control. The execution of procedures in dependence on the dynamically changing properties in the domain of control answers to what would be spoken of colloquially as a person following a conversation or being 'with it'. This accords with the observation that what a person says or understands in any given situation depends on the situation, which is dynamically changing. Thus the proper handling of context is a central feature of the theory, and a major difference from grammar. Rules of grammar do not involve the situation or context in this ongoing sense. They are generally set up on the basis of examples taken out of context.

The term human linguistics is thus apt for another reason. Since it focuses on what individual persons do and say and understand in particular circumstances, it accommodates the uniqueness of situations of the humanities. It can do this in a true science because it generalizes in terms of individual properties rather than in terms of a whole language.

The linguistics of language, in concentrating on separating the grammatical from the ungrammatical, is not far removed from the prescriptive tradition and ideals of correctness.

Procedures are often organized in terms of a hierarchy of communicative tasks and subtasks. There can also be parallel tasks to accommodate the possibility that a person

sales linkage
Sales Assemblage

role part	role part	prop part
customer	**clerk**	**merchandise**
participant	participant	prop
collector	**dealer**	**antique**
individual	individual	e.g., vase
Person A	**person B**	**real object**
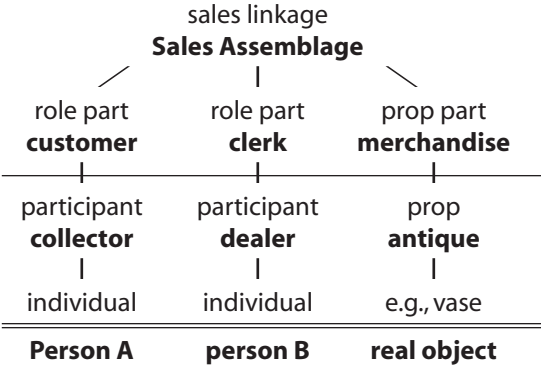
Figure 2. Two people bargaining over the price for an antique. (Real-world objects are in **bold face**, the corresponding systems are in light face.)

or a group may do several things at the same time. Communicative tasks are often sub-tasks of nonlinguistic tasks representing what it is that the communicative behavior is coordinating. Thus hard-science linguistics is closely connected with other social and social psychological disciplines and interfaces naturally with practical affairs. It does not suffer the isolation of language and autonomous grammar.

The organization of properties, procedures, task hierarchies, etc. of a system is called the plex structure of that system. So in hard-science linguistics we observe and experiment on how people communicate so as to postulate and test plex structures for them. This is quite different from taking people as informants or witnesses for the study of language.

If we keep well in mind the kinds of differences highlighted here so as not to be confused or misled by them, it should be a fairly straightforward though lengthy and challenging task to reconstitute all of linguistics piece by piece on hard-science foundations.

7. EVIDENCE. Let us now look at some more examples with a particular focus on evidence. Evidence is always relative to the theory that it is interpreted in terms of —the theory it is designed to support or contradict. Thus it is also relative to any hidden assumptions underlying that theory. Hidden false assumptions can lead to false interpretations. The depth hypothesis proved untestable because of hidden false assumptions underlying the phrase-structure theory it was interpreted in terms of.

In hard-science linguistics we seek evidence for properties of systems modeling real-world people from the point of view of how they communicate, not evidence for properties of immaterial objects like words and sentences. We seek evidence for similarities and differences between different persons and the same person at different times, and between different groups and the same group at different times.

Let us take the example of two people bargaining over the price for an antique offered for sale. See Figure 2.

First, there are properties postulated and tested against evidence from individual persons. Here at the *individual* level there are general properties representing the person's knowledge, abilities, aspirations, values, etc. and at the *participant* level there are properties concerned with how much the collector would be willing to give for the antique during this bargaining session and properties concerned with how little the dealer would be willing to accept.

Second, there are properties postulated and tested against evidence from assemblages of persons and other associated real-world objects. Here at the *role-part* level there are properties concerned with customer offering, clerk refusing, making a counter offer, customer making a show of walking out, etc., and at the *linkage* level, there are properties representing the current changing state of negotiation of the sales group, the antique, the money, and other relevant real objects.

As another example, consider two strangers getting acquainted, as videotaped and described in earlier publications.

Here at the *individual* level we find properties representing the personal backgrounds of the two persons (that they later discuss) and at the *participant* level there are properties representing the original request of the researcher separately to each, what little he told them about each other, and the task he gave them of getting acquainted.

Regarding social properties: at the *role part* level are properties representing each person's moves and responses in dialog, and such things as checking with each other on the extent of their actual commonalty in knowing what the task is. Thus we find the woman saying, 'and, I don't know how much you know about me—at all', and he says, 'I know nothing about you at all. It's all a big secret'. To which she replies, 'All right—should I start, then?' And he says, 'You start (single nod)'. In this way the information about their common assigned task is moved up from the individual participant level to the social role-part level. They both now know what activity 'start' refers to. We see them both get acquainted. Then at the *linkage* level are properties of the changing current state of their dialog and their growing acquaintanceship.

There are also ongoing investigations of historical change, linguistic variation, multi-lingualism, translation, etc. (Yngve & Wąsik forthcoming). The specific nature of evidence in each case is different and relative to the area under investigation.

In spite of what some philosophers of science may say, scientific research is not focused single-mindedly on so-called falsification. We're in the physical domain, not the logical domain of theorems and proofs. In seeking the truth about nature, we emphasize real-world exploration. Research in the physical domain is more like a detective trying to solve a crime. There's no single simple route to a guaranteed solution. A number of techniques are available to us including field observations. videotapes, and interviews.

In hard-science linguistics, we are all studying the same physical reality, people, from the point of view of how they communicate, and the relevant physical surroundings. Thus evidence and theory from one area of linguistics is often relevant to the study of questions in other areas and even in neighboring disciplines that study people from other points of view. Linguistics thus moves from the isolation of

autonomous grammar into the real world of the natural sciences. We can say good-bye to the era of grammatical fads and fashions and enter a new era for linguistics where our theories will have the solidity, permanence, and real-world relevance already familiar in the other natural sciences.

REFERENCES

YNGVE, VICTOR H. 1996. *From grammar to science: new foundations for general linguistics.* Amsterdam: John Benjamins.
——— & ZDZISŁAW WĄSIK. forthcoming. *Exploring hard-science linguistics.* London: Continuum.

CLASSICAL ARABIC VS. DIALECTS: A COMPARATIVE STUDY

Fa'eqa Alsadeqi
University of Bahrain

THE ARABIC LANGUAGE exists in two forms; the classical (sometimes called standard or formal, hereafter CA) and the dialects. CA is a highly sophisticated language with complex grammatical rules and irregularites; it is reserved for official use and is a lingua franca among Arabic speaking communities. The dialects are spoken languages that vary from one region to another, and they are sometimes unintelligible to Arabs in different parts of the Arab world.

Similarities between CA and the dialects have led Arab grammarians and linguists to compare CA to the dialects now spoken. Some argue that the dialects developed from the CA; others say that the CA is a variety of Arabic that was spoken in the past. When one examines data from Arabic dialects, one finds many common features that are exclusive to dialects come to the surface, indicating that their grammar differs from that of CA, should one consider the latter to be the standard.

This paper is a comparative study of some grammatical features of both forms of Arabic. Its purpose is to show through grammatical evidence that CA is a language that was probably not spoken but rather contrived for special purposes. (This is not to dispute the existence of CA, but its existence as a spoken language.) How and when this form of Arabic came about is unsure, because of the lack of evidence from the pre-Islamic era, but we know that CA was used for poetic works. Special fairs were set up in pre-Islamic times for poetry and literature (Hilaal 1998 and Lutfi 1976). In such markets, the language of poetry was the *fus-ħaa* 'classical', although people reverted to their own dialects when communicating in private and social matters (Hilaal 1998).The famous *muᶜallaqaat* 'suspended' poems were said to have been selected as the best poems composed during pre-Islamic times. 'They were hung up in the *Ka'aba* in Mecca on account of their merit; that this distinction was awarded by the judges at the fair of *ᶜukaz* near Mecca', where poets competed and the successful compositions were transcribed in letters of gold (Nicholson 1985). None of these poems reached the Arabs in their written form, because the oral tradition was prevalent. To retrieve the old poems, Arabs had to rely on narrators such as ħammaad, who memorized a lot of poetry and could remember them. It is claimed that the Arabs were able to document most of the pre-Islamic poems thanks to *ħammaad 'al-raawiya* 'the narrator'.

Because Arabs considered poetry to be the best form of literature, little attention was paid to prose. Nicholson (1985) states that 'since the art of writing was neither understood nor practised by the heathen Arabs (i.e. Arabs in pre-Islamic times) in general, it was impossible that prose, as a literary form, should exist among them'. Therefore, most of the documentation in later stages concerned poems.

1. CLASSICAL ARABIC. What do we know about CA? To find evidence in a language, we need to examine what is written and what is spoken. What can we say about CA based on the available written and oral data?

The current use of CA may shed some light on its use as an exclusive language. To summarize, here are some features specific to CA.

1. Arabic is a Semitic language. Logically, therefore, CA is also a Semitic language.
2. Classical Arabic is the only Semitic language that retains all cases. (Some cases are retained in Akkadian as well.) That is, it is a fully vocalised language, where cases are realized via the vowels a, i, u and endings.
3. CA is generally reserved for official use, media reports through television and radio, and religious ceremonies. The official news on all Arabic speaking radio stations or television channels is presented in written form to the newsreader, who reads it in CA. In interviews or live reports, either CA alone or a mixture of both CA and dialect or the dialect alone is used.
4. CA is a written language that exists in and through texts.
5. The people who speak CA are the educated class who are either trained or trained themselves to use it (often with mistakes) or religious figures or scholars. To repeat, you need to study it to speak it fluently.

The above indicate that CA was and still is an exclusive language.

2. THE GRAMMAR OF CA. The grammar of CA was first considered during the time of the fourth Caliph, 'Ali bin Abu Taaleb, who agreed that the grammarian 'Abul 'Aswad 'Al-Du'ali should write it down (Shaami 1997). The reason is given in a story often mentioned in Arabic grammar sources . Before citing this curious story, let me explain two grammatical rules to make it easier to understand the text.

The word *maa* in classical Arabic has various functions. For example, if it is used to form a question, then the noun following it should be in the nominative case, indicated by the vowel /u/; if, however, it is used for exclamation, then the noun following it should be in the accusative case, indicated by the vowel /a/.

The story runs as follows. 'Al-Dua'li's daughter was looking at the sky one day and she exclaimed *maa 'ajmalul samaa'*, using the nominative case indicated by the vowel /u/ (she meant how beautiful is the sky). Her father thought she was asking him a question and said 'What part of it is beautiful?' The daughter replied 'I meant how beautiful the sky was'. The father replied, 'In that case you have said *maa 'ajmalal samaa'*, using the accusative case'. Due to the mistake his daughter made, 'Al-Du'ali was alarmed and decided that the grammar of CA should be written down in order to preserve it. So he immediately went to the Caliph, who comissioned him to work on this project.

There are, however, there are no extant written documents on grammar by 'Al-Du'ali, although he is credited with the initial steps. Arabic references say that he

invented the dot system to differentiate consonants and the signs used for vocaliza-tion were later concocted by his students. The story, in my opinion, indicates: (a) CA was not a natural language and had to be taught, and (b) people were not used to vocalizing the ends of their words in speech.

The first complete work on CA grammar was documented in the 7th century AD by Sibawayh, a scholar of Persian origin and a student of ʿAl-Duʾali. His motive was to preserve the language, because people were making mistakes, or *laḥn* 'sole-cism', when using it. Why were people making mistakes? The claim was that many nationalities moved to the Arabian peninsula after the introduction of Islam and they were affecting Arabic in a negative way. This seems dubious, because the movement of non-Arabs to the Arabian peninsula was not prominent until later stages in Islamic history (probably at the end of the Ummayyid dynasty). In fact, the languages that were spoken in areas conquered by Arabs were supplanted by Arabic or marginalised, thus causing some languages. e.g. Coptic and Syriac, to disappear as living languages (Versteegh 1997).

Many historians and linguists feel that the divergence between the CA and the vernacular was the principle motivation for the the emergence of grammar as an independent discipline (Versteegh 1997). In addition, Arab grammarians wanted to unify Arabs via a lingua franca and preserve the Holy Quran, in the which the highest or best form of CA is used. Before Islam, Arabic was the language of its own people in the Arabian peninsula, but later it became 'the language of a large empire, in which it functioned as the language of religion, culture and administration' (Versteegh 1997). It was logical, then, to have a grammar for the dominant language, so that every Moslem, regardless of origin, could understand the Holy Quran, which was a social constitution as well as a religious text. Other evidence that CA is linked directly with the Holy Quran is the official use I mentioned earlier; media reports find reporters and newsreaders using both CA and dialects. This is not the case with religious inter-views or religious lectures. They are conducted in pure CA.

3. CLASSICAL ARABIC AND THE DIALECT OF QURAYSH. When philologists tried to codify the Holy Quran, one of the problems they faced was the varied readings of the text. The so-called professional readers of the Quran during the time of the Caliphs tried to promote their own readings, which were, I suspect, based on their own dia-lects. As a result, *ʿIbn Majaahid* confirmed seven authentic readings of the Holy book (Hilaal 1998). *ʿIbn ʿabbaas* followed suit, but said there were ten readings. Scholars, however, suggest that more than seven readings of the Quran are acceptable, due to the number of tribes and dialects at that time. So despite efforts to unify the language, the vernaculars interfered, rendering other readings of the Quran valid.

Another point worth mentioning is that Arab historians say that the Holy Quran was largely based on the dialect of Quraysh, a tribe that lived in the Hijaaz region, because the people of this area were the most fluent speakers of Arabic. Yet we find many causes for doubt here, arising from what we know about the geography and language of Quraysh. I cite some of them now.

1. The Prophet Mohammad once said 'I speak Arabic fluently, although (*bayda*) I am from Quraysh'. The word *bayda* 'although' was interpreted as *because* by some historians, a most exceptional rendering of the word in Arabic.

2. The geographical map of the Hijaaz region differs from one book to the next. Some books say that Hijaaz is the area from the border of Iraq to the western coast of Saudi Arabia, others say it extends from central Saudi Arabia to the west coast of the peninsula. Thus it is unclear where the Quraysh lived.

3. Sometimes linguists talked about the difference between the language of Quraysh and that of Hijaaz. This is strange because Quraysh was in fact part of Hijaaz and not a separate region. Again this question concerns inferences drawn from the geography of the region and not the language.

4. Another problem regards any claim of Qurayshis as more eloquent speakers of Arabic than members of neighboring tribes. Normally, the people who live close to each other speak almost the same form of language (unless we are talking about urban vs. adjacent rural areas). In fact, the literature at hand admits that many tribes (e.g. Tamiim, Huthail, Bani 'assad, Rabii'a) had eloquent Arabic speakers.

5. Some examples of grammatical rules pertaining to case endings contradict the rules cited in books. For example, the people of Hijaaz were said to use the nominative where the people of Tamiim used the accusative when negating with the word *maa*. Upon examination of some poetry of the people of these regions we find that the usage is exactly the opposite. If the rule given were accurate, the poetic usage would be unnatural. A person would apply the rules of his own language or dialect instead of borrowing a rule from another region (Al-Sammirraa'i 1997). This casts doubt on the accuracy of the documented forms. Here are examples on the usage of *maa* in the dialects of Tamiim and Hijaaz.

Tamiim:	*maa 'al-dars**u** sahl**un** (ACC)*
Hijaaz:	*maa 'al-dars**u** sahl**an** (NOM)*
	not the-lesson easy
	'The lesson is not easy'

6. In examples cited by Arab grammarians concerning the pronunciation of the glottal stop in the accusative case, the people of Tamiim retain the sound while the people of Hijaaz change it. In CA the glottal stop is pronounced. Here is an example cited by Al-Sammirraa'i (1994:41):

Tamiim	*'amlaytuhu **'imlaa'an*** (as in the classical form)
Hijaaz	*'amlaythu 'imlaalan* (changing the glottal stop into /l/

In view of the above, we can conclude that the Quran was based on the dialect of Qurayshis not because of their mastery of Arabic, but for the following reasons:

Language	dual masc.	dual fem.	plural masc.	plural fem.
CA	*Katabaa*	*Katabataa*	*Katabuu*	*katabna*
Egyptian	*Katabuu*	*Katabuu*	*katabuu*	*katabuu*
Bahraini	*Kitbau*	*Kitbau*	*kitbau*	*kitbau*

Table 1. *Comparison of CA and dialectal forms of* kataba *'wrote' for masculine and feminine dual and plural.*

Language	plural nominative	plural accusative	plural genitive
CA	*Kaatibuun*	*Kaatibiin*	*kaatibiin*
Bahraini	*Katbiin*	*Katbiin*	*Katbiin*
Lebanese	*Kaatbiin*	*Kaatbiin*	*Kaatbiin*

Table 2. *Comparison of CA and dialectal forms of* kaatibun *'writer, writing (gerund)'* *for nominative, accusative and genitive plural.*

1. The Prophet Muhammad was from Quraysh and since the Holy Quran was revealed to him, it would be in his dialect.
2. Quraysh was the hub of commercial and literary activity. It gathered people from all tribes and regions. As a result, its language became a mixture of various dialects that could be understood by people from different regions. This implies that it was not a pure form of Arabic, but a dialect mixture.

4. CA VS. SPOKEN ARABIC. Many argue that Arabs in the past used to vocalise their words fully, as in CA. But when we examine the dialects, the following are noted:

1. In spoken Arabic and the dialects, there is no vocalization and no case endings, an essential feature of the classical form. For example 'il-walad fil beit 'the boy is at home' as opposed to CA 'alwaladu fil baiti (cf. Table 1).
2. Dual forms and sound feminine plural verb inflections used in CA are absent from the dialects. Eg. 'il-bannat gaaloo 'the girls said' as opposed to 'al-banaatu qulna (cf. Table 2).
3. Dual pronouns and feminine plural pronouns are absent in the dialects. Instead the masculine plural form is used for both the dual and sound feminine. In the classical form *humaa* refers to they (dual), *hunna*, refers to they (feminine plural) and *hum* for (masculine plural). In the dialects, however, only one form (usually the masculine plural) is used for all three forms of the pronoun.
4. The masculine sound plural in active particles is used with one case ending not two, as is the case in CA. In the classical form *kaatibuun* 'writers' (nominative) or *kaatibiin*, (accusative and genitive), in the dialects only *kaatbiin*.

Language	Type	Sentence 'My mother wrote the letter'
CA	VSO	*katabat* (V) *ummii* (S) *ar-risaala* (O) Wrote mother+my the letter
dialect 1	SVO	*ummii* (S) *kitbat*(V) *ir-risaala* (S) mother+my wrote the letter
dialect 2	SVO	*Immii* (S) *katabit* (V) *ir-risaale* (S) mother+my wrote the letter

Table 3. *Word order variations between CA and dialects.*

The same applies to dual forms: in CA *kitaabaan* 'two writers' or *kitabayni*, in the dialects, *ktaabein*.

5. Compound numbers (11 to 19) are pronounced as (1 to 9) + 10. Thus 13 in Arabic would be 3 +10 *thalaatha ᶜashar*. They have one form in dialect, e.g *thalataᶜash* 'thirteen' in the Gulf dialect, regardless of the gender of the noun counted. In CA, however, they are written or pronounced in two ways, depending on the following noun: the first part of the compound number (three) has gender opposite to that of the noun and the second part (ten) has the same gender. To clarify this, here is an example using the number 13 in CA:

13 = three + ten   *thalaatha ᶜashar*
13 books (masc)   *thalaathata ᶜashara kitaaban* (*ta* is the feminine suffix)
13 schools (fem)   *thalaatha ᶜashrata madrasatan*

In addition to the above, the variety between dialects is based on lexical items and phonological differences, but the grammar is almost uniform. In the three major dialect groups, Gulf, Levantine, and North African Arabic, we find different words to reflect the same meaning: 'he wants', for example, would be *yabbi* (Bahrain), *baddu* (Lebanon) , or *ᶜaawiz* (Egypt). This parallels lexical differences in English: American English has *truck* and *apartment*, British English has *lorry* and *flat*.

The phonological differences between dialects and CA lie in vowel lengths and specific consonants. The consonants include the emphatic phonemes (dh, zh, th, q, j).

The grammatical structures are parallel in almost all forms of spoken Arabic. However, SVO is favored in verbal sentences when the subject is third person, while in other cases, we find VSO (cf. Table 3). Linguists classify CA as a VSO language. This is an overgeneralization, since Arabic also has verbless sentences with the subject before the predicate and it also allows OVS. This again shows that dialects differ in their grammatical preferences, if not their structures, when compared to CA. (For a detailed study of some dialects, see Brustad 2000).

Why then do we find only these features common to all dialects? Why can't we find some grammatical elements that are specific to (at least) certain dialects and why (if Arabs used to vocalise the ends of their words) do we not find a tribe or people

who still do? It is true that language changes over time, yet CA has not changed much thanks to the preserving influence of the Holy Quran. But if this is the case, then one would expect to find a region where this formal type of Arabic was spoken.

The problem encountered when studying Arabic linguistically is due to the fact that little attention was paid to documenting the dialects in the past. Whenever examples were cited, they would be to prove a grammatical point related to CA. The late linguist 'Ibrahim 'Al-Saamirraa'i disputed the use of such examples, saying that grammarians concocted them to prove or support a rule in CA grammar ('Al-Sammirraa'i 1994). Hilaal (1998) says that when the grammar of Arabic was written, the dialects, were neglected and whenever they were mentioned, they were considered part of CA. The grammarians would mould the dialects to force them into the framework of CA, and if the examples did not fit, they would be considered ugly or deviant (Hilaal 1998).

5. CONCLUSIONS. The authenticity of documented material on CA is doubtful, making it difficult to find conclusive proof about the existence of this form of Arabic as a spoken language in the past and, if the dialects diverged from CA, then we lack evidence pertaining to the linguistic drift. In addition, the dearth of documentation on Spoken Arabic among various tribes and city dwellers poses another problem for researchers.

Based on what is available on both forms of Arabic, I conclude that CA was probably not a spoken language and therefore the dialects may have not been derived from it. The opposite makes more sense. That is, CA was based on the dialects to make communication easier. Also, since we find no traces of case endings in the dialects, I conclude that the dialect transcriptions need not be contemporaneous with CA, since the latter has not changed. Finally, though CA first came about to facilitate communication among different tribes, it became a unifying force in Islam, intended for all Moslems, be they Arabs or non-Arabs.

The study of the grammar of CA has been exhausted. Most books repeat whatever Sibawayh and his students mentioned with little modification, making no significant contribution to the field. Dialects were ignored for such a long time and in the case of Arabic were sacrificed for the sake of the Classical form. This makes it difficult to find definite answers to some questions. Fortunately, dialectology has developed considerably in the past one hundred years or so, and this should encourage more studies that can be documented on current Arabic dialects, unlike old dialects that have almost all been lost because they were not studied in their entirety.

REFERENCES

'Al-Sammirraa'i, Ibrahim. 1994. *fil lahajaat 'al^carabiyya 'al-qadiima* . Beirut: Daar al-Hadaatha.

———. 1997. *'a-ltatawwur 'al-lughawi 'al-taariikhi*. Beirut: Daar al-Andalus.

Brustad, Kristen E. 2000. *The syntax of spoken Arabic*. Washington D.C.: Georgetown University Press.

HILAAL, ABDUL-GHAFFAR HAAMID. 1998. *ʾAl-lahajaat ʾal-ᶜarabiyya nashʾatuhaa wa tatawwuruhaa*. Cairo: Daar ʾalfikr ʾal-carabi.

HUSAIN, TAHA. 1969b. *min taarix al-adab al-ᶜarabi*. Beirut: Daar al-ᶜilm lil-malajiin.

LUTFI, MUSTAFA. 1976. *al-lugha al-arabiyya fi itaariha al-ijtimaaᶜiyy.*, Beirut: maᶜhad al-ʾinmaaʾ al-arabi.

NICHOLSON, REYNOLD ALLEYNE. 1985. *A literary history of the Arabs*. Cambridge: Cambridge University Press.

VERSTEEGH, KEES. 1997. *Landmarks in linguistic thought III: The Arabic linguistic tradition*. London: Routledge.

ARABIC LANGUAGE RESOURCES

BAKALLA, MOHAMMAD H. 1983. *Arabic linguistics: An introduction and bibliography*. London: Mansell.

HASSAAN, TAMMAAM. 1985. *al-lugha al-ᶜarabiyya maᶜnaaha wa mabnaaha* (third edition). Cairo: al-hayʾa al-misriyya lil-kitaab.

HOURANI, ALBERT. 1991. *A history of the Arab peoples*. Cambridge MA: The Belknap Press of Harvard University Press.

HUSAIN, TAHA. 1969a. *fil ʾadab al-jaahilyy.* Cairo: Daar al-maᶜaarif.

SHAAMI, AHMAD JAMEEL. 1997. *ʾAn-nahw ʾal-ᶜarabi qadhaayaahu wa maraahilu tatawwurihi*. Beirut: Daar ʾalhadhaara.

SIBAWAYH, ABU BISHR CAMR BIN OTHMAN BIN QAMBAR. 1983. *Kitaab Sibawayh*. Beirut: ᶜaalam al-kutub.

WRIGHT, W. 1986. *A grammar of the Arabic language.* Cambridge: Cambridge University Press.

∾

PRONOMINAL EVIDENCE IN SLAVIC AND THE MEANING OF CASES

Barbara Bacz
Université Laval

PRONOUNS, IN CONTRAST TO THE OTHER part-of-speech categories traditionally distinguished in the IE languages (nouns, verbs, adjectives, adverbs and even prepositions) are characterized by the absence of lexical content[1]. Therefore, they are the best representatives of purely grammatical meaning, and their different forms can be regarded as legitimate indicators of possible differences in that meaning.

In a footnote to his 1936 classic on case meaning *Beitrag zur allgemeinen Kasuslehre,* Jakobson suggests that the various morphemic oppositions observed in the forms of pronouns denote **semantic** differences. He mentions three such differences in Slavic signaled by the formal, morphemic oppositions in Slavic pronouns: a) the difference 'animate' versus 'inanimate' manifested through the opposition of *k* and *č* in the declension paradigm of the Russian pronouns *kto*-N.Sg 'who' and *čto*-N. Sg. 'what'; b) the difference in the grammatical category of person indicated through the opposition of the Russian *ja* 'I' versus *ty* 'you (sg)', and *on* 'he', etc., and c), most significant for the discussion to follow, the difference in the grammatical category of case with respect to the case's 'relatedness' to a preposition, manifested in the *j- /n'-* morphemic opposition in the third person Slavic pronouns (Jakobson 1936/1995:535, footnote 17, emphasis added):

(1)   'The pronouns, which, in contrast to the other parts of speech, express not real but formal meaning in their root morpheme, often denote by their root morpheme such semantic differences as are otherwise conveyed as morphological or syntactic oppositions: on the one hand, the categories of animacy and inanimacy (opposition of the root morphemes *k* and *č*: *kto* [N] [who] and *čto* [N] [what], *kogo* [G] [whose] and *čego* [G] [of what], etc.), of person (*ja* [I], *ty* [you (sg.)], *on* [he]) and, on the other hand, in highly unusual fashion **the opposition of relatedness versus unrelatedness to a prepositional construction, which is consistently expressed in third person pronouns by the distinction *n'* versus *j*:** *nego-jego, nemu-jemu* [he], *neë-jeë* [she], and so forth.'

1. PREPOSITIONLESS AND PREPOSITIONAL FORMS OF PRONOUNS. In case languages such as Polish or Russian, the grammatical category of case manifests itself in discourse (i.e. in actual usage) under two forms: a form without a preposition, in cognitivist case semantics referred to as *prepositionless case* (also known as 'morphological' or 'synthetic' case) and a form representing a combination of a preposition and a case-marked category, known as *prepositional case* (sometimes also referred to as 'analytical' case). In the

casual paradigm of both Polish and Russian, third person pronouns (*on, ona, ono, oni, one* in Polish) have two contrasting morphological forms, the *j*-form and the *n'*-form, which are in complementary distribution: the *j*-form is found in prepositionless uses of a given case (such as the prepositionless adnominal genitive in **jego**-G. *ojciec* 'his father'), and the *n'*-form occurs in preposition+case combinations, such as the prepositional genitive with *do* 'to' in *Idę **do niego**-G.* 'I am going to him'. The difference between the prepositionless *j*-forms and the prepositional *n'*-forms is illustrated by the uses of the Polish third person masculine pronoun *on* 'he' quoted in (2).

(2)   Prepositionless *j*-**forms**:   Prepositional *n'*-**forms**:
        G. **jego** 'his'   do/do/z … + **niego** 'to/from/of…him'
        D. **jemu** 'him'   ku/wbrew… + **niemu** 'to/against…him'
        A. **jego** 'him'   przez/w/na..+ **niego** 'by/in/on…. him'

The complete declensional paradigm for the third person pronouns in Polish is reproduced in Table 1.

    As shown by the examples in (2) and the pronominal paradigm in Table 1, the distribution of the *j*- and *n'*- forms in the third person pronouns in Polish is very systematic, the two pronominal forms corresponding almost perfectly to the prepositionless and the prepositional uses of the Polish cases[2].

2. SEMANTIC DIFFERENCE BETWEEN PREPOSITIONLESS AND PREPOSITIONAL CASES. On the assumption that a difference in form indicates a difference in meaning (an assumption which underlies the research of both Jakobson and the contemporary cognitivist semanticists such as Langacker, Rudzka-Ostyn, and Janda), the systematic *j-/n'*- opposition in Slavic pronouns suggests that there is a semantic difference between the prepositionless and the prepositional forms of a given case. In terms of grammar, the difference between the two forms of case can be attributed to the formal (structural) opposition between two categories belonging to two different grammatical levels: the morphological category of a word, represented by prepositionless case, and the syntactic category of a phrase, represented by prepositional case. In terms of the semantics [i.e. the underlying mental representation] of the case-marked elements, the *j-/n'*- opposition in the pronominal paradigm indicates a distinction in meaning between a case-marked bare noun and the same case-marked noun used in a prepositional phrase. That means, to give a practical example, that the speaker's conceptualization (mental representation) of the accusative-marked lexical item *tydzień* 'a week' in (3)a is not identical to the conceptualization of the same, accusative-marked noun in (3)b:

(3) a. Pracował tam **tydzień**-Acc. (prepositionless accusative)
        He worked there **a week**.
     b. Pracował tam **przez tydzień**-Acc. (prepositional accusative)
        He worked there **for a week**.

		Masculine	Neuter	Feminine
Singular	N.	*on*	*ono*	*ona*
	G.	*jego, go, niego*		*jej, niej*
	D.	*jemu, mu, niemu*		*jej, niej*
	A.	*jego, go, niego*	*je, nie*	*ją, nią*
	I.	*nim*		*nią*
	L.	*(o) nim*		*(o) niej*
		Masculine	**Either**	**Non-masculine**
Plural	N.	*oni*		*one*
	G.		*ich, nich*	
	D.		*im, nim*	
	A.	*ich, nich*	*d*	*je, nie*
	I.		*nimi*	
	L.		*(o) nich*	

Table 1. Declensional paradigm of the third person pronouns in Polish (based on Doroszewski & Wieczorkiewicz 1972:91–92).

The difference between two conceptualizations of the same noun with the same case-marked nominal, which depends on whether it is used with or without a preposition, is very difficult to specify because the relationship between the event (the subject's working) and the temporal setting of the event (a week) indicated by the case-marker remains the same. Most grammarians agree that the presence of a preposition in the preposition + case combination makes more specific the relationship expressed by the case-marker. The *j-/n'-* pronominal contrast additionally suggests that the presence of a preposition also affects our mental representation (construal or conceptualization) of the case-marked lexical item, and even if the semantic difference between a case-marked nominal and the same case-marked nominal combined with a preposition is very small, its existence has to be acknowledged[3].

Case semanticists have tried to define the semantic difference between prepositionless and prepositional cases. Jakobson (1936/1995:339) stated it as follows: 'In a language which combines a system of prepositional constructions with an independent system of case, the meanings of the two systems are different in the sense that when prepositions are used, the relation itself is focused upon, while in constructions without prepositions the relation becomes a kind of property of the object denoted'. Langacker (1992) attempted to pinpoint the difference in terms of the Cognitive Linguistics framework by providing two image-schema models of the instrumental case. In my opinion, his (1992:301) explanation of the difference between the prepositionless (which he calls 'true') instrumental indicated by the instrumental case-marker

in typical case languages and the prepositional phrase with what he calls the 'instrumental preposition' in typical non-case languages, such as English, is essentially the same as Jakobson's.

In Langacker's image-schema of the prepositionless instrumental, the prepositionless form profiles the thing denoted by the case-marked nominal. 'The true case-marker profiles a schematically characterized thing and incorporates some specification of its role in the process' (Langacker 1992:301). Conversely, in the prepositional construction, the relationship between the event and the intermediary participant in the event, the instrument, is profiled by the instrumental preposition (ibid.). In other words, in the prepositionless use, the property of being an instrument is ascribed to the case-marked nominal, which then, at a higher level of organization, enters into a relationship with the process evoked by the clause, whereas in the prepositional use, the relational property of being affected via an instrument is part of the process evoked by the clause[4].

3. HISTORY OF THE N' PRONOUNS. The *j-/n'*- opposition in Slavic pronouns is, to my knowledge, the only piece of evidence in linguistic form for postulating a semantic difference between prepositionless and prepositional cases. In view of the fact that the difference in meaning between the two forms of case is not readily apparent (and can be conveyed to a non-linguist merely as a difference in focus), the question of the reliability of the pronominal *j-* versus *n'*- evidence can be raised. The issue of whether the *j-/n'*- opposition in Slavic constitutes satisfactory linguistic evidence becomes even more of a problem when the history of the *j-/n'*- opposition and the origin of the *n'*- pronouns is considered.

According to the historical grammars of Polish (e.g. Kuraszkiewicz 1972:130–31), the pronominal third person *n'*- forms replaced the original suppletive *j*-forms in the declensional paradigm of the pronouns *on, ona, ono* 'he, she, it'[5] when *-n*, the final consonant of the prototypical Slavic prepositions **vъn* (modern *w*) 'in' and **sъn* (modern *z*) 'with' shifted and mechanically attached itself to the locative and the instrumental *j*-forms of the following pronouns, respectively. The shift is illustrated by the examples in (4) taken from Doroszewski and Wieczorkiewicz (1972:92).

(4)   **Forms before the shift   Forms after the shift**
      *vъn-jemъ-LOC. 'in him'   *w nim*-LOC.'in him'
      *sъn-jimъ-INST. 'with him'   *z nim*-INST. 'with him'

The initial *n'* of the prepositional pronominal forms in the locative and the instrumental after the prepositions *w* 'in' and *s/z* 'with' has, with time, generalized to the other prepositions used with these cases (such as *przy nim*-L 'next to him', *po nim*-L 'after him', etc.) and to the other prepositional cases: genitive, dative and accusative (*do niego*-G 'to him', *ku niemu*-D 'toward him', *przez niego*-A 'because of him').

A contemporary example confirming the historical *j-* to *n'*- shift in the morphemic structure of third person pronouns in Slavic can be found in the attested occurrence

of both the *j*- and the *n'*- forms in the prepositional phrase *dzięki niemu/ dzięki jemu*-D 'thanks to him' in modern Polish (Kuraszkiewicz 1972:131)[6].

The original preposition *vъn* 'in' attached to the accusative, as in *vъn -jь*-Acc. 'in him', has produced a contracted prepositional form of the masculine accusative of the pronoun *on* 'he', the form ***weń*** (as in *Wpatrywali się **weń** [w niego] z niepokojem* 'They were staring at him with apprehension' – see Dunaj 1996:629 for more examples), and later, analogical contracted forms of prepositional pronouns in the accusative and the genitive derived from combinations with other prepositions, for example: *doń (do niego)* 'to him', *zeń (z niego)* 'from him', *nań (na niego)* 'on him', *przezeń (przez niego)* 'because of him', etc. In modern Polish contracted *n'*- pronouns are considered a mark of very formal, literary style. A few examples of these uses taken from the 16th century Polish writer Mikołaj Rej (Kuraszkiewicz 1972:131) and from modern literary Polish (Dunaj 1996:629) are given in (5)a and b, and (5)c and d, respectively:

(5)   a. Zgrzytali **nań (na niego**-A) zębami.
          They gnashed their teeth **at him** (= because of him)
      b. Trudno się **oń (o niego**-A) było pokusić.
          It was difficult to be tempted **about him** (= to have him).
                                                (Kuraszkiewicz 1972:131)
      c. Zwrócili się **doń (do niego**-G) z prośbą.
          They turned **to him** with a request.
      d. Gotowi byli **zań (za niego**-G) umrzeć.
          They were ready to die **for him**.
                                                (Dunaj 1996:629)

4. RELIABILITY OF THE *J*-/*N'* EVIDENCE. The shift of the nasal *n* from the final position in a preposition to the initial position in the following pronoun in Slavic can hardly be considered to have been motivated semantically. A parallel to the Slavic example under discussion is provided by the English words *newt* (a kind of lizard), which in fact stands for *an ewt* (from the original AS form *efeta* 'a lizard') or *a nickname*, which is an alternative form of the original *an eke-name* (with the two co-existing forms in ME: *a nekename = an ekename*). A converse shift of the consonant *n* in English from the initial word position onto the preceding indefinite article, observed in the history of the words *apron* (formerly *napron*, from OF *naperon < nape*), *adder* (originally *nadder*, from OE *naddere*) or *umpire* 'a non-pair' (a more recent version of *numpire*, from ME *nonpere,* from OF *nomper, nompair*) has been explained by some historical grammarians bluntly as the result of 'a speaker's mistake' (see Skeat 1980:5).

The *n'*-forms in Slavic pronouns have come into being as a result of a mechanical shift of a consonant in a previous stage of a language. Yet, the *j*-/*n'*- opposition in third person pronouns created through that mechanical operation has come to indicate a semantic difference between the two pronominal forms, and by extension, between the two (prepositionless and prepositional) forms of case.

In my opinion, the way in which a particular formal distinction arose diachronically is irrelevant to its synchronic semantic status. Grammatical systems of a language change just as do individual forms in that language. Thus, the history of a linguistic sign should have no necessary bearing on its significance in present-day systems.

5. HISTORY OF THE TWO POSSESSIVE PRONOUN FORMS IN ENGLISH. The history of the *j-/n'-* opposition in Slavic is similar to the history of the possessive pronouns in English. In the contemporary pronominal system, English possessives are used in discourse under two morphological forms: the short, 'adjectival' form, with the specific paradigmatic realizations: *my, your, his, her, its, our, your, their,* and the long form of the 'possessive pronouns': *mine, yours, his, hers, its, ours, yours, theirs.* The two forms remain in complementary distribution, the adjectival form being restricted to the attributive position in a noun phrase (as in **my** *book*), the possessive pronoun occurring in the predicative position only (as in *This book is* **mine**). The 'adjectival' versus 'truly pronominal' formal opposition in the system of the English possessives indicates clearly (to my mind, at least) that the attributive and the predicative categories (specifically, the category of attributive and predicative adjectives) are not semantically identical, as an early Chomskyan Adjective Transformation would have it. In other words, possessive pronoun evidence from modern English, manifested through the short versus long form morphological opposition, can be considered to prove the existence of a semantic difference between the attributive and the predicative uses of a lexical category[7].

In the earlier stages of English, however, the distribution of the two forms of the possessive pronouns was quite different, and the short (*my*)/long (*mine*) formal opposition did not indicate a semantic difference between the attributive and the predicative systems. The original genitive-case long form of the pronoun was used in the attributive as well as in the predicative position in a sentence, and if the two forms were found in the prenominal position, the short form (which has lost the final *-n*) tended to occur before nouns beginning with a consonant while the full, long form appeared before nouns starting with a vowel (Pyles & Algeo 1993), as illustrated in (6).

(6)   **Possessive pronoun distribution in English**
       My egg/book. : This egg/book is **mine.**   (Modern English)
       Mi book / **min** eg/ey.   (Middle English)

6. CONCLUSION. The *my/mine* example from English shows that the origin of a form (such as a mechanical loss of the final *–n* from the ME *min*) has nothing to do with the form's grammatical distribution, and consequently, with the form's significance in the modern system. Although created through a mechanical shift, the short possessive forms in English, just as the *n'-* forms in Slavic, have eventually become indicators of meaningful oppositions between different grammatical categories. How the difference in meaning between these categories should be defined is a matter of the linguistic theory at our disposal. A systematic opposition of forms, however, always

indicates a difference in meaning, for two different forms in the same category never co-exist for long in the same semantic capacity.

---

1   I would like to thank the two *LACUS Forum* reviewers of this paper for their careful and inspiring comments.

2   The nominative, as the prototypical form of the casual declension, never combines with a preposition; thus, it has no corresponding *n'*- form in the pronominal paradigm. The locative, which is always prepositional, predictably has no *j*-form in the pronominal declension. The Polish Instrumental, however, which can have both the prepositionless and the prepositional realizations (as in: *Szedł żołnierz lasem*-I. 'Was walking a soldier **through the forest**', with the prepositionless instrumental of place, and *Cyganka mieszka za lasem*-I. 'The Gypsy woman lives **beyond/ on the other side of the forest.**', with the preposition *za+Instrumental* combination (prepositional instrumental of place)), is rendered by the *n'*-form of the pronoun only. Kuraszkiewicz (1972: 131) explains this apparent inconsistency in the otherwise strikingly regular pattern of correspondence as an overgeneralization of the *n'*-form which has spread onto the prepositionless uses of the case. His illustrative examples are: *Idę z nim, z nią, z nimi* 'I am going **with him, with her, with them**' (prepositional instrumental) versus *Gardzę nim, nią, nimi* 'I despise **him, her, them**' (prepositionless instrumental).

3   It goes without saying that the specific meaning imported by a preposition has to be compatible with the meaning of the case the preposition combines with. In example (3)b the Polish preposition *przez* 'through, across' combines with no other case but the accusative, so the two senses are compatible almost by definition. However, when a preposition combines with more than one case (as does e.g. the Polish preposition *w* 'in', which 'governs' [(Janda 2000) uses the term 'motivates'] two cases: the accusative *w tydzień* 'in a week' and the locative *w tygodniu* 'during the week'), semantic compatibility of the two elements is much harder to establish.

4   For a discussion and an interpretation of Langacker's 1992 graphic schemas of *Instrumental Case Marker* versus *Instrumental Preposition*, see Bacz 2000:10–12. It should be noted that Langacker's explanation of the difference between the 'true' (prepositionless) instrumental and the instrumental preposition is necessarily cross-linguistic since it is based on examples taken from typologically different languages: the 'true instrumental' represents a morphological case in a typical case language while the 'instrumental preposition' is illustrated by the preposition *with* in English. The semantic import of preposition+case combinations, typical of Slavic, has to be taken into consideration when an explanation of a difference between morphological (prepositionless) and prepositional uses of a case is attempted.

5   Originally, the forms *on, ona, ono* denoted demonstrative pronouns – cf. the archaic Polish uses: *onego czasu*-G. '(at) that time' or *naonczas*-Adverbial 'in/at that moment', the original third person pronouns being: *ji, ja, je* (see Doroszewski & Wieczorkiewicz 1972:91)]

6   According to my native speaker intuition, the expression with the *j*- form (*dzięki jemu*) sounds less natural than the expression with the *n'*- form (*dzięki niemu*), a fact which suggests that *dzięki* 'thanks to' has clearly become grammaticalized as a preposition here.

7   There are other arguments proving that attributive adjectives are semantically different from predicative adjectives, e.g., in Russian and in Polish the so-called 'short adjectives' occur only in the predicative position – cf. **Zdrowy i wesoły** *chłopiec* 'a healthy and happy boy' versus *Chłopiec jest* **zdrów/ zdrowy i wesół/wesoły** 'The boy is healthy and happy'. In

my opinion, the possesive pronoun evidence found in modern English (yet not in Slavic or in Latin—cf. **Moja** *książka* 'my book' and *Ta książka jest* **moja** 'This book is mine' in Polish) is just one more indicator of a semantic difference between attributive and predicative categories.

REFERENCES

BACZ, BARBARA. 2000. On the status of prepositions in case languages: Does preposition govern case? *Langues et linguistique* 26:1–22.

DOROSZEWSKI, WITOLD & BOGUSŁAW WIECZORKIEWICZ (eds). 1972. *Gramatyka opisowa języka polskiego z ćwiczeniami*, vol. 2. Warszawa: Państwowe Zakłady Wydawnictw Szkolnych.

DUNAJ, BOGUSŁAW. 1996. *Słownik współczesnego języka polskiego*. Warszawa: Wydawnictwo Wilga.

JAKOBSON, ROMAN. 1936a/1995. Contributions to the General Theory of Case. In *On language. Roman Jakobson*, ed. by Linda R. Waugh & Monique Montville-Burston, 332–85. Cambridge, MA: Harvard University Press.

JANDA, LAURA. 2000. Cognitive Linguistics. SLING2K position paper. http://www.unc.edu/depts/seelrc. Accessed 1 July 2001.

KURASZKIEWICZ, WŁADYSŁAW. 1972. *Gramatyka historyczna języka polskiego*. Warszawa: Państwowe Zakłady Wydawnictw Szkolnych.

LANGACKER, RONALD W. 1992. Prepositions as grammatical(izing) elements. *Leuvense Bijdragen* 81:1–3, 287–309.

PYLES, THOMAS & JOHN ALGEO. 1993. *The origins and development of the English language.* New York: Harcourt, Brace, Jovanovich College Publishers.

SKEAT, WALTER W. 1980. *A concise etymological dictionary of the English language.* New York: Perigee Books.

❧

TOWARD A BETTER UNDERSTANDING OF CLITIC SYSTEMS

David C. Bennett
SOAS, London

THIS PAPER[1] aims to contribute to our understanding of clitic systems, from which point of view the Slavic languages provide a useful source of data, since they exhibit an especially rich variety of systems[2]. The paper addresses the theme of the conference—the nature of linguistic evidence—in the following three ways. First, since clitics represent an intermediate stage between independent words and affixes, they cry out to be treated diachronically. Secondly, corpus-based data provide important insights into a range of issues. Thirdly, adequate attention needs to be paid to both formal and functional considerations. Sections 1.1, 1.2 and 1.3 of the paper expand briefly on each of these views in turn. Sections 2 and 3 are devoted to on-going changes in the Polish clitic system. Section 4 then briefly discusses the formalization of the diachronic development in question; and the paper concludes with a summary of key points in section 5.

1. CLITICS AS LINGUISTIC EVIDENCE.
1.1. DIACHRONIC CONSIDERATIONS. Clitics are unstressed, and in many cases shorter, counterparts of independent words. Thus the Serbo-Croatian (S-Cr) clitics *ga* 'him' and *mu* 'to him' correspond to the full forms *njega* and *njemu*, respectively. An overview of diachronic developments involving clitics is shown in (1):

(1)	A		B		C		D		E
	Independent words	>	P2 clitics (word-based)	>	P2 clitics (constituent-based)	>	Verb clitics	>	Verb affixes

Second position (P2) clitics sometimes occupy the position after the first accented word of a clause (stage B in (1)), even when it is the first word of a complex constituent, as in S-Cr example (2)a, and sometimes occupy the position after the first syntactic constituent (stage C), as in S-Cr example (2)b[3]. (The clitics in these and subsequent examples are italicized.)

(2)	a.	moj	*će*	*vam*	sluga	dati	rječnik	[Cro]
		my	aux.future	to-you	servant	give	dictionary	
	b.	moj	sluga	*će*	*vam*	dati	rečnik	[Ser]
		my	servant	aux.future	to-you	give	dictionary	

'my servant will give you the dictionary'

Moreover, there is ample evidence that word-based P2 represents an older position for clitics than constituent-based P2. For S-Cr, Browne writes (1975:114):

> in general it is more old-fashioned and literary to break up a phrase by putting the enclitics after the first word… In everyday and conversational style, enclitics are more likely to be put after the whole phrase.

For Slovenian (Sln), Stone reports that the oldest texts reveal examples of clitics interrupting complex syntactic constituents (1996:217). However, a Sln translation of (2) that parallels (2)a—see (3)a—is ungrammatical, the preferred version being (3)b, which is parallel to (2)b:

(3) a. *moj *vam*   *bo*   služabnik izročil   slovar   [Sln]
         my   to-you aux.future servant   hand-over dictionary
     b. moj služabnik *vam*   *bo*   izročil   slovar   [Sln]
         my servant   to-you aux.future hand-over dictionary

Outside of Slavic, Latin (Lat) examples such as (4), in which the subject noun phrase *populus Romanus* 'the Roman people' is interrupted by the clitic reflexive pronoun *se*, represent an older Lat word order than examples in which clitics follow a complex initial constituent (Benacchio & Renzi 1987:5):

(4)   populus *se*   Romanus erexit   [Lat]
      people itself Roman   raised
      'the Roman people rose up'

With regard to the hypothesized shift from the constituent-based P2 stage—C in (1)—to the stage at which the clitics are positioned adjacent to the verb—D in (1)—there is, again, ample supporting evidence. For instance, 'Older Bulgarian was clitic second' (Franks & King 2000:318) but Bulgarian now positions its clitics adjacent to the verb. Similarly, Latin had P2 clitics, of both stage B and stage C, but modern Romance languages such as French and Spanish are at the verb-clitic stage, stage D. And, outside of Indo-European, Steele uses Uto-Aztecan data to support the claim that verb-clitic systems derive from P2-clitic systems (1977:539).

Finally, the change from stage D to stage E—i.e., the development of verb affixes from verb clitics—is also well attested. This has affected the inflections of the French future tense (Decaux 1955:224) and what is now the reflexive suffix *-ся* (*-sja*) of Russian (Jakobson 1971:19). Haas (1977) discusses comparable developments within the Muskogean languages.

The Sln clitics with which we are concerned occur in a fixed sequence in a clitic cluster, and in the vast majority of cases the clitic cluster occurs at P2 syntactically defined, i.e., after the first syntactic constituent of a clause. In other words, Sln seems to be at stage C of (1). Ser and Cro represent transitional stages between B and C, but

are otherwise similar to Sln in that they seem to belong at particular points along the scale shown in (1). However, it is by no means necessarily the case that languages occupy just one position along this scale. Russian (Rus) is a case in point. On the basis of the fact that its reflexive marker is a verb suffix, Rus seems to have reached stage E. However, Rus also has an interrogative marker *ли* (*li*) which is a P2 clitic, and specifically a stage B P2 clitic which follows the first accented word of its clause, as is seen from example (5)a (taken from Franks & King 2000:189) and the fact that (5)b, where *ли* (*li*) follows the clause-initial complex constituent, is ungrammatical[4]:

(5) a. Она не   знает в этой *ли* аудитории будет   лекция
         Ona ne   znaet, v ètoj *li* auditorii   budet lekcija   [Rus]
         she neg know in this Q auditorium will-be lecture
         'She does not know whether the lecture will be in this auditorium'
     b. *Она не   знает в этой аудитории *ли* будет   лекция
         Ona ne   znaet, v ètoj auditorii   *li* budet lekcija   [Rus]
         she neg know in this   auditorium Q will-be lecture

As for auxiliary-verb or pronominal clitics, these have been lost in Russian; and as a result Russian does not have a clitic cluster (Franks & King 2000:188).

The most dramatic shift along the scale shown in (1) is that between P2 clitics and verb-clitics. It is this change to which we shall devote most attention below on the basis of Polish data. (We shall see, though, that Polish also illustrates the change from verb-clitics to verb-affixes.)

1.2. CORPUS-BASED DATA. It is well-known that the Slavic languages have a rather free word order, the function of which is not so much to signal grammatical relations such as 'subject' and 'object' as to signal what information is 'given' and what information is 'new' in a particular discourse context. This point can be illustrated by reference to the Serbian, Croatian, Slovenian and Polish (Pol) translations of a clause from Orwell's *Nineteen Eighty-Four*—see (6)[5]. None of the four Slavic translations reproduces the syntactic structure of the English original, with its passive verb and agentive prepositional phrase, but each preserves the discourse structure of the original to the extent that the equivalent of the noun *fear*—which in each translation is the subject of its clause—is placed in the unmarked focus position at the end of the clause. The four Slavic versions are also in agreement with each other in placing the adverbial phrase 'at every step', the verb and its subject in that order—constituents 1, 2 and 3 in the Cro and Sln versions, constituents 2, 3 and 4 in the Ser and Pol versions. The first constituent of the Ser and Pol versions is a resumptive adverbial expression—*ipak* 'nevertheless' and *mimo to* 'despite that'—which reinforces the meaning of the preceding concessive clause, 'although he had a good pretext for coming here'. (The decision not to assign numbered positions to the clitics in these examples merely reflects a desire not to prejudge the issue of their syntactic status.)

(6)   he was haunted at every step by the fear that…   [Eng]

ipak   *ga   je*   na svakom koraku proganjao strah, da [Ser]
nevertheless him aux.past on every step   persecuted fear   that
—— 1 ——   ——— 2 ———   — 3 — — 4 —

na svakom koraku progonio *ga   je*   strah,   da   [Cro]
on every step   persecuted him aux.past fear   that
——— 1 ———   — 2 —   — 3 —

ga   je   pri vsakem koraku preganjal   strah,   da   [Sln]
him aux.past   at every step   persecuted fear   that
——— 1 ———   — 2 — — 3 —

mimo to,   nieustannie prześladowała *go*   myśl,   że   [Pol]
despite that unceasingly persecuted   him thought that
— 1 —   — 2 —   ——— 3 ———   — 4 —

Textual evidence can be used to support, or disprove, certain hypotheses; and nega-
tive evidence is as important as positive evidence. From earlier observations it was
clear that there is a higher incidence of complex constituents interrupted by clitics in
Cro than in Ser, and that Sln yields no such examples. Thus with regard to the transi-
tion between stages B and C of (1), Sln has apparently proceeded all the way, Ser has
proceeded less far, and Cro is the most conservative of the three languages/dialects.
It had also been previously observed that there is a strong tendency for main verbs
to occur earlier in clauses in Sln than in S-Cr (Bennett 1986:13–14, 1987:277–78). On
the basis of these observations, Bennett hypothesized that Sln might already be closer
to the verb-clitic stage (stage D) than S-Cr and that, within S-Cr, Ser might have pro-
ceeded further than Cro (1990:1311, 2000:453–54). In the *Nineteen Eighty-Four* texts,
Table 1 indicates, for each language, what proportion of the clitics immediately pre-
ceded the main verb, what proportion immediately followed the main verb, and what
proportion were separated from the main verb. (The corresponding Pol figures are
included even though they are not relevant to the point at issue.)

    From the point of view of proximity to the verb-clitic stage, we are interested in
the combined percentages for clitics adjacent to the main verb, i.e., the sum of the first
two columns: Sln – 72%; Ser – 70%; and Cro – 73%. The differences between these per-
centages are presumably not statistically significant, and in any case do not reflect the
predicted order of Sln, Ser, Cro. The hypothesis was therefore not supported. More-
over, the textual data enable us to see why the hypothesis was ill-conceived (Bennett
2000:460). Admittedly, where clitics follow the first clause-constituent in each of Sln,
Ser and Cro, the possibility of the clitics and main verb being adjacent depends on the
possibility of the main verb occurring earlier, and is therefore higher in Sln than in
S-Cr. There is indeed no shortage of examples such as (7):

Language	Clitic(s) + MV	MV + Clitic(s)	Non-adjacent
Slovenian	61%	11%	28%
Serbian	50%	20%	30%
Croatian	41%	32%	27%
Polish	11%	82%	7%

Table 1. Proportion of clitics immediately preceding, immediately following or separated from their main verb, based on approximately 400 examples in Slovenian, Serbian and Croatian and 153 examples in Polish[6].

(7) a. O'Brien *je*   **imel** v rokah kos   papirja   [Sln]
        O'Brien aux.past had in hands piece of-paper
     b. O'Brien *je*   među   prstima **držao** komadić papira   [Cro]
        O'Brien aux.past between fingers held   scrap   of-paper
        'O'Brien had a slip of paper between his fingers'

What was overlooked was that when clitics occur later than after the first constituent of a clause, which is frequently the case in Ser and Cro, this may result in the main verbs and clitics being adjacent in S-Cr but separated in Sln. Relevant examples are provided in (8) and (9):

(8) a. Iz   neznanega razloga *je*   vedno **mislil**, da…   [Sln]
        from unknown reason aux.past always thought that
     b. Zbog nekog razloga uvijek *je*   **mislio**   da…   [Cro]
        for   some   reason always aux.past thought that
        'For some reason he had always thought that…'
(9) a. Spodaj na ulici *so*   vrtinci vetra **sukali** prah in…   [Sln]
        down   on street aux.past eddies of-wind twisted dust and
     b. Dolje na ulici, mali virovi vjetra **vrtljeli** *su*   prašinu i… [Cro]
        down on street little eddies of-wind turned aux.past dust   and
        'Down in the street little eddies of wind were whirling dust and…'

To judge from the figures in Table 1, it would seem that the two separate tendencies illustrated by (7), on the one hand, and (8) and (9), on the other, cancel each other out. The value of using corpus-based data in this case, therefore, was twofold: on the one hand it revealed that a particular hypothesis was ill-founded and on the other hand it revealed why the hypothesis was ill-founded.

Examples (8)b and (9)b, and also the Sln, Ser and Cro versions of example (6), allow us to mention the topic of deviations from a strict application of P2, which is not merely important for an understanding of the Sln and S-Cr clitic systems but will also be relevant in the discussion of Pol in section 2. Sln deviates from P2 in allowing P1 occurrences of clitics—see, for instance, the Sln version of (6), where the clitic cluster is in initial position in the main clause, following a subordinate clause. For

such an order of elements to be possible depends on the Sln clitics being prosodically neutral, i.e., able to occur either as enclitics or as proclitics (Toporišič 1976:58, 535). In S-Cr, and also in Pol, the clitics with which we are concerned are strictly enclitic. Thus clause-initial clitics are not possible in S-Cr, since this would presuppose the possibility of proclitic uses. The deviation from P2 that is encountered in S-Cr consists in the clitics occurring at P3 or even P4. Occurrence at P3 is seen in (8)b: the clause-initial constituent (*Zbog nekog razloga* 'For some reason') is complex and likely to be followed by an intonational break and/or pause. The clitic is therefore attached not to the complex first constituent but to the short second constituent (*uvijek* 'always'). In (9)b each of the first two constituents is complex, i.e., *Dolje na ulici* 'Down in the street' and *mali virovi vjetra* 'little eddies of wind'. In this case the clitic is attached to the third constituent—the participial verb *vrtljeli* 'turned'—and therefore occurs at P4 rather than P2. The Croatian version of example (6) provides a further instance of P3 placement, i.e., the clitics are attached to the short second constituent *progonio* 'persecuted' rather than the more complex first constituent *na svakom koraku* 'at every step'. A further possibility in either Ser or Cro would have been to place the participial verb *proganjao/progonio* 'persecuted' in first position in the clause and attach the clitics to this. Neither the Ser nor the Cro translator opted for this particular strategy in this instance. What the Ser translator did was insert a short word at the beginning of the clause that has no counterpart in the English original, i.e., the adverb *ipak* 'nevertheless', which reinforces the meaning of the preceding concessive clause 'although he had a good pretext for coming here'; the clitics are attached, in P2, to this extra word.

1.3 FORMAL AND FUNCTIONAL APPROACHES. Franks and King (2000) discuss clitics using the Minimalist version of formal generative grammar, and distinguish between approaches which are essentially syntactic, those which are essentially phonological, and those which combine syntax and phonology. They themselves favor a mixed approach which is mainly syntactic. As for S-Cr examples such as (10), Franks and King contrast them (2000:220) with sentences such as (11):

(10) Anina *im*   sestra nudi čokoladu   [S-Cr]
      Ana's to-them sister offers chocolate
      'Ana's sister is offering them chocolate'
(11) Anina dolazi   sestra   [S-Cr]
      Ana's   comes   sister
      'Ana's sister is coming'

They claim that in general the complex constituents in examples such as (10) can also be interrupted by non-clitic constituents, as in (11). They conclude that the mechanism involved is syntactic rather than phonological. Another demarcation issue that they discuss involves the order in which individual items occur in a clitic cluster in particular languages. They reject the approach that posits a separate morphological template for each language, on the grounds that such an approach treats the facts of

each language as arbitrary. Instead they favor a syntactic treatment, which they claim is better able to capture cross-linguistic generalizations (2000:320–23). Moreover, they claim that the same kinds of syntactic generalizations apply across languages with verb-adjacent clitics and languages with P2 clitics and use the same mechanism for ordering the individual clitics in each case. Specifically, all the clitics occur in the head position of various kinds of functional phrases: interrogative *li*, in languages that have it, occupies the highest functional head position, C^0; auxiliaries (except 3rd person *je* and the Sln future auxiliary *bom, boš, bo*, etc.) occupy the subject agreement slot ($AgrS^0$); dative clitics occupy an indirect object agreement slot ($AgrIO^0$) and accusative clitics a direct object agreement slot ($AgrO^0$); reflexive pronouns are the head of a reflexive phrase (Ref^0); and *je* and the Sln future auxiliaries occur as head of a tense phrase (T^0). In languages with verb-adjacent clitics, the clitics are base-generated in the above-mentioned positions, and the auxiliaries and pronominal clitics are regarded as pure agreement markers, or 'nonargument clitics' (2000:318). Adjacency between the verb and the clitics is achieved by the verb moving to be adjacent to the clitics. In P2 clitic languages, on the other hand, the pronominal clitics are base-generated as arguments of the verb and then move into the various agreement phrases—'for case-checking reasons' (Franks & King 2000:318)—irrespective of what happens to the verb.

As for functional treatments, Delbrück (1900:49, 51) distinguished P2 systems—which for him were only word-based rather than constituent-based—from verb-clitic systems by claiming that in the former the clitics are positioned on a 'rhythmic-musical' (i.e., phonological) basis, whereas in the latter the clitics are attached to the word to which they are most closely related semantically ('ihrem Sinne nach'). However, it is misleading to think simply in terms of phonologically vs. semantically organized systems, since phonological and semantic factors are involved in both kinds of system. The reduced phonological prominence of clitics of either type—i.e., their lack of stress and the fact that in some cases they are shorter versions of stressed counterparts—is a reflection of their reduced semantic salience. Alternatively, we should perhaps speak of their reduced informational salience: pronouns, for instance, refer to entities which are contextually or situationally 'given'; and the meaning of particles with a connective function is often predictable from the content of the clauses that they connect (Delbrück 1900:48). Clitic auxiliaries that are used to form particular tenses—and indeed tense inflections, too—are similar to pronouns in that they have an antecedent in the form of a temporal adverbial which gives more specific information about the time in question. What unites these three kinds of clitics when they occur in a single clitic cluster at P2 seems to be precisely their reduced informational and phonological prominence. In other words, the occurrence of the clitics at or near the beginning of a clause reflects their 'thematic' nature, in the Hallidayan or Prague-school sense of the term. Sln example (3b), discussed in section 1.1, was invented for the purpose of contrasting Sln and S-Cr (especially Cro) on one particular issue. The actual sentence which occurred in the Sln translation of *Nineteen Eighty-Four* was:

(12) [Če ne,] *vam bo*   slovar   izročil   moj služabnik   [Sln]
        if   not to-you aux.future dictionary hand-over my servant
      '[If not,] **my servant** will give you the dictionary'

(I have added emphasis to the constituent in the English original that would be the
obvious candidate for tonic stress in the discourse context in which the sentence
occurs.) The main clause of the Sln sentence is a classic example of a clause which
proceeds from what is maximally thematic and 'given' to what is maximally rhematic
and 'new'—or, alternatively, which exhibits a progressively increasing level of 'commu-
nicative dynamism'. What prevents the clitics from occurring in clause-initial position
in the majority of the P2-clitic languages is the fact that they are specifically enclitic
and have to 'lean on' an accented word to their left. The shift from a P2-clitic system
to a verb-clitic system can perhaps profitably be thought of as involving a change
from discourse-oriented positioning to semantically-oriented positioning. But even
in a primarily discourse-oriented system, semantic relatedness of particular clitics
to (e.g.) the verb is already a relevant factor. Thus although Proto-Slavic is assumed to
represent the P2 stage in general (Meillet 1934:481–83, Jakobson 1971:16–18), the (accu-
sative) reflexive clitic of the two oldest Slavic languages, Old Church Slavic and Old
Russian, frequently occurs immediately after the verb rather than at P2 (Ard 1975:
96–97, quoting Havránek 1963:22). Stone makes a similar point about Old Church
Slavic (1996:216), referring to Večerka (1989:47–48). Thus some clitics appear to be
affected by conflicting pressures—on the one hand, the pressure to congregate with
other informationally and phonologically non-prominent items near the beginning
of a clause and, on the other hand, the pressure to be attached to the word to which
they are most closely related semantically, normally the verb. That, within a basically
P2 system, it is specifically reflexive pronouns that are more likely to be attached to the
verb than other pronouns is related to the fact that reflexive verbs are often equivalent,
semantically, to middle voice verbs or intransitive verbs (Ard 1975:112). Change from
the one type of system to the other would depend on a gradual shift in the magni-
tude of the two pressures affecting particular clitics, though it is likely that the inter-
rogative clitic *li* and various clause-connective clitics would not be sufficiently closely
related semantically to the verb for them ever to become verb-clitics.

2. POLISH: A CASE OF CHANGE IN PROGRESS. According to Spencer (1991:367), the Pol
clitic system is 'unlike that of other Slav languages in a number of respects'. Moreover,
whereas S-Cr and Sln are P2-clitic languages and Bulgarian and Macedonian are verb-
clitic languages, in Pol 'the distribution is determined largely by phrase-level prosodic
considerations' (1991:367, interpreting the account of De Bray 1980:326–28). Among
the specific idiosyncrasies of Pol that Spencer mentions are that 'it is possible to break
up strings of clitics' and that the auxiliary verb 'clitics' are ambiguous as between verb
affixes, on the one hand, and clitics capable of being attached to any host, on the other
(1991:369–73). Franks and King also regard Pol as constituting a separate category of
language from those with 'verb-adjacent clitics' and those with 'second-position clitics'

(2000:295–305). And they, too, illustrate the possibility of individual clitics occurring separated rather than in a cluster (2000:338). As for the auxiliary clitics, Franks and King point out that they are more like inflections than clitics (2000:298), and indeed that they are changing from clitics to inflectional affixes (2000:269). The other respect in which Pol clitics have changed, according to Franks and King (2000:298, 338), is that the pronominal clitics, which used to be 'special clitics' (Zwicky 1977), i.e., both prosodically and syntactically idiosyncratic, have now become 'simple clitics', i.e., there are (allegedly) no longer any syntactic restrictions on their occurrence.

Andersen's (1987) account of the Pol auxiliary clitics/verb inflections, reporting on the work of Rittel (1975), points out that over the last five hundred years Pol clitic usage has gradually undergone a change from a Wackernagel-type P2 system toward a verb-clitic system and the present situation, where in many cases the clitics have already become verb-suffixes. At the beginning of this period the Wackernagel principle was already beginning to be relaxed, but even at the present time it is still residually in effect. The penultimate-syllable stress of Pol provides one kind of evidence whether or not particular clitics have been incorporated into the verb as affixes. Northern dialects have proceeded further in this respect than southern dialects and the standard language represents an intermediate situation. In northern dialects the addition of any of the 'auxiliary clitics' (singular or plural) effects a change in the stress pattern of the resulting formation, which suggests that they have become affixes. In some southern dialects, on the other hand, all of the clitics (singular and plural) seem to be more loosely attached to the verb stem, in that they have no effect on the stress pattern of the resulting formation. The process of absorption of the clitics into the verb as affixes has affected the singular forms before the plural forms for phonological reasons—the longer plural clitics had a greater degree of autonomy than the shorter singular forms. The standard language represents the half-way stage at which the singular clitics have been absorbed as affixes and the plural suffixes are still independent of the stem in terms of its stress pattern.

With regard to the gradual process of deviation from the Wackernagel principle over the last five hundred years, Andersen reports Rittel's (1975) findings in the following terms (1987:44):

> One type of deviation is placement after an initial phrase (rather than the first word), which safeguards the adjacency of its constituents. Another type is placement after the first word or phrase that follows an intonational caesura, which helps set off the thematic elements that precede it. Yet another is placement after a word that carries emphatic stress.

These types of deviations from strict P2 placement are specific cases of the factors that Spencer refers to generally as 'phrase level prosodic considerations' (1991:367–69). They are also reminiscent of the ways in which modern S-Cr deviates from strict P2 placement that were discussed at the end of section 1.2. (Pol is also like S-Cr insofar as its clitics are strictly enclitic.) In the following section we shall see to what extent

our textual data for Pol are compatible with Andersen's account of the stage that Pol has reached.

3. TEXTUAL DATA FOR POLISH. The first thing to note in relation to the Pol data is that all the examples of clitics can be interpreted either as P2 clitics or as verb clitics/affixes. This fact is consistent with the suggestion that present-day Pol is in the process of changing from a P2 system to a verb-clitic system (and beyond).[7] Some examples are unambiguously P2 clitics, e.g., *was* 'you (plur.)' in (13), and some can perhaps only be analyzed as verb clitics, e.g., *by* 'would' in (14):

(13)   Gdy *was* w   końcu złapią, …   [Pol]
        when you in end   will-catch
        'When they catch you in the end, …'

(14)   W   takim wypadku zmianie uległa*by*   jego twarz, …   [Pol]
        in such event   to-change succumb-would his face
        'In such an event, his face…would undergo change'

Given that the clitic in (14) is an auxiliary adjacent to the main verb, there seems little point in considering whether it could also involve one of the regularly attested deviations from P2 order. On the other hand, since it seems likely that pronominal *ich* 'them' in (15) follows the first constituent after an intonational caesura, it could perhaps reasonably be analyzed as being both a verb-clitic and a somewhat liberally defined P2 clitic. Other examples clearly illustrate simultaneous P2 and verb-clitic positioning without any need to invoke attested deviations from strict P2 placement, e.g., *was* 'you (plur.)' in (16).

(15)   W ten sam sposób rozpoznał   *ich*   w   chwili,   gdy…   [Pol]
        in that same way   he-recognized them in   moment when
        'He had identified them in that same way at the moment when…'

(16)   Zapewniam *was*, że Braterstwo   istnieje   [Pol]
        I-assure   you that Brotherhood exists
        'I assure you that the Brotherhood exists'

The 5000–6000 words of text analyzed so far have revealed no cases of split clitic clusters. However, the fact that Pol clitics occur either at P2 or adjacent to the verb predicts such a possibility, and further analysis may reveal relevant examples. Alternatively, such structures may be generally absent from the personal style of the translator of *Nineteen Eighty-Four*.

Two further things are of interest in relation to the Pol data. First, P2 placement of Pol clitics is more common in subordinate clauses than in main clauses. This is consistent with the observation that, in all cases of word order change in progress, subordinate clauses exhibit a more conservative word order than main clauses. For Pol, Andersen (1987:29, 35) reports the findings of Rittel (1975), that main clauses have

been changing ahead of subordinate clauses over the whole period during which the shift from P2 positioning to verb-clitic positioning has been taking place. With regard to the conditional formative *by* 'would', Decaux observes that in main clauses it tends to follow the *-l* participle but that in a subordinate clause introduced by a conjunction it is obligatorily attached to the conjunction (1955:24). This, too, is borne out by our data; (14) can be contrasted, in this connection, with (17).

(17) jak*by*   *go*   ktoś   zdzielił   pałką gumową po ciemieniu   [Pol]
    as-would him someone imparted stick rubber   on temple
    'as if someone imparted him [a blow] with a rubber truncheon'

(18) Jeśli*by*   coś   powiedział   [Pol]
    if-would something he-said
    'If he should say something'

It may be that in cases like (17) two separate factors are at work: on the one hand, the position after the conjunction involves the original Wackernagel positioning, which persists longer in subordinate clauses; and, on the other hand, the conjunction—rather than the verb—may be precisely the constituent of the clause to which the conditional formative bears the closest semantic relationship. This certainly seems plausible in an example such as (18), where there is an obvious semantic relationship between the conjunction *jeśli* 'if' and the conditional meaning of *by* 'would'.

Finally, and more significantly, there are restrictions on the positions in which individual clitics may occur. Thus, with regard to auxiliary clitics, although most discussions of Pol illustrate one or more possibilities of the more conservative Wackernagel positioning—e.g., (19)b–d—in addition to the verb-clitic positioning (19)a, the Polish translator of *Nineteen Eighty-Four* attached auxiliary clitics exclusively to the verb[8].

(19) a. Ciekawą   książkę   kupiła-*ś*   Janowi   [Pol]
      interesting   book   bought-2sg. for-John
      'You bought an interesting book for John'

    b. Ciekawą-*ś*   książkę   kupiła   Janowi

    c. Ciekawą   książkę-*ś* kupiła   Janowi

    d. Ciekawą   książkę   Janowi-*ś*   kupiła

Also, as might be expected in the light of our earlier discussion, the reflexive pronoun *się* occurs only as a verb clitic. As for other pronominal clitics, the accusative forms were attested in either position, but all of the dative clitics occurred exclusively as verb clitics. Other discussions of Pol clitics certainly illustrate P2 occurrences of dative clitics, e.g., Spencer (1991:369). It would seem, then, that we may again be dealing here with the personal style of the translator of *Nineteen Eighty-Four*. Nevertheless, it is possible that there is a general tendency in the language for dative clitics to gravitate to the verb ahead of accusative clitics.

4. FORMALIZING THE CHANGE FROM P2 CLITICS TO VERB-CLITICS. Franks and King present the following 'diachronic scenario' for the change from the P2 system of 'Older Bulgarian' to the verb-clitic system of the present-day language (2000:318). Slavic NPs were, and in most cases still are, embedded in a KP (case phrase), but in Bulgarian (and also Macedonian) the loss of case and the introduction of articles have resulted in the KP giving way to a DP (determiner phrase). Moreover, the K^0 morpheme, 'instead of moving to Agr (as an argument for case-checking reasons)… was reanalyzed as being base-generated in Agr'. This scenario was not invoked by Franks and King in relation to the changes going on in Polish, since (as pointed out in section 2) they do not see these changes as involving an ongoing shift from P2 clitics to verb-clitics. However, on the basis of the work of Rittel (1975) and Andersen (1987), as reported in section 2, and my own textual analysis of section 3, I have argued that Polish has been undergoing just such a change, coupled with a further change of verb-clitics to verb-inflections. It is relevant, therefore, to ask to what extent the same 'diachronic scenario' might fit the facts of the Polish case; and the answer is: not at all well, and for two reasons. First, Polish has not lost the category of case or developed articles. Thus the development of verb-clitics by no means necessarily correlates with the rise of articles. Secondly, the term 'reanalysis' implies that word order change as such was not involved in the Bulgarian case. As for Polish, the phenomenon of split clitic clusters and the main body of our textual data, which indicates that particular clitics favor the position adjacent to the verb whereas others still regularly occur at P2, strongly suggest not a reanalysis but an ongoing word-order change, with certain clitics being attracted to the newer position ahead of others.

In any case, the Minimalist framework is by no means the only framework in which to formalize the change with which we have been concerned. In recent years there have been increasing numbers of applications of Optimality Theory (OT) to the topic of clitics, e.g., Anderson (2000), Legendre (2000) and Vincent (2001). Moreover, in sections 1.3 and 3 it was suggested that Polish clitics have been subjected to two different pressures—on the one hand, the pressure to occur early in the clause for reasons of discourse structure   and, on the other hand, the pressure to occur adjacent to the constituent to which they are most closely related semantically. There is thus competition between the different positions in which the clitics may occur, and it was suggested that over time the strength of the two pressures has changed. A further promising framework to explore, therefore, is that of network grammars of the stratificational, and now 'neurocognitive', variety (Lamb 1999), since the notion of competition has occupied a prominent place within spreading activation implementations of these grammars for more than twenty years, e.g., Dell & Reich (1980), Lamb (1999:218–21, 233–36)[9].

5. CONCLUSION. This paper began with the suggestion that clitics, by their nature, need to be examined diachronically; and chose to focus on Polish, within Slavic, since it represents a particularly interesting case of change-in-progress. The textual data that were used in the paper provided evidence for various sorts of conclusions. The Sln, Ser and

Cro data demonstrated, first, that none of these three languages/dialects is any closer to reaching the verb-clitic stage than the other two and provided an explanation why an earlier hypothesis on this issue was ill-founded. At the same time, the data allowed us to see in what ways Sln, Ser and Cro clitics deviate from a strict application of P2 placement; and the deviation facts for Ser and Cro, in turn, mirrored the observations of Rittel (1975) and Andersen (1987) concerning the gradual loss of P2 placement by Polish. The Polish data supported the view that the clitics of this language are still in the process of changing from P2 to verb-clitic placement, and that the development in question has involved a word-order change rather than a structural reanalysis. The paper also argued that adequate attention needs to be paid to both formal and functional considerations. From a functional point of view, it was suggested that the change that Polish has been undergoing can best be understood as a gradual shift from discourse-oriented positioning of its clitics to semantically-oriented positioning, with individual clitics differentially affected by the two separate pressures at any given time and the change from one type of system to the other depending on a gradual shift in the magnitudes of the two pressures. Finally, doubts were raised over the applicability of a particular Minimalist formalization of related diachronic phenomena.

---

[1]   This work has been supported by grants from the British Academy and the School of Oriental and African Studies, University of London, which are gratefully acknowledged. I am also pleased to acknowledge the help of Harry Leeming, co-author of Bennett and Leeming (1996), and Monica Leeming, in relation to the discussion of Polish; and that of Simona Bennett in relation to Slovenian and Serbo-Croatian examples. Finally, I am grateful for comments on an earlier version of the paper by Thea Bynon and two anonymous reviewers.

[2]   The clitics with which we shall be concerned correspond to independent words from a number of syntactic categories, such as pronouns, auxiliary verbs, conjunctions and modal particles. They are specifically enclitics, though in, e.g., Slovenian and Macedonian they may occur also as proclitics. The paper is not concerned with such specifically proclitic items as short unstressed prepositions and the marker of negation.

[3]   Examples (2)a and (2)b are taken from the Croatian (Cro) and Serbian (Ser) translations, respectively, of George Orwell's novel *Nineteen Eighty-Four*. Interruption of a clause-initial complex constituent can be encountered also in Ser but is less common than in Cro.

[4]   The preposition *v* 'in' in this example is proclitic to *этой* (*ètoj*) 'this'. Thus the first accented word in the embedded clause is *этой* (*ètoj*).

[5]   Between 5000 and 6000 words of the four Slavic texts were analysed. Roughly half of the chosen material consists of predominantly past tense narrative, while the remainder represents predominantly present tense dialogue.

[6]   The reason the Pol text has so few clitics, by comparison with the Sln, Ser and Cro texts, is that the 3rd person forms of the auxiliary (both singular and plural) are null.

[7]   This observation contrasts with Franks and King's (2000:298, 338) suggestion, reported in section 2, that the Pol clitics have become simple clitics capable of occurring in any syntactic position.

[8]   Example (19) is adapted from Franks and King (2000:158).

9   Limitations of space in the present paper prevent me from comparing the three above
     formal theories—Minimalism, OT and neurocognitive networks—by applying them to
     the same diachronic data. However, I hope to remedy this situation in the near future in
     a sequel to this paper.

REFERENCES

ANDERSEN, HENNING. 1987. From auxiliary to desinence. In *Historical development of auxiliaries*, ed. by Martin Harris & Paolo Ramat, 21–51. Berlin: Mouton de Gruyter.

ANDERSON, STEPHEN R. 2000. Towards an optimal account of second-position phenomena. In Dekkers et al. 2000, 302–33. Oxford: Oxford University Press.

ARD, WILLIAM J. 1975. *Raisings and word order in diachronic syntax*. UCLA Ph.D. dissertation.

BENACCHIO, ROSANNA & LORENZO RENZI. 1987. *Clitici slavi e romanzi*. Quaderni Patavini di Linguistica, Monograph 1, Padova: University of Padova Department of Linguistics.

BENNETT, DAVID C. 1986. Towards an explanation of word-order differences between Slovene and Serbo-Croat. *Slavonic and East European review* 64:1–24.

———. 1987. Word order change in progress: the case of Slovene and Serbo-Croat and its relevance for Germanic. *Journal of linguistics* 23:269–87.

———. 1990. On the progress of clitics: evidence from Slovene and Serbo-Croat. In *Proceedings of the Fourteenth International Congress of Linguists*, ed. by Werner Bahner, Joachim Schildt & Dieter Viehweger, 1309–12. Berlin: Akademie-Verlag.

———. 2000. Investigating syntactic change through synchronic textual comparison: a case study. In *Functional approaches to language, culture and cognition: papers in honor of Sydney M. Lamb*, ed. by David G. Lockwood, Peter H. Fries, & James E. Copeland 445–63. Amsterdam: John Benjamins.

——— & HARRY LEEMING. 1996. The evolution of clitic systems: textual evidence from the Slavonic languages. *SOAS working papers in linguistics & phonetics* 6: 340–70.

BERNEKER, ERICH. 1900. *Die Wortfolge in den slavischen Sprachen*. Berlin: Behr.

BROWNE, WAYLES E. 1975. Serbo-Croatian enclitics for English-speaking learners. In *Contrastive analysis of English and Serbo-Croatian*, ed. by Rudolf Filipović, 105–34. Zagreb: Institute of Linguistics, University of Zagreb.

DE BRAY, REGINALD G.A. 1980. *Guide to the Slavonic languages*, part 2: *West Slavonic* (3rd ed.). Columbus, Ohio: Slavica.

DECAUX, ÉTIENNE. 1955. *Morphologie des enclitiques polonais*. Paris: Institut d'Études Slaves.

DEKKERS, JOOST, FRANK VAN DER LEEUW & JEROEN VAN DE WEIJER (eds.). 2000. *Optimality theory: phonology, syntax, and acquisition*. Oxford: Oxford University Press.

DELBRÜCK, BERTHOLD. 1900. *Vergleichende Syntax der indogermanischen Sprachen*, part 3, vol. 5 of Karl Brugmann *&* Berthold Delbrück, *Grundriss der vergleichenden Grammatik der indogermanischen Sprachen*. Strassburg: Trübner.

DELL, GARY S. *&* PETER A. REICH. 1980. Toward a unified model of slips of the tongue. In *Errors in linguistic performance: slips of the tongue, ear, pen, and hand*, ed. by Victoria Fromkin, 273–86. New York: Academic Press.

FRANKS, STEVEN *&* TRACY HOLLOWAY KING. 2000. *A handbook of Slavic clitics*. Oxford: Oxford University Press.

HAAS, MARY R. 1977. From auxiliary verb phrase to inflectional suffix. In Li 1977, 525–37.

HAVRÁNEK, BOHUSLAV. 1963. Zalog (genera verbi) v staroslavjanskom jazyke v sravniteľnom plane. In Bohuslav Havránek, *Issledovanija po sintaksisu staroslavjanskogo jazyka*. Prague.

JAKOBSON, ROMAN. 1971. Les enclitiques slaves. In *Selected writings*. vol. 2, *word and language*, 16–22. The Hague: Mouton.

LAMB, SYDNEY M. 1999. *Pathways of the brain: the neurocognitive basis of language*. Amsterdam: John Benjamins.

LEGENDRE, GÉRALDINE. 2000. Morphological and prosodic alignment of Bulgarian clitics. In Dekkers et al. 2000, 423–62. Oxford: Oxford University Press.

LI, CHARLES N. 1977. *Mechanisms of syntactic change.* Austin: University of Texas Press.

MEILLET, ANDRÉ. 1934. *Le Slave commun*. Paris: Librairie Ancienne Honoré Champion.

RITTEL, TEODOZJA. 1975. *Szyk członów w obrębie form czasu przeszłego in trybu przypuszczającego*. Wrocław: Ossolineum.

SPENCER, ANDREW. 1991. *Morphological theory*. Oxford: Blackwell.

STEELE, SUSAN. 1977. Clisis and diachrony. In Li 1977, 539-79.

STONE, GERALD. 1996. Word order in the Freising texts. In *Zbornik Brižinski spomeniki*, ed. by Janko Kos, Franc Jakopin, Jože Faganel, 213–24. Ljubljana: Slovenska akademija znanosti in umetnosti.

TOPORIŠIČ, JOŽE. 1976. *Slovenska slovnica*. Maribor: Založba Obzorja.

TRÁVNÍČEK, FRANTIŠEK. 1956. *Historická mluvnice česká*, vol. 3: *Skladba*. Prague: Státní Pedagogické Nakl.

VEČERKA, RADOSLAV. 1989. *Altkirchenslavische (altbulgarische) Syntax*, vol. 1, *die linearische Satzorganisation*. Freiburg i. Br.: U.W. Weiher.

VINCENT, NIGEL. 2001. The evolution of Romance clitics: an Optimality Theory approach. Paper presented at a Diachronic Syntax Workshop, School of Oriental and African Studies, University of London, 29 January 2001.

WACKERNAGEL, JOSEF. 1892. Über ein Gesetz der indogermanischen Wortstellung. *Indogermanische Forschungen* 1:333–434.

ZWICKY, ARNOLD. 1977. *On clitics*. Bloomington: Indiana University Linguistics Club.

THE MORE THINGS CHANGE:
THE LIMITS OF SOLUTIONS TO THE ACTUATION PROBLEM

DAVID BOWIE
Brigham Young University

AT THEIR CORE, historical linguistics and much of sociolinguistics study the same phenomenon: language change over time. Because of this focus, both of these subfields continually come up against the 'actuation problem', or the question of what exactly causes any particular linguistic change at any particular time in any particular speech community. Although this is an important question for studies of language change, there is some question as to whether it can be solved in the sense of developing what might be called principles of actuation[1].

Unfortunately, in attempts to solve the actuation problem, some of the work that has been done is based on the perpetuation of conventional wisdom rather than rigorous original scholarship. For example, the claim that Latinate usages in English texts as late as the fourteenth century was the result of French influence left over from the Norman Conquest has been widely repeated in the academic literature, but this has been shown to be false, since the Norman population was fairly quickly assimilated linguistically (Clark 1992). There are, however, a few cases in which the actuation question has been solved for particular changes in particular speech communities, and the solutions hold up under close scrutiny. The two this paper looks at most directly are the reduction of monophthongal /ay/ in urban Texas (Thomas 1997) and the merger of /ɑ/ and /ɔ/ in eastern Pennsylvania (Herold 1997). These are particularly interesting, because both of these solutions to the actuation problem have found the same underlying cause: large-scale immigration creating a dialect leveling situation. Given that solutions to the actuation problem have been found in these cases, and the solutions are so similar, a possibly more important question has emerged: can solutions to the actuation problem for a particular change in a particular speech community be extended to similar changes in other speech communities? This paper seeks to answer this question.

1. SOLUTIONS TO THE ACTUATION PROBLEM. To set the stage I need to briefly review the two solutions to the actuation problem I referred to earlier. First I will sketch out the process by which /ɑ/ and /ɔ/ merged in eastern Pennsylvania, and then I will discuss the reduction in rates of monophthongal /ay/ in urban Texas.

1.1. THE MERGER OF /ɑ/ AND /ɔ/ IN EASTERN PENNSYLVANIA. Herold (1997) noted that certain communities in eastern Pennsylvania show a tendency toward the *cot-caught* merger, and the evidence shows that the merger developed in eastern Pennsylvania

independently of contact from other varieties where the merger exists. Also, perhaps more interestingly, the status of the merger is different in different cities, depending on whether they are historically coal-mining communities (generally merged) or not (generally unmerged); of the twelve coal-mining communities studied, ten show the merger, and all eight non-mining communities do not exhibit the merger (Herold 1997: Figure 7). This difference between mining and other communities points to the possibility of some sort of demographic or social difference between them. After studying the various ways in which the mining and non-mining communities differ, Herold found a significant difference in early twentieth century immigration—the mining towns attracted a significantly larger number of Lithuanian and Polish immigrants. From this, Herold concluded that the merger of /ɑ/ and /ɔ/ in the mining towns of eastern Pennsylvania was caused by contact with a massive influx of speakers who, because their first language had no such distinction in the oral vowels, put pressure on the rest of the speech community to abandon the distinctions. On the other hand, the number of speakers without that distinction who came to the non-mining towns was much smaller. Therefore, they did not place as much pressure on the local varieties there, and the distinction survived[2].

1.2. MONOPHTHONGAL /ay/ IN URBAN TEXAS. Thomas' study of /ay/ in Texas (1997) deals with a somewhat different sort of sound change—the reversal of a trend. Thomas found a shift away from monophthongal /ay/ among native Texan Anglos, but found that this trend was limited to large urban areas, with rural and smaller urban areas continuing to favor the monophthongal form[3]. Once again, this sort of result raises a clear question: Is there a social or demographic difference between residents of very large cities and others? Thomas investigated further and found that there is, indeed, a difference—residents of very large urban areas were surrounded by a larger proportion of relatively recent immigrants to the state of Texas. Specifically, 19.1% of residents of very large metro areas at the time of the study had lived there ten years or less, compared with only 14.8% and 10.5% of residents of smaller metro and town/rural areas, respectively (Thomas 1997: Table 4). As recent migration to Sunbelt states like Texas has come largely from non-Southern areas of the United States, Thomas concluded that there was enough dialect contact between Texans in large urban areas and non-Texans without monophthongal /ay/ to put pressure on the varieties used in the local speech communities.

1.3. THE COMMON THREAD. A common thread links Herold's (1997) and Thomas' (1997) studies—in each case, the cause of the sound change under investigation was found to result from large-scale immigration putting pressure on local varieties to bring about a change. These are fairly solid solutions to the actuation problem, and one might ask whether this could be a general principle of actuation. That is, if large-scale immigration and the resulting dialect contact can cause linguistic changes, can we say that all (or at least most) changes are caused by immigration and dialect contact? Along the same lines, can solutions to the actuation problem for particular changes in particular

speech communities like the ones described above be extended to similar changes in different speech communities?

2. PARALLEL STUDIES. To determine whether it is possible to say that we can develop principles of actuation from the observations described above, I present two cases of changes that are at least roughly parallel: the merger of /ɑ/ and /ɔ/ in Utah (Di Paolo 1992; Bowie et al. 2001) and monophthongal /ay/ in Southern Maryland (Bowie 2001a). These changes will be compared directly with their equivalents, which will give insight into whether we can claim that the causes are the same in each case.

2.1. THE MERGER OF /ɑ/ AND /ɔ/ IN UTAH. That /ɑ/ and /ɔ/ are merged in Utah (along with most of the United States West) is widely enough known that the fact rarely gets more than a passing mention, if that, by most descriptions of the local varieties (Labov et al. in press). However, studies have been conducted that deal with the status of this merger in some depth both in modern times (Di Paolo 1992) as well as the nineteenth century (Bowie et al. 2001). Preliminary results from the Early Utah English Project on the development of Utah English during the first half-century of its existence (1847–'96), looked at this merger and found that it first appears appreciably among individuals born in the 1870s. To this point, there seems to be a parallel with the context for the *cot-caught* merger in eastern Pennsylvania as described by Herold (1997), particularly since there was quite a bit of immigration to the Utah Territory around that time. The 1880 census shows 30.54% of Utah's population as born outside of the United States. The parallel breaks down fairly quickly after that, though, as the nature of the immigration was quite different. A review of place of birth as reported in the 1880 census shows that less than 8% of the residents of the Utah Territory were born in countries with widely spoken languages that lack something analogous to the /ɑ/-/ɔ/ distinction. Further, less than 13% of Utah residents were born in countries in which languages other than English were spoken at the time. This is a very small percentage of total immigrants when compared to the proportion of Polish and Lithuanian immigrants that Herold found in the cities with the *cot-caught* merger in eastern Pennsylvania, particularly when one considers that that there were other groups of immigrants to eastern Pennsylvania who could also have placed pressure on the local variety to simplify its vowel system. Although one would expect that the coming together of speakers of a variety of dialects of English must have given rise to a dialect leveling situation, dialect leveling situations seem to function differently from language contact situations (compare, for example, the conclusions of Kerswill & Williams 2000 and Thomason & Kaufman 1988). Given this, it is clear that even though the situations in eastern Pennsylvania and Utah led to the same merger, the actual mechanics that would have resulted from the different situations must have differed[4]. This means that if we see the same linguistic change occurring in two different speech communities, we cannot automatically assume that the causes are the same. This is perhaps only common sense, but it is still necessary evidence regarding the possible extension of solutions to the actuation problem.
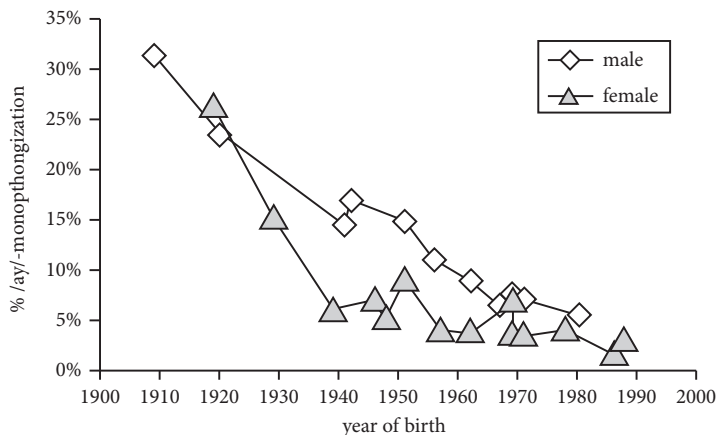
Figure 1. /ay/-monophthongization among Waldorfians by year of birth (Bowie 2001a, Figure 3).

2.2. MONOPHTHONGAL /ay/ IN SOUTHERN MARYLAND. A comparison of the regression of monophthongal /ay/ in Southern Maryland (Bowie 2001a) and in urban Texas (Thomas 1997) sheds light on different, and perhaps more important, issues than does the low back vowel merger discussed above. Waldorf, a town in Southern Maryland, has experienced a reduction in rates of /ay/-monophthongization during the twentieth century similar to that reported in urban Texas. In Waldorf, the abandonment of monophthongal /ay/ appears to be a change in progress, with women leading the change; a graph showing the decline of /ay/-monophthongization in apparent time among twenty-five white middle-class Waldorfians (fourteen female, eleven male) is shown in Figure 1. The decrease over apparent time provides a good fit to an exponential curve, with $R^2=0.7734$ for the women and $R^2=0.9593$ for the men. Multivariate analysis confirms that this apparent time effect exists and also that it has an extremely strong effect. In addition, it is important to note that Waldorf's population increased faster than national and state rates for population increase for much of the twentieth century, as shown in Figure 2[5].

At first glance, it might seem that the same process has occurred in Waldorf as in urban Texas—Waldorf has also experienced massive immigration (primarily from elsewhere in the United States), necessarily leading to a high level of dialect contact. It could seem then, that the reduction in the rate of /ay/-monophthongization over time in Waldorf is due to this high level of dialect contact, just as it was in urban Texas. A closer look at the demographic situation in Waldorf as it relates to this change, however, shows that this cannot be the case. The population figures in Figure 2 show that Waldorf's population did not begin to climb until the 1940s, and the local population did not begin to climb sharply until the 1950s. This stands in contrast, however, to the rates of /ay/-monophthongization shown in Figure 1, which show a sharp drop in /ay/-monophthongization rates well before the local population
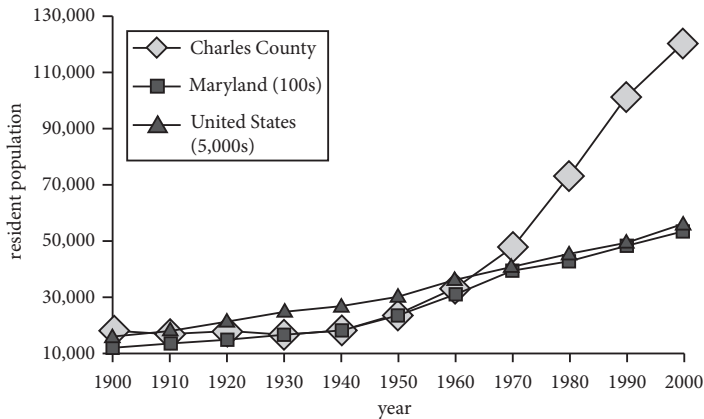
Figure 2. United States, Maryland, and Charles County population, 1900–2000 (Bowie 2001a, Figure 2).
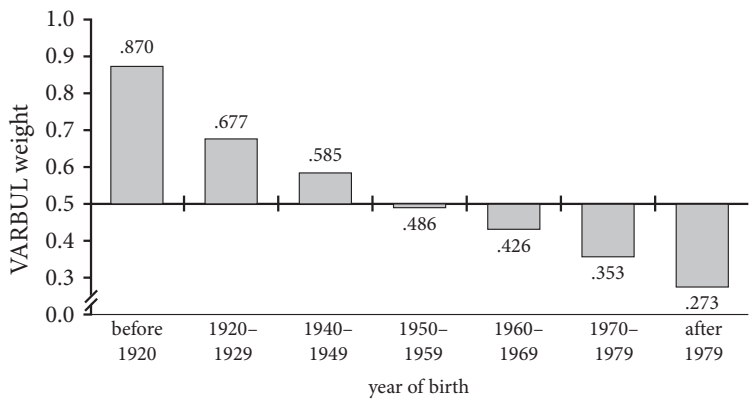
Figure 3. VARBRUL *weights by year of birth (Bowie 2001a, Figure 7).*

began to climb. Further evidence supporting this observation is shown in Figure 3, which shows VARBRUL weights by year of birth from a larger analysis of this change. This factor—which is highly significant, as evidenced by the large distance between the factors most and least favoring /ay/-monophthongization—shows the oldest age group strongly favoring monophthongal /ay/, the second-oldest age group favoring it less strongly, and so on to the youngest age group, which fairly strongly disfavors it.

As can be seen from the VARBRUL weights in Figure 3, /ay/-monophthongization was declining well before the middle of the twentieth century. Although the group of individuals born between 1940 and 1949 favor /ay/-monophthongization, they do so less than those born between 1920 and 1939. Similarly, those born between 1920 and 1939 favor /ay/-monophthongization quite strongly, but less than those born before 1920. As can be seen from Figure 2, though, large-scale migration into the Waldorf area did not start until the 1940s. As a result, an argument based on immigration

cannot explain the beginning of the move away from monophthongal /ay/ in the Waldorf speech community. It should be noted, though, that by the end of the twentieth century Waldorfians certainly had more contact with speakers of other varieties than they did just after World War II. This came about not just because of migration into the area, but also because increasing numbers of Waldorfians began commuting to Washington D.C. for work in the intervening years (Edelen et al. 1976). This dialect contact may have accelerated the trend toward diphthongal /ay/, or it may have caused the variation to tend toward a lower level than it might have otherwise, even though it does not appear possible to name it as the cause of the trend[6].

3. CONCLUSIONS AND DISCUSSION. As mentioned at the beginning of this paper, standing in the background in nearly all studies of language change is the actuation question: why did this change ever start? Occasionally this question can be answered for a particular change in a particular speech community; given this, it seems only reasonable to ask: can such solutions for one speech community safely be extended to similar changes in other speech communities?

This paper has compared parallel sound changes in different speech communities and, from the evidence presented, it seems clear that the simple answer to that question is 'no'. A closer review, however, shows that there is more to the question than that simple answer can provide.

Eastern Pennsylvania (Herold 1997) and Utah English (Bowie et al. 2001) have both experienced the same sound change (the merger of /ɑ/ and /ɔ/), and urban Texas (Thomas 1997) and Southern Maryland (Bowie 2001a) also share a common sound change (a trend away from monophthongal /ay/). On the surface, each of these pairs appears to have gone through similar demographic changes—specifically, they have all experienced large-scale immigration—but upon probing deeper, one finds that the specifics of the demographic changes are different. This result is problematic for anyone attempting to build a theoretical framework accounting for causes of linguistic change, as it means that what had appeared to be a promising possibility for explaining actuation cannot result in a predictive theory. However, the goal of studies of language change and actuation should, presumably, lead to the development of a truly predictive and explanatory theoretical framework.

Recent steps toward general principles of this sort have been made in connection with studies of dialect leveling (Kerswill & Williams 2000), vowel shifts (Labov 1994), and to some extent variation in language perception and production (Bowie 2001b; Niedzielski 2000). However, in general, the move toward a predictive and explanatory theory of actuation remains as described by McMahon (1994): Theoretical frameworks explaining the progress of language changes exist, but a theoretical framework for actuation—the beginning of language change—remains as yet out of reach.

Given this, what should we be looking for so that we can develop a theory of actuation? Obviously, more work along the lines of that done by Herold (1997) and Thomas (1997), studying the actuation of particular changes in particular speech communities, is necessary—such data is required if we are to test the theory we eventually develop.

However, the crucial thing is to look for new patterns in the results that come out of such work. The pattern of Herold's and Thomas's studies — as well as studies such as Boberg and Strassel's work on short-*a* in Cincinnati (2000)—had shown a possible pattern of certain types of migration as triggers for language change. The comparison with the facts from the other studies given in this paper shows that that cannot be the sole possible trigger for language change. We likely have to move toward a more general (if not abstract) set of principles of actuation, along the lines of Kerswill and Williams' (2000) principles of dialect leveling or Labov's (1994, 2001) principles of linguistic change. By focusing on general patterns rather than the specifics of individual situations, we can eventually bring sociolinguistics and historical linguistics to a point where a theory of actuation is within our grasp[7].

---

[1]   Several have noted the importance of this issue. To give just one example, McMahon (1994:252) reviewed some of the work that has been done on language variation and its relationship to the actuation problem and noted the importance of the issue but stated that a general solution 'remains almost as mysterious and unattainable as ever'.

[2]   Herold's (1997) conclusions on why the Polish and Lithuanian immigrants had this effect are somewhat tentative and incomplete. Note, though, that for present purposes it is unimportant exactly *why* the Polish and Lithuanian speakers had this effect on the local variety; what is important is that they triggered a particular pattern of language contact that resulted in the effect.

[3]   A VARBRUL analysis gave weights of .60 for very large metro areas, .39 for metro areas and .43 for towns and rural areas, where higher weights reflect a favoring of the diphthongal form (Thomas 1997:Table 2).

[4]   Of course, small differences in demography can have disproportionate effects on the local speech community. As Mufwene (1996) points out, there is presumably a tipping point at which the linguistic impact of immigrants is great enough to seriously affect a local variety, but the exact level at which the tipping point is reached is not known. If one speech community faces immigration slightly above the tipping point and another slightly below it, the small difference in immigration levels may be reflected by a change in the first community's variety and little to no change in the second community.

[5]   Waldorf is an unincorporated municipality for which population counts before 1990 are not readily available, so census figures for Charles County are used in Figure 2. From historians' discussions of demographic changes in Southern Maryland, this should be adequate for present purposes (Potyraj 1994; Edelen et al. 1976).

[6]   It has been pointed out to me that other factors may be at work in Waldorf, such as a possible perception of 'urbanness' versus 'ruralness' or the like in regard to this change. This is a possibility, and certainly needs to be looked at more closely, but for present purposes it suffices to point out that the relationship between demographic and linguistic changes in Waldorf does not fit the same pattern as in urban Texas.

[7]   An appendix of sorts: this paper has looked at parallel sound changes and whether they can be traced to similar demographic causes. A probably more interesting question is whether parallel demographic changes result in similar linguistic changes. I suspect that the answer to that question would once again be no, but there is as yet no evidence for or against that suspicion and it remains an important and open question.

REFERENCES

BOBERG, CHARLES & STEPHANIE M. STRASSEL. 2000. Short-*a* in Cincinnati: A change in progress. *Journal of English linguistics* 28:108–26.

BOWIE, DAVID. 2001a. The diphthongization of /ay/: Abandoning a Southern norm in Southern Maryland. *Journal of English linguistics* 29:329–45.

——. 2001b. Dialect contact and dialect change: The effect of near-mergers. *Selected papers from NWAV 29*, ed. by Tara Sanchez & Daniel Ezra Johnson, 17–26. Philadelphia: University of Pennsylvania.

——, WENDY MORKEL & ELLEN LUND. 2001. Early trends in Utah English. Paper read at NWAV 30, 12 October 2001, Raleigh NC.

CLARK, CECILY. 1992. The myth of 'the Anglo-Norman Scribe.' *History of Englishes: New methods and interpretations in historical linguistics*, ed. by Matti Rissanen et al., 117–29. Berlin: Mouton de Gruyter.

DI PAOLO, MARIANNA. 1992. Hypercorrection in response to the apparent merger of (ɔ) and (ɑ) in Utah English. *Language and communication* 12:267–92.

EDELEN, VIVIAN B., JANIE MACINNIS, MOIRA MAHONEY & GRETCHEN WILLIAMS. 1976. Towns and villages: Changes with transportation. *Charles County, Maryland: A history: Bicentennial edition*, ed. by Jack D. Brown et al., 53–77. South Hackensack NJ: Custombook.

HEROLD, RUTH. 1997. Solving the actuation problem: Merger and immigration in Eastern Pennsylvania. *Language variation and change* 9:165–89.

KERSWILL, PAUL & ANN WILLIAMS. 2000. Creating a new town koine: Children and language change in Milton Keynes. *Language in society* 29:65–115.

LABOV, WILLIAM. 1994. *Principles of linguistic change, volume 1: Internal factors*. Oxford: Blackwell Publishers.

——. 2001. *Principles of linguistic change, volume 2: Social factors*. Oxford: Blackwell Publishers.

——, SHARON ASH & CHARLES BOBERG. In press. *Atlas of North American English*. Berlin: Mouton de Gruyter.

MCMAHON, APRIL M.S. 1994. *Understanding language change*. Cambridge, England: Cambridge University Press.

MUFWENE, SALIKOKO S. 1996. The founder principle in creole genesis. *Diachronica* 13:83–134.

NIEDZIELSKI, NANCY. 2000. Chipping away at the perception/production interface. Presented at NWAV 29, 6 October 2000, East Lansing, Michigan.

POTYRAJ, JOHN E. 1994. *Common bond: A Steeple Press book commemorating the bicentennial anniversary of St. Mary's Catholic Church, Bryantown, Maryland*. Norfolk MA: Steeple Press.

THOMAS, ERIK R. 1997. A rural/metropolitan split in the speech of Texas Anglos. *Language variation and change* 9:309–32.

THOMASON, SARAH GREY & TERRENCE KAUFMAN. 1988. *Language contact, creolization, and genetic linguistics*. Berkeley: University of California Press.

EVIDENCE FOR THE IMPERATIVE AS A SPEECH-ACT CATEGORY

Inga B. Dolinina
McMaster University

IN THIS PAPER I ARGUE that the Imperative should be regarded as a category of speech act rather than a verbal category. I elaborate a new theoretical framework for this categorization, and show that the new framework fits the evidence better than the traditional treatment of the Imperative as a verbal category. A crucial part of my argument is the legitimacy of so-called 'mixed paradigms' which include all person-values and in which the Imperative can be marked in some persons synthetically and in other persons periphrastically. I show that my argument rests on both primary linguistic evidence and theoretical assumptions.

1. CONTROVERSIES ABOUT THE IMPERATIVE. There are many controversies about the imperative. Is it a verbal category or a speech act category? That is, is it a Mood clustering with indicative, subjunctive, etc., or a phenomenon of another type clustering with declaratives and interrogatives? How are we to describe its grammatical semantics, as a meaning component inside a proposition or one outside it? Is the imperative meaning a semantic primitive, or a complex of components? What constructions can be recognized as Imperative? What is the scope of the Imperative paradigm? Can it include only synthetic forms like (1) or also periphrastic forms like (3), (4), and (5)? If it is restricted to synthetic forms, can it include only second-person forms or also first- and third-person forms?

There is no consensus among contemporary linguists on any of these questions. As a result, descriptive grammars define the imperative paradigm for a language differently. Some grammars include only specialized second-person constructions (Mithun 1999, Švedova 1980, Wierzbicka 1995). Others (e.g., those for Greek, Cree, Church Slavonic, Celtic languages) include all synthetic forms, irrespective their person value (Hopper & Traugott 1993, Jespersen 1992/1924, etc.). Still others include values for all persons and allow as members both synthetic and periphrastic constructions; such paradigms are called 'mixed' paradigms (Khrakovskij 1992).

2. PECULIARITIES OF MIXED PARADIGMS. Mixed paradigms are heterogeneous. They are expected to include, in the first place, all synthetic constructions represented by specialized 'imperative forms' of the verb (with or without pronouns), which can refer to any person value, as in (1).

(1) a. English   *Go!* (2SG/PL)
     b. Russian   *Idi!* (2SG); *Idite!* (2PL)

(1) c. German   *Gehe!* (2SG); *Geht!* (2PL)
     d. Spanish   *Ve!* (2SG); *Vayan!* (2PL)
     e. French   *Va!* (2SG), *Allez!* (2PL), *Allons nous!* (1PL)
     f. Scots Gaelic *Seas!* (2SG), *Seasaibh!* (2PL), *Seasamaid!* (1PL),
                       Seasadh esan! (3SG), for 'stand up' (Mcaulay 1992:165)

These paradigms also include diverse non-synthetic, periphrastic constructions. Some periphrastic constructions in mixed imperative paradigms have causative or modal verbs, as in (2).

(2) a. English   **Let's** go!
     b. German   **Laß** uns gehen!
     c. German   **Wollen** wir gehen!

Others use forms primarily associated with categorial meaning other than imperative, e.g., Subjunctive, Future or Present Indicative, which are often combined with a subordinator, as in (3).

(3) a. French   **Qu'**ils **aillent!** (3PL)   'Let them go!'
     b. Spanish   **Que** nostros **vayamos!** (1PL)   'Let us go!'

Others use delexicalized verbs or auxiliaries, some of which are reduced to particles, as in (4).

(4)   Russian   **Davaj/**te (2SG/PL) pojdem (1PL)!   'Let us go!'
                       Pust' on idet! (3SG)   'Let him go!'

In contrast to standard verbal paradigms with a permanent categorial marker throughout person/number oppositions, 'mixed' paradigms characteristically change the encoding of Imperative with the shift in person/number. Compare the examples from in English (5), where 2SG/PL is a specialized synthetic form and first- and third-persons are identical periphrastic forms (the lexeme of the content verb is underlined, the formants are bolded), with the Russian examples in (6), where all three person-values are marked differently: second-person synthetically, first- and third-persons as different periphrastic constructions.

(5) a. 1SG/PL: **Let** me/us <u>go</u>!
     b. 2SG/PL: <u>Go</u> -(**Ø**)!
     c. 3SG/PL: **Let** him/them <u>go</u>!

(6) a. 1SG/PL   **Dava-j/te**   ja/ my   **po-**<u>jd</u>**-u/em!**
                       give'-IMP.2SG/PL   1SG/PL.NOM   PRF-go-1SG/PL
                       'Let (thou/you) me/us go!'

(6) b. 2SG/PL *Id-**i**/**te**!*
               go-IMP.2SG/PL
               'Go!'

     c. 3SG/PL ***Pust'*** *id-**et**/**ut**!*
               PART go-PRS.3SG/PL
               'Let him,her/them go!'

Conversely, in Yucatec Maya, one form is periphrastic, (7)a, and two forms are different synthetic forms, (7)b and c:

(7) a. 1PL   **Ko'ox j**   kay !
               HORT   SUBORD sing
               'Let's sing'

     b. 2SG/PL tz'íb' **-t**   **- e**!
               Write -TR   IMP
               'Write it!'

     c. 3SG   Wen-**ek**   (Ø) !
               sleep-IRREAL (3SG)
               'Let him sleep!'
                              (Andrew & Ojeda 1994)

Singling out mixed paradigms raises the issue of 'multiple marking'. There can be a whole set of constructions associated with non-second-person values, sometimes even with second-person. Thus (8) shows the Russian 1PL Imperative expressed in a variety of ways.

(8) a. *Davaj (te) pojdem!*
     b. *Pojdem (te)!*
     c. *Pojdem (te) - ka!*
          'Let's go! (1PL)

Spanish has alternative constructions for all persons, including second-person, shown in (9):

(9) a. *Vete!*
     b. *Ve!*
     c. *Que tu vayas!* 'Go! (2SG)'

Because of this multiplicity of constructions, there is a problem of choice. Should all the constructions be included in the paradigm? If not, which of them should be considered as basic, and consequently included in the mixed paradigm? This is a special issue not to be discussed in detail here. But at least two criteria of choice can be mentioned. First, one may choose the construction with the highest pragmatic neutrality;

thus, in (9) *Ve!* is more abrupt than the neutral *Vete!*, and (8)c is too unceremonious as compared to (8)a and b. Second, one may choose the construction which combines with more verbal lexemes; for example, the construction in (8)a does not have restrictions on combining with verbal lexemes or aspectual forms, whereas (8)b certainly does. Thus (10)a inarguably has a straightforward Imperative meaning, but (10)b has a strong inchoative component of immediacy, and (10)c is acceptable only under special contextual circumstances.

(10) a. *Davaj(te) čitať!*
     b. *Čitajem!*
     c. ? *Čitajemte!*
         'Let's read!'

3. INCOMPATIBILITY OF THE CONCEPTS 'MIXED PARADIGM' AND 'VERBAL CATEGORY'. A regular verbal paradigm is characterized by the following features: 1) its members are verb-forms, 2) categorial marking is uniform throughout person/number oppositions, 3) the form of a marking mechanism is specific for each category, and 4) one member of a paradigm has only one way of actualization. Mixed paradigms are incompatible with these properties of a 'regular' paradigm. First, members of mixed paradigms are not all verb-forms, even in the weakest sense of the term, when 'analytical' forms Aux+V can be accepted as morphological (cases like (2)a and c, or (4)b), but not cases like (3), (4)a, or (7)a, when the marking mechanisms are scattered over the whole construction. Second, the categorial meaning is often marked differently in different persons, cf. (5), (6), and (7). There are two other features of mixed paradigms. First, forms associated primarily with other categories appear in them (e.g., subjunctive in (3)a and (7)c; future indicative in (4)a, present indicative in (4)b, (6)c, and (9)c; hortative in (7)a). Second is the existence of multiple mechanisms of marking imperative for one and the same person-value ((9) and (10)). The first two properties of verbal paradigms are indisputably basic and crucial, and a violation of them is incommensurable with its essence. The commensurability of the last two features can be disputed. It is common knowledge that many grammatical markers are multifunctional and can be used as markers of several grammatical meanings, as a rule distinguishable from one another in the lexico-grammatical context. The existence of a multiplicity of forms marking one and the same person-value can be explained, as was mentioned earlier, by pragmatic and combinatorial factors.

Thus the concept of mixed paradigms contradicts in at least two respects the principles of 'regular' verbal paradigms.

4. THEORETICAL IMPLICATIONS OF THE RECOGNITION OF MIXED PARADIGMS. Acceptance of a mixed paradigm requires justification for extending the Imperative category to non-second-person periphrastic constructions which violate the basic properties of a verbal paradigm. But refusal to accept mixed paradigms is also unsatisfactory, on at least two grounds. First, from a cross-linguistic point of view, it

seems arbitrary to exclude strictly grammaticalized constructions from a given paradigm simply because they are not expressed synthetically in a particular language; cross-linguistic description deals with languages with a variety of marking patterns, and needs common theoretical grounds for describing any category. Second, it is not satisfactory to exclude all non-second person forms, whether synthetic or periphrastic, from Imperative paradigms (as in Mithun 1999:171 ff.) because, if even synthetic non-second-person forms which are morphologically parallel to second-person Imperative forms are not Imperative forms, what category are we to affiliate them with, declarative or interrogative?

So we have a paradox. We cannot neglect some data, i.e., constructions functionally identical to synthetic forms and expressing an immediate directive (and thus inherently imperative). But the existing theory cannot accept them because of its internal restrictions. This paradox is evidence that the existing theoretical framework is inadequate to the data and that there is a need for a new and more adequate framework. The theory that the imperative is a verbal category of mood predicts, given certain assumptions, that the imperative will be a verb form at least in all languages where this category exists and that there will be consistent marking of this category throughout the person/number paradigm. But there are languages in which imperative meaning is apparently expressed by periphrastic constructions which cannot be reduced to the notion of a verb-form and there is no consistency in marking elements of this paradigm. This fact refutes the assumption that the imperative is a universal verbal category. We have a typical example of Popper's 'method of falsification' which considers the inability of a hypothesis to withstand refutation as evidence of its falsehood. In short, the hypothesis that the imperative is a verbal category does not withstand efforts to refute it.

To accommodate mixed paradigms, the new framework must be able to explain at least five types of evidence:

1. the inclusion of verb-forms and periphrastic constructions in a single category;
2. the multiple constructions used by different languages to mark each person-value;
3. the frequent difference in marking of Imperative meaning in first, second, and third persons;
4. the frequent use of forms associated with other verbal categories (causativity, modality, subjunctive, hortative, future indicative) as markers of Imperative in non-second-person synthetic forms; and
5. the reconciliation of the need for an addressee with the fact that the Directive is issued to the first or third person.

5. SPEECH-ACT-BASED INTERPRETATION OF THE IMPERATIVE. All this evidence can be easily explained within a framework where the imperative is interpreted as a category of speech act. The speech-act nature of the imperative is widely discussed (Hamblin

1987, Jakobson 1995/1976, Lyons 1977, Padučeva 1985, Sadock and Zwicky 1985, Wierzbicka 1972, 1995, etc.); it is also considered a form of directive (Searle 1975, 1976). Within a speech-act approach three specific claims are made. First, the imperative does not modify the verb but a proposition (Bybee 1985, Hamblin 1987, Foley & Van Valin 1984, etc.). Second, the semantics of the imperative is not a matter of subject-predicate relations inside a proposition, but is speaker-oriented and thus outside the proposition (Bybee et al. 1994, Grice 1957, Foley & VanValin 1984, Jespersen 1992/1924, etc.): the speaker wants to cause cooperation or willingness, etc. Third, the semantics of the imperative is not a simple primitive but is a complex of semantic components (Wierzbicka 1972/1995, Hamblin 1987, Moutafakis 1976). But these three claims are made as separate considerations. They have never been put together as a system of interconnected properties. So proponents of the speech-act approach have never proposed that we should consider the imperative a special type of grammatical category uniting various synthetic forms (morphological forms) and constructions ('syntactic' forms) within one paradigm. The consensus remained that the central synthetic forms belong to a verbal category of mood and that the periphrastic constructions belong to syntax.

I incorporate the discoursal features of imperative in one system and claim that the imperative is a regular grammatical category, but of a special type. It is not a part-of-speech category but a 'frame-forming' type of category. Such categories are formed by the introduction of additional predicates which specify the content of a proposition in certain ways. I discuss the implications of this approach for the theory of the imperative and demonstrate that within this approach the concept of mixed paradigms can be naturally accepted.

6. PLACEMENT, GRAMMATICALITY AND MULTIPLICITY OF MARKERS IN MIXED PARADIGMS. If the imperative is a frame-forming category, it follows that the invariant component of this category is a proposition, not a verb. If so, then the imperative paradigm is a set of constructions and the marker of the grammatical variable (imperative) can be positioned anywhere in the construction. The marker is not necessarily attached to the verb. Thus it is irrelevant whether members of the paradigm are constructions or verb-forms, since both realize a proposition.

One might question how we can tell whether a periphrastic construction is part of a grammatical paradigm. I suggest that in order for a formal change in a construction to qualify as a grammatical mechanism it must be grammaticalized and combine regularly with propositions to form the meaning in question. In the present example, these conditions are met. Every language has a set of permanent markers to encode the transition from declarative to imperative, or distinguish imperative from an interrogative.

The existence of multiple marking mechanisms for each non-second person is, as factual evidence suggests, superficial. In such a set of 'synonymous' constructions only one construction is neutral; the others encode imperative with all kinds of face work components (politeness, etiquette forms, hierarchy, etc.) or are not as regular as the basic one. Several languages have multiple constructions even for the second-person,

in the database used—Spanish, Russian, Mayan, and Mandarin. Mandarin, according to informants, has several options for second-person constructions with diverse particles. The selection among them depends on a variety of social factors (e.g., politeness strategies, strength of pressure).

Thus, the selection of basic constructions (the ones least loaded with pragmatic additions) constituting a mixed paradigm is governed by quite strict grammatical rules and is never random. In short, the mixed paradigm meets the properties of formal grammaticality which can be expected from any grammatical category.

7. COMPONENTS OF IMPERATIVE MEANING. The composite meaning of the imperative explains why it can be (and actually is) encoded differently in different person-value constructions, why it uses 'alien' forms in non-second-person construction, and how the addressee's role is preserved in all constructions.

I claim that the semantic structure of the imperative (considered as a marked member in the opposition declarative-imperative) includes at least the following four components:

1. an appellative component (Jakobson's 'conative function'),
2. a causative/volitional component (which, if necessary, can be broken into volition and causation),
3. the proposition represented by a content verb, and
4. 'framing-inclusion' relations (to frame and to be framed).

Identification of these components is supported by three independent types of evidence: cross-linguistic (a list of types of marking mechanisms reiterated in different languages), pragmatic (analysis of semantic components singled out within communication studies), and logical (decompositions of Imperative meaning by logicians for formal language description). The lists of proposed components gathered from these three independent sources are amazingly similar. On my analysis, then, the Imperative situation is a semantic hybrid, and consequently a syntactic hybrid, of diverse components. It encompasses three component situations: the appellative situation, the causative/volitional situation, and the content situation.

8. MECHANISMS OF MARKING IMPERATIVE. Each component situation has its own predicate and its own set of arguments. As in any hybrid, the component situations interact and overlap. Arguments interact, and predicates interact, including first-order predicates in the framed situation and second-order predicates in the framing situation. Each type of interaction must be marked explicitly, but the question of how it can be marked needs elaboration.

8.1. ARGUMENTS. The appellative situation necessarily includes two arguments: Addresser and Addressee. It may or may not include a third participant, a non-direct interlocutor. The causation/ volition situation includes a Causer (issuer of causation

and bearer of volition) and a Causee (someone who is caused to act, but can have wilfulness of his own). The content situation has at least one argument, a 'Doer' (D), though it can be more complicated in languages with prototypical passive-based imperatives (e.g., Austronesian languages), where the Patient of the content situation is more important than D (Manaster Ramer 1995, Polinsky 1992). Blending the three sets of participants results in a new set with three macroroles: the speaker S, the listener L, the third party T. D does not exist on its own; it overlaps with S, L, or T.

The macrorole S (Speaker) must combine the roles of an Addresser and a Causer and can overlap with D. Since it is always associated with 1SG, it does not need marking, unless it overlaps with D. The macrorole L (Listener) must include the Addressee role and can also include the roles of Causee and Doer; this combination is the prototypical combination in the imperative situation. L is associated with the second person and is part of a central specialized form of the imperative. The macrorole T (third party) becomes part of the imperative situation only if it overlaps with D; this role must be explicitly marked.

Thus, the marking of the imperative must obligatorily include marking D if D is not the Addressee. Even where D is the Addressee (i.e., in second-person constructions), marking of D is strictly obligatory if specialized pronouns are used for imperative as in Palauan, which has 'hypothetical' pronouns for imperative (Josephs 1975:110). D is also obligatorily marked if person/number categories are marked on the imperative form of the verb. The mechanisms for marking D are standard: verb-agreement and/or pronouns (Sadock & Zwicky 1985:171).

(11) a. English   *Go!*   *Let **us** go!*   *Let **him** go!*
     b. Russian   *Idi/Idite!*   *Davajte pojd**em**!*   *Pusť id**et**!*
     c. German   *Gehe! Geht!*   *Wollen **wir** gehen/ Laß **uns** gehen!*   *Laß **ihn** gehen!*

The marking of D is an indispensable component of an imperative construction, not an agreement category as in verbal categories. Evidence from cross-linguistic comparisons proves that all other personal markers are optional, though some languages (Russian, Spanish) mark person/number values for Listener/Addressee on Auxiliaries or particles separately from person/ number marking on the content verb that communicates D, as in (12) and (13).

8.2. PREDICATES. The predicates constituting an Imperative situation are of two types, 'framing' (appellation, causation/volition) and 'framed' (content verb). The framing predicates define the nature of the speech act; the framed predicate defines its content. The framed predicate must be actualized by the content verb; the only variation is in the morphological form of this verb, which I will discuss shortly. The framing part must also be actualized. But the way it is actualized varies. It can be encoded by a special inflection on the content verb, as is the rule for second-person Imperatives and sometimes for other synthetic constructions. In first and third person constructions it can take the form of an actualization of one of the framing predicates. Selection

among these predicates evidently is based on what was diachronically chosen as a semantic dominant of a directive: causation, volition, or the need for the addressee's approval, etc. The appellative predicate can be actualized directly, as in Russian, where it combines with a component of expectation of cooperation on the part of L. Diachronically this auxiliary goes back an imperative with the meaning 'give', which is preserved in the content verb.

(12) **Dava -j /   -te   ja   po-jd-u!**
      [give] -2SG.IMP 2PL.IMP 1SG.NOM PRF-go-FUT.IND.1SG
      'Let me go'; literally, 'Hey-you (SG/PL), I will go!'

More commonly the appellative predicate is represented indirectly via its arguments: by vocatives or by number-agreement marking of the Addressee, as in Spanish.

(13) *Déj -**a**   -me   leer   la   nota!*
      let -**SG.ADR** -SG:REFL read   the note!
      'Let me read the note!'

Actualization of the appellative predicate evidently highlights the recognition of the existence of L or of the need for L's cooperation/agreement. The fact that the address function of imperative is explicitly encoded in many constructions is evidence that it is inherently present in each of the three-person forms or constructions of the imperative paradigm.

Causation/volition predicates are more commonly actualized than the appellative predicate in imperative constructions. They can be marked straightforwardly by grammatical causatives, though no longer preserving the causative grammatical meaning (English *let*, German *lassen* 'let'). Alternatively, they can be marked by particles (Russian *pust'*, going back to causative *pustit'* 'release/let go'), by other delexicalized verbs, or by other auxiliaries which go back to grammatical inchoatives originating from 'movement' verbs as with Hebrew *ba* 'come' (Malygina 1992:146) and Yucatek Mayan *ko'ox/ko'on-e'ex* 'go' (Hofling & Ojeda 1994:282).

In summary, the framing predicates must be actualized, but the way they are actualized depends on the selected dominant, level of grammaticalization, or paths of grammaticalization (Bybee et al. 1994, Hopper & Traugott 1993). There can even be zero actualization, marked only in the demonstrated dependent status of the content verb (e.g., in Spanish *que*-constructions).

8.3. FRAMING. Marking the framed status of the propositional predicate is obligatory. There is a variety of mechanisms to actualize this meaning. They all mark the non-actual character of the action, its shift from an objective reality to a desired or possible one. So the content verb may be 'marked' as an infinitive (as in English, German), a future (Russian), an optative form (as in Turkic languages), or a Subjunctive form (Romance languages). Or the content proposition may be introduced by

subordinators (all equivalents of *that*) like French *que*, Spanish *que*, Rumanian *să*, Mayan *-j-*. Sometimes the framed status of the predicate in indicative forms is marked by a different word order, as in German *Gehen wir schneller!* 'Let's go quicker!'.

8.4. AUTONOMY AND GENERIC FUNCTION OF MARKING MECHANISMS. Almost no Imperative construction explicitly encodes all the semantic components which mark a shift from declarative to imperative; only some are marked. Marking the shift from indicative to imperative within each of the three domains discussed above (person of D, 'framing', and 'inclusion') is obligatory, but the complexity of imperative meaning makes it impossible to predict which marker is selected.

The independence of marking each component explains the existence of diverse ways of marking the imperative within a paradigm and at the same time preserves its holistic meaning. I speculate that different markings realize the component(s) of the complex imperative meaning that were historically treated as dominant. Thus first-person constructions often imply the cooperation of L, or even L's willingness to participate (cf. German *wollen* 'will/want'). Since the whole set of markers is never realized within one construction, those which are realized serve as generalized 'representatives' of the whole set of semantic components constituting the imperative meaning. Which of the markers is actually present in the construction is language-specific. One can predict that there will be some marking, and one can specify the possibilities from which the marking can be chosen, but one cannot predict the actual choice.

9. CONCLUSION. I have claimed that the mixed imperative paradigm better reflects the nature of imperative than the second-person synthetic paradigm. I argue that the peculiarities of mixed paradigms can be explained within a framework treating the imperative as a speech-act category rather than a verbal category. I have laid out this framework as a systematic complex. The imperative is a frame-forming category of a proposition, clustering with declarative and interrogative. The semantics of this category is oriented to the interlocutors and is a composite of numerous components. Within such an approach, the mixed paradigm is a grammatically regulated set of person/number oppositions with predictable linguistic behaviour, not a disorganised set of 'imperative syntactic constructions' with random properties.

In support of my claims I discuss diverse types of evidence: factual evidence, evidence of inconsistences in the old approach, and evidence of the greater explanatory force of my position. These are the three basic types of evidence relevant to a change of theoretical approach in science.

REFERENCES

ANDREW, CHARLES & FERNANDO L. OJEDA. 1994. Yucatek Maya imperatives and other manipulative language. *International journal of American linguistics,* vol. 60.3:272–94.

Bybee, Joan. 1984. *Morphology.* Amsterdam: John Benjamins.

———, Revere Perkins & William Pagliuca. 1994. *The evolution of grammar: Tense, aspect, and modality in the languages of the world.* Chicago: The University of Chicago Press.

Foley, William & Robert Van Valin. 1984. *Functional syntax and universal grammar.* Cambridge: Cambridge University Press.

Grice, H.P. 1957. Meaning. *The philosophical review*, vol. 66.3:377–88.

Hamblin, C.L. 1987. *Imperatives.* Oxford: Basil Blackwell.

Hopper, Paul J. & Elizabeth Closs Traugott. 1993. *Grammaticalization.* Cambridge: Cambridge University Press.

Jakobson, Roman. 1995/1976. The speech event and the structure of language. In *Roman Jakobson. On language,* ed. by Linda R. Waugh & Monique Monville-Burston, 69-79. Cambridge ma: Harvard University Press.

Jespersen, Otto. 1992/1924. *The philosophy of grammar.* Chicago: University of Chicago Press.

Josephs, Lewis S.1975. *Palauan reference grammar*. Honolulu: The University Press of Hawaii.

Khrakovskij, Victor S. (ed.). 1992. *Tipologija imperativnyx konstrukcij.* St. Petersburg: Nauka.

Lyons, John. 1977. *Semantics.* Cambridge: Cambridge University Press

Malygina, L.B.1992. Imperative sentences in Mordern Hebrew. In Khrakovsij 1992, 144–52.

Manaster Ramer, Alexis.1995. On the subject of Malagasy imperatives. *Oceanic linguistics,* vol. 34.1:203–10.

MacAulay, Donald (ed.). 1992. *The Celtic languages*. New York: Cambridge University Press.

Mithun, Marianne. 1999. *The languages of native North America.* Cambridge: Cambridge University Press.

Moutafakis, Nicholas J. 1975. *Imperatives and their logic.* New Delhi: Sterling Publishers PVT.

Padučeva, Elena V. 1985. *Vyskazyvanije i ego sootnesennosťju dejstviteľnostju*. Moscow: Nauka.

Polinsky, Maria. S. 1992. Imperativ i drugije sposoby vyrazhenija povelenija k dejstviju v jazyke maori. In Khrakovskij 1992, 219–27.

Sadock, Jerrold M. & Arnold M. Zwicky. 1985. Speech act distinctions in syntax. In *Language typology and syntactic distinctions, vol. 1. Clause structure.* ed. by Timothy Shopen, 155–96. Cambridge: Cambridge University Press.

Searle, John R. 1975. Indirect speech acts. In *Syntax and semantics*, *vol. 3*, ed. by Peter Cole & J.L. Morgan, 59–82. New York: Academic Press.

———. 1976. A classification of illocutionary acts. *Language in society* 5:1–23.

Švedova, N. Ju. 1980. *Grammatika russkogo jazyka.* Moskva: Akademija Nauk, SSSR

Wierzbicka, Anna. 1972. *Semantic primitives*. Frankfurt-am-Main: Athenäum.

———. 1995. A semantic basis for grammatical typology. In *Discourse grammar and typology*, ed. by Abraham Werner, Talmy Givón & Sandra A. Thompson, 179–209. Amsterdam: John Benjamins.

NEGATIVE-IMPERATIVE CLITIC PLACEMENT IN ITALIAN: SYNTAX OR PHONOLOGY?

Floricic Franck
ERSS - Université de Toulouse II – Le Mirail

THE QUESTION OF CLITICS[1] is a very complex one constituting a field of inquiry in its own right. This paper addresses the question of clitics in negative imperative constructions in Italian. As is well known, Italian clitics precede finite verb forms, as indicated in (1). However, the examples in (2) show that clitics follow the verb in non-finite contexts, as well as in the imperative.

(1)   a. *Lo prendo*
          it take-1SG.
          I take it.

      b. *Bisogna che lo prenda*
          [it is] necessary that it take
          I have to take it.

(2)   a. *Presolo per mano*
          taken-him for hand
          Once I took his hand.

      b. *Prendendolo*
          taking-him/it

      c. *Prendilo*
          take-it!

      d. *Penso di prenderlo*
          think-1SG. of take-it
          I think of taking it.

Now, in the negative imperative and when the infinitive appears after a modal, both linear orders are possible:

(3)   a. *Non lo fare / Non farlo*
          not it do / not do it
          Don't do it!

      b. *Lo devi fare / Devi farlo*
          it must-2SG. do / must-2SG. do-it
          You have to do it.

      c. *Non lo devi fare / Non devi farlo*
          not it must-2SG. do /
          not must-2SG. do-it
          You mustn't do it.

      d. *Lo puoi fare / Puoi farlo*
          it can-2sg. do / can-2sg. do-it
          You can do it.

The problem is how to explain this double pattern in the negative imperative. Is it a syntactic phenomenon or a phonological one?

1. KAYNE (1992) AND THE NOTION OF 'EMPTY MODAL'. Kayne (1992) introduces the notion of empty modal to account for Clitic–Verb order in the Italian negative imperative. According to Kayne's hypothesis, the infinitive is embedded under the empty modal. The empty modal is in turn licensed by the negation *non*, that is, by the head of

the NegP projection. This analysis therefore allows Kayne to see the Clitic-Verb order as an instance of clitic climbing. In a sentence like *Non lo fare!* 'Don't do it!' the object clitic *lo* is raised up to the empty modal, as shown in (4).

(4)   Non ⎴far - lo
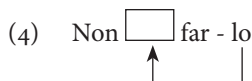
Given that the infinitive is embedded under the empty modal, the existence of this empty modal seems to account both for the linear ordering and for the appearance of the infinitive. Despite its interest, this analysis has the drawback of putting on the same level expressions like *Non farlo!* and *Non devi farlo!* 'you mustn't do it', and it fails to capture the relationship between the non-injunctive use of the infinitive and the injunctive one (cf. examples such as *Non farlo sarebbe un errore* 'not to do it would be a mistake'). On the other hand, it is also possible to consider that in the case of *Non farlo!*, the infinitive is selected because of its semantic neutrality. From this point of view, there is no need to postulate the existence of any underlying empty modal.

2. GRAFFI (1996) HYPOTHESIS. Another account is that of Graffi 1996. According to Graffi, in the negative imperative, the negation and the verb are raised to the head COMP of the Complementizer Phrase (CP). In this case, the sentence *Non farlo!* shares the same syntactic representation as a sentence like *Non prendetelo!,* as shown in (5).

(5)   [CP[COMP Non prendete]] [IP lo]   [CP[COMP Non far]] [IP lo]
       Don't take it!   Don't do it!

Agr is not, in the imperative, an autonomous projection but a feature of the head COMP; according to Graffi, this Agr feature is defective in the sense that it contains number features but no person features. Therefore, in the case of *Non lo fare!*, the clitic precedes the verb because the features of Agr in COMP are weak in the sense of Chomsky (1995, chapter 3)[2]. As a matter of fact, in the minimalist theory, all movement is triggered by the need for feature checking. Depending on the strength of the feature, the movement is either overt (when induced by a strong feature) or covert (when induced by a weak feature). Therefore, given the weakness of the Agr features in COMP, overt raising of the verb is not required: the verb raises in COMP at LF and the imperative value is brought about by the negation in COMP.

Now the contributions of Kayne and Graffi touch on many interesting questions that deserve a more detailed discussion. The main point is that their approaches share the presupposition that an autonomous syntactic analysis can adequately account for the distributional properties of clitics; in this kind of approach, the only question is to identify, within the functional organization of the sentence, the base position and the landing site of the items. However, the points to which I would like to draw attention are the semantic neutrality of the infinitive and the prosodic weakness or deficiency of clitics.

3. DESCRIPTION OF THE ITALIAN DATA. First of all, recall an important fact about clitics in Italian: they never precede the negation *non* or the infinitive in root contexts; therefore, the essential frames are those in (6)a–b.

(6) a. [[non] (cl)]   b. [[V*inf*] (cl)]

The converse holds for contexts of clitic climbing: they always precede modal verbs in root contexts, as in (6)c–d.

(6) c. [(cl) [modal V]]   d. [[V*inf*] (cl)]

Now when we put these frames together, we obtain the structures in (7).

(7) a. [[[non] (cl)] [[V*inf*] (cl)]]   b. [[(cl) [modal V]] [[V*inf*] (cl)]]

At this point, two questions arise. Given the structures in (7)a–b, why does Italian prohibit sentences like *Non lo farlo* or *Lo voglio farlo*, with reduplication of the clitic[3], and why is the so-called clitic climbing phenomenon limited to modal contexts? To answer the first question, we suggest that the two clitics are adjacent in the Verbal domain, as in (8).

(8)

Now a configuration like (8) must be ruled out because of an OCP violation: two adjacent elements of the same nature cannot co-occur in the same domain[4]. Therefore, one of the two clitics must be deleted. To answer the second question, we must recall that from a semantic point of view, the infinitive stands, in the mood system, in the same position as the present in the tense system, the nominative in the case system or the third person in the person system[5]. In other words, it is the neutral element of the system. It is neutral insofar as it lacks any specification, and it is neutral insofar as it can be assigned a default value. Of course, this characterization of the infinitive is not new. It is commonly assumed among philologists that the infinitive is semantically unspecified. This idea is defended by Skytte (1983:24):

> At the semantic level, we can say that the infinitive… is the verbal form which expresses in the most neutral way the pure content of the verbal stem. A content which, naturally, in a given context, lets itself be modified in different ways.

In other words, the infinitive is a non-specified form which is nonetheless contextually specifiable; and its specifiable nature stems from being a tense *in posse,* as Guillaume (1929) says[6]. This is the reason why infinitival imperatives are compatible with

different linear orders, while, for example, negated gerunds only allow enclisis. From this point of view, we can consider that the double possibility of *Non lo fare!* and *Non farlo!* is at least in part governed by the syntactic and semantic properties of negation and the infinitive.

4. PROSODIC CONSTRAINTS ON CLITIC PLACEMENT. From a prosodic point of view however, it looks like the two configurations are certainly not equivalent. Now this non-equivalence is mainly the result of the properties of the negative particle *non*. In keeping with Zanuttini (1991) and against Belletti (1990), it should be noted first of all that the Italian negation *non* differs fundamentally from clitics. While clitics like *lo, la, le, li, ci, vi* are atonic and prosodically dependent, *non* can indeed bear stress. More importantly, *non* can be used in a wide range of contexts in which only independent content words are licensed. As shown in examples (9)a–c and (9)d–e, *non* can appear as part of a conjunct or of a dependent clause.

(9) a. *Dimmi se ti piace* **o non** *(ti piace)* (Serianni & Castelvecchi 1989:506)
          tell me if you like it or not
     b. *Gli sposati* **e non**
          the married and unmarried
     c. *Noi gli facciamo molte domande, lui qualche volta ride, risponde ad alcune* **e non** *ad altre, si vede bene che evita certi argomenti* (Levi 1958:22)
          We are asking him a lot of questions; sometimes he laughs; he is answering some of them but not others; it is clear that he is escaping some topics.
     d. *A che cosa potevo io aspirare ormai* **se non** *ad un uomo come lui?*
          (Moravia 1988:415)
          What could I desire other than a man like him?
     e. *Vado più spesso in Italian che* **non** *in Germania*
          I am going to Italy more often than to Germany

From the examples in 9, it is clear that *non* shows syntactic and prosodic properties that make its clitic status questionable. These examples also show how different are the forms *ne* in French and *non* in Italian. Actually, the properties of *non* clearly show that it has the status of Prosodic Word. In a sentence like *Non lo fare!,* the negative morpheme defines a domain—the *Prosodic Word*—of which it is the head or nuclear element. At the same time, however, there is no reason to associate the clitic to the initial foot rather than to the following one. Therefore, I would like to suggest that in *Non lo fare!,* the clitic actually is amphipodic. In other words, as shown in Figure 1, the object clitic depends at the same time on the foot dominating the syllable *non* and on the foot whose head is the strong syllable of the infinitive.

It should be observed that the amphipodicity/amphicliticity hypothesis is not new:

In most languages, enclisis does not occur when the imperative is introduced by a tonic word, which allows the amphiclisis of the pronoun… Negation belongs to

PrW   PrW
│   │
F_h   F
├┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄┄
σ   σ   σ_h   σ
╱│╲   ╱│   ╱│╲   ╱│
μ μ   μ   μ μ   μ
│ │   │   ╲│╱   │
n ɔ n   l o   f a: r e

Figure 1. *The object clitic depends at the same time on the foot dominating the syllable*
non *and on the foot whose head is the strong syllable of the infinitive.*
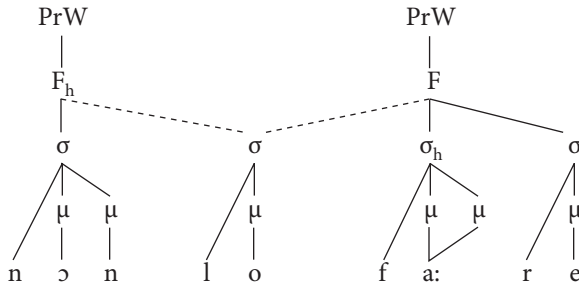
> the tonic introductory elements (French *Ne m'aide pas* ; Italian *Non mi aiutate*),
> as well as strengthening particles (Old French. *Si m'aidez, or m'aidez* ; Italian
> *Or m'aiutate*), or conjunctions (French *prends ton luth et me donne un baiser*
> (Musset)). (Lausberg 1971: §723 and §725, 2., a), β), III. (cf. p. 4, Lausberg)

We should nonetheless notice that in some cases—namely as a result of both the tight relationship between negation and the clitic and the focus prominence on negation—the initial consonant of the object clitic *lo* can assimilate the final consonant of *non*: e.g., *Nollo fare!* ['nɔl:o'fa:re] 'don't do it!'. In this context, *non* therefore shows a high degree of cohesion with the clitic. This also recalls expressions like *Dammi quello!* ['dam:i'kwɛl:o] 'give it to me!'. In both cases, we therefore find an optimal configuration of two trochaic feet, as in Figure 2 (overleaf).

As shown in Figure 2A, the clitic of the sentence *Dammi quello!* prosodically and syntactically depends on the verb alone. Conversely, in *Nollo fare!*, the syllable corresponding to the clitic is delinked from the following foot and is exclusively attached to the preceding one. Now the assimilation of [n] can be interpreted as the segmental correlate of this prosodic reorganization. From that point of view, the expressions *Dammi quello!* and *Nollo fare!* show rather similar prosodic configurations. From a rhythmical standpoint, they share the same optimal alternation between strong and weak positions, as in (10):

(10) a. $\begin{matrix} * \\ * \\ * \\ * \end{matrix}$ $\begin{matrix} \\ \\ * \\ * \end{matrix}$ $\begin{matrix} \\ * \\ * \\ * \end{matrix}$ $\begin{matrix} \\ \\ \\ * \end{matrix}$ b. $\begin{matrix} * \\ * \\ * \\ * \end{matrix}$ $\begin{matrix} \\ \\ * \\ * \end{matrix}$ $\begin{matrix} \\ * \\ * \\ * \end{matrix}$ $\begin{matrix} \\ \\ \\ * \end{matrix}$

      Dam mi quel lo   Non lo fa re

As shown in (10)a–b, *non* and the strong syllable of the imperative form have a prominence at the highest level of the prosodic hierarchy. Now this prominence also favours the second position of the clitic in the negative imperative, and the semantic/prosodic weakness of the adjacent clitic enhances in turn the focus prominence of the strong syllable:

A.

B.
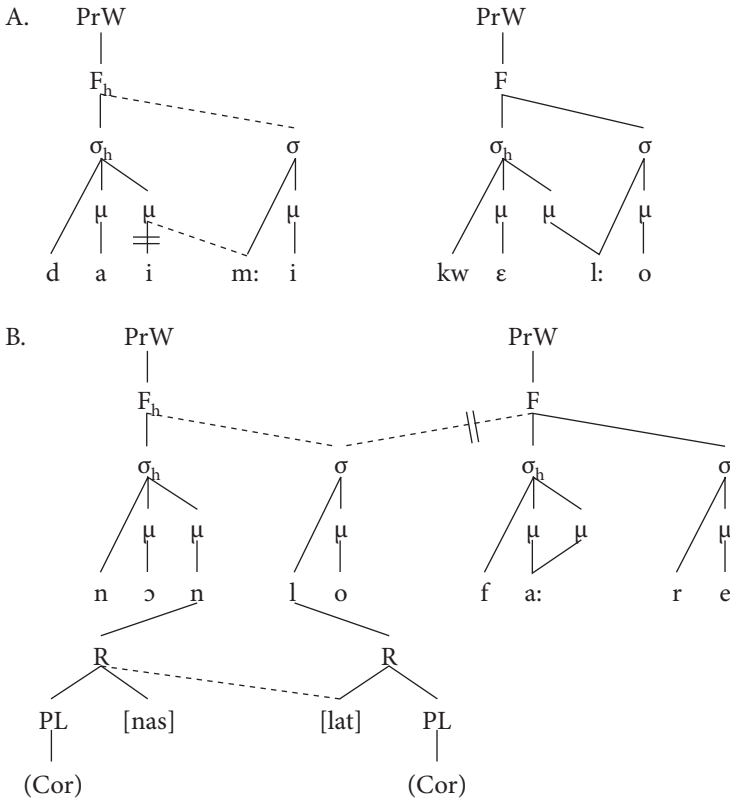
Figure 2. *An optimal configuration of two trochaic feet.*

(L)'accent d'un mot ou d'une syllabe est d'autant plus fort qu'il y a plus de mots ou de syllabes sur lesquels il domine… S'il faut appuyer fortement sur un mot, mettez près de lui un autre mot sur lequel le sens n'exige pas qu'on appuie; et le mot accentué, quand même il ne se trouve ni au commencement ni à la fin de la phrase, aura une place avantageuse; car l'accent est mis en relief par un *repos d'accent* qui l'accompagne (Weil 1844:91).

(…) the stress of a word or of a syllable is all the more strong since it dominates more numerous words. (…) If a word must be strongly strengthened, just put next to it another word which need not be strengthened from a semantic point of view; then, the stressed word will have a prominent place even though it is not at the beginning or at the end of the sentence; for the stress is focused by a 'stress pause' which goes together with it.

Now the absence of such a prominence on *non* brings about the obligatory enclisis in sentences such as *Non farlo sarebbe un errore*. As pointed out by Nespor (1993:237),
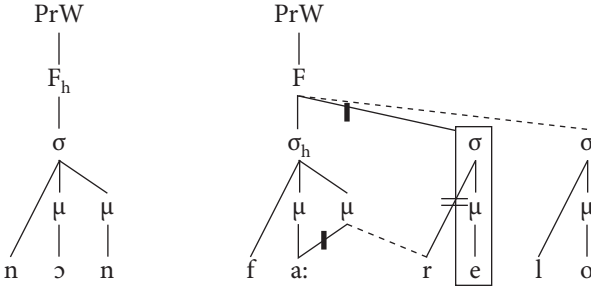
Figure 3. The clitic lo *attached to the preceding foot.*

'In Italian, the main stress of the rightmost word in the sentence is the strongest of the whole sentence, **in the absence of particular emphasis**'. Therefore, a sentence like *Non farlo sarebbe un errore* has End Rule Final. On the other hand, the second singular imperative, being a form *on-focus*, can bear the main phrasal/sentential stress. That's why in this case, the strong syllable of the imperative shares with *non* the major prominence. In short, the Pitch Accent Prominence Rule (PAPR) takes precedence over the Nuclear Stress Rule (NSR).

The next question is how to analyze the other variant of *Non lo fare!,* namely *Non farlo!* First of all, it should be noted that the final vowel of the infinitive *fare* is elided before the initial consonant of the enclitic. In this case too, we should interpret the elision as a cue for the metrification of the clitic at the foot level[7].

In Figure 3, the clitic *lo* is indeed attached to the preceding foot, which preserves its trochaic structure. We should, however, add another important fact: the expression *Non farlo!* shows two adjacent stressed syllables, *non* and *far*. Therefore, we have a clash which is, in this case, tolerated because it represents a minimal clash[8]. In other words, as shown in (11), we find two adjacent prominences at the level of Prosodic Word; the clash thus creates an arhythmic structure which doesn't trigger any 'repair strategy'.

(11)
```
 *
 *    *
 *    *
 *    *    *
Non  far  lo
```

Moreover, in some contexts, the first syllable of the infinitive can in turn receive the uppermost prominence of the negation—needless to say, this is a double prosodic pattern.

(12)
```
 →    *
 *    *
 *    *
 *    *    *
Non  far  lo
```

On the other hand, when the negation is followed by an imperative form like *Fallo!* 'do it!', the clash occurs at a higher level; therefore, the violation is greater. It is worth pointing out that the second person singular of the imperative has an intrinsic focus. Given that the stressed syllable of the imperative form is also the most prominent in the sentence, and given that *non* has as its domain the whole sentence, the result is a clash at the highest level of the prosodic hierarchy, as in (13)

(13)   *   *
        *   *
        *   *
        *   *   *
     *Non   far   lo

Therefore, we have in this case a prosodic and semantic clash which requires the implementation of a repair strategy; this repair strategy is brought about using a neutral, amodal form, the infinitive.

5. THE DIACHRONY OF CLITIC PLACEMENT. At this point, I would like to mention a diachronic fact which seems to be relevant to this discussion. In Medieval Italian, expressions like *Non lo fare!* were very common; the clitics indeed couldn't stand in initial position, and they needed the presence of a tonic form at the beginning of the sentence or of the period. Therefore, they usually stood in the second position in the sentence– the position known as Wackernagel's position. But Wackernagel's main work dates to 1892. The end of the nineteenth century gave rise to a large amount of literature on word order. Adolfo Mussafia (1886) showed, on the basis of a corpus of medieval texts, that in Old Italian, clitics were absolutely excluded from the first position of the sentence; thus, expressions like *Pregoti* ['prɛgoti] 'I pray you', *Pregailo* [pre'gajlo] 'I prayed him)', or *mostròllo* [mos'trɔl:o] '(s)he showed it' were very common. As we can see, the clitic placement rules have undergone a deep change from the Old to the Modern Standard Italian system. However, Mussafia does not really explain how and why this change took place. It is not the aim of this paper to answer this difficult question. Nonetheless, we should mention an interesting hypothesis put forth by Weil (1844): that is, the general rhythm of the language has evolved from descending to ascending. From this point of view, Italian has evolved from End Rule Initial to End Rule Final. Therefore, this change would have shifted from enclisis to proclisis. This hypothesis is attractive, but it deserves a more detailed analysis than can be carried out here. Mussafia's crucial point is the synchronic manifestation of the constraints mentioned above. That is, the second position of the clitic in expressions like *Non lo fare!* is a relic of the old constraint on the clitic order. In uassafia's own words, 'We find yet a relic of the old use when the negation *Non* precedes the verb; the ancients said: *Non lo ajutate* for the same reason that they said *Or lo ajutate*' (Mussafia 1886).

It must be recalled that from a Wackernagel-Mussafia perspective, the constraint on the placement of clitics in Old Italian and in other Indo-European languages has an essentially phonological *raison d'être*. In other words, clitics are phonologically weak elements which need a host. If we accept the hypothesis that Old Italian had a descending
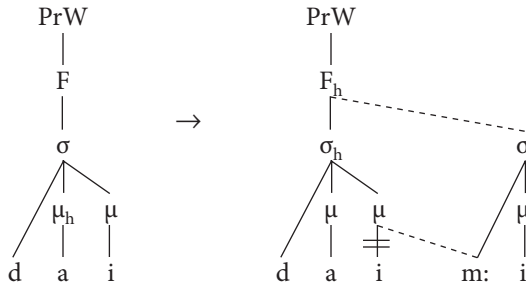
Figure 4. *Gemination of the initial consonant of the clitic* mi *when attached to a monosyllable imperative.*

rythm, enclisis at the beginning of the sentence therefore naturally follows. Evidently, Modern Standard Italian has maintained and even generalized enclisis in the imperative because once again, the imperative is a focused form. Now the enclitic imperative forms show a very interesting phonological pattern: when attached to a monosyllabic imperative, the initial consonant of the clitic is systematically geminated (Figure 2A, a portion of which is here repeated as part of Figure 4). In the imperative forms of the verbs *dare* 'to give', *fare* 'to do', and *stare* 'to stay', gemination indeed is the result of a delinking–relinking process.

We therefore have a change from a moraic trochee to a syllabic trochee; but in any case, the trochaic pattern is preserved. The clitic is thus incorporated into the foot of which it constitutes the weak element. That the imperative forms *da', fa', sta', va', di'* are truncated is confirmed by the other imperatives of Italian as well as those of other languages. In fact, crosslinguistically, imperatives are often truncated forms. Now in the case of *Stammi!, Fammi!, Dammi!, Vammi!, Dimmi!,* the question is why gemination is preferred to, say, vowel lengthening or simple adjunction to the base form? In other words, why don't we find something like *Daimi!* ['dajmi] or *Dami!* ['da:mi]? The question is all the more interesting since forms like *Pregailo* [pre'gajlo] 'I prayed him' are usual in Old Italian. Actually, there is a bundle of convergent criteria which results in gemination. As mentioned, gemination can be considered a cue for the metrification of the clitic at the foot level ; in a parallel fashion, the final syllable of the present indicative forms *sanno, danno, stanno, fanno* mustn't be considered extrametrical, but indeed forms part of the foot. Moreover, the syllabic trochee is the unmarked foot of the Italian metrical system. Therefore, the construction of a syllabic trochee converges on the unmarked. It should also be pointed out that in standard Italian, vowel lengthening occurs in Stressed Penult syllables only: in other words, Italian absolutely prohibits long vowels in stressed final syllables. Now in the case of words like *Dai!*, the head of the falling diphthong becomes final when the glide is deleted; therefore, it cannot be lengthened. The same holds for old forms like *mostrommi*, in which the initial consonant of the clitic is lengthened. We should at this point mention the illuminating analysis of Schuchardt (1874: 14):

Toute voyelle finale accentuée en Italien est brève. Par conséquent, si l'on ajoute par exemple à **andrò** l'enclitique **vi,** pour que la voyelle **o** conserve sa quantité, il faut que le **v** s'allonge et que le groupe devienne **androvvi,** car on ne peut pas avoir de voyelle brève dans aucune syllabe accentuée et ouverte, à la seule exception de la syllabe finale.

(Every stressed final vowel is short in Italian. Therefore, if for instance we add to *andrò* the enclitic *vi*, the *v* must be lengthened for the vowel *o* to maintain its quantity, thus resulting in the group *androvvi*. For we cannot have a short vowel in any stressed and opened syllable, apart from the final syllable.)

Schuchardt's account rightly insists on the importance of the constraint which in Italian prohibits word-final long vowels: vowel lengthening essentially occurs in word-internal penultimate stressed syllables. Now the conflict between the constraint which prohibits word-final long vowels and the one which imposes bimoraicity on (penultimate) stressed syllables probably is a key aspect of the so-called phenomenon of Raddoppiamento Sintattico.

6. TO SUM UP, I have argued that the double possibility of enclisis and amphiclisis in negative imperative is the result of the interaction of various constraints. First, the infinitive is semantically neutral and can be associated with a set of contextually ascribable values. Second, clitics are phonologically weak elements which need a host. In expressions like *Non lo fare!* we are dealing with a rhythmically optimal configuration, as we find a harmonic sequencing of Strong and Weak positions. At the same time, it is worth noting that the clitic stands in a position it filled systematically in Old Italian. From this point of view, the second position of the clitic can be considered a relic of a constraint systematically observed in Old Italian.

---

[1]   I would like to thank Lucia Molinu and especially Prof. Larry Hyman for their comments on earlier versions of this paper. Needless to say, I am solely responsible for any shortcomings.

[2]   According to Chomsky (1995:198), '***Agr** is a collection of φ-features (gender, number, person). …French-type languages have 'strong' Agr, which forces overt raising; and English-type languages have 'weak' Agr, which blocks it'.*

[3]   Kayne (1992) mentions Rizzi's observation that in the course of acquiring Italian, his son passed through a stage in which he produced sequences like *Non lo farlo!*. Various Italian dialects also show this kind of clitic reduplication. This (complex) phenomenon, however, deserves a much more detailed account than can be carried out here.

[4]   The OCP (Obligatory Contour principle) was introduced by Bantuists to account for dissimilation phenomena involving tones.

[5]   As Peškovskij (1956:131) puts it, 'Like the nominative case…, which is regarded as a simple, naked name of the object, without any of the complications in the thought process which are introduced by the forms of the oblique cases, so the infinitive, because of its abstract nature, appears to be a simple, naked expression of the idea of action, without those com-

plications which are introduced into it by all the other verbal categories' (quoted from Žirmunskij 1966:79).

6   This analysis also recalls that of Heidegger (1952:77):

'L'infinitif, tel qu'il est entendu dans l'appellation latine, est une forme de mot qui, pour ainsi dire, coupe ce qui est signifié en elle de tout rapport significatif déterminé. La signification est détachée (abstraite) de tout rapport particulier. En raison de cette abstraction, l'infinitif se borne à rendre ce qu'on se représente d'une façon générale dans le mot. C'est pourquoi on dit dans la grammaire actuelle: l'infinitif est le «concept verbal abstrait». Ce à quoi on pense, l'infinitif se contente de le saisir et concevoir abstraitement et en général. Il désigne uniquement cette idée générale. Dans notre langue l'infinitif est la forme d'appellation du verbe. Dans la forme de l'infinitif, et dans la signification qu'elle fait apparaître, réside un manque, un défaut'.

7   Further evidence for the metrification of the clitic at the foot level comes from 'dactylic shortening'; as a matter of fact, the stressed vowel of a trochaic form like *posa* ['pɔːza] 'put!' is lengthened, while the stressed vowel of the enclitic form *posalo* ['pɔzalo] 'put it!' is shortened when a clitic is added. Interestingly, the resulting dactyl recalls the structure of verbal forms like *posano* ['pɔzano] 'they put', in which the addition of the third person plural morpheme *-no* triggers the same 'dactylic shortening'.

8   Recall that according to Nespor amd Vogel (1989), adjacent *'s on the first three levels of the grid represent minimal clash in Italian.

```
*    *    word
*    *    foot
*    *    syllable
p    p
```

Therefore, (a) is an instance of a clashing configuration, but not (b) (c) or (d):

```
(a)    *   *          (b)    *
       *   *                 *   *
       *   *                 *   *
    Sarà  forse partito   Sarà   ritornato
    'he may have left'    'he will have returned'


(c)    *             (d)    *        *
       *                    *        *
    *   *                 *    *   *
    Avrà mangiato        Quattro grandi libri
    'he will have eaten'  'four big books'
```

REFERENCES

ANDERSON, STEPHEN. 1993. Wackernagel's revenge: clitics, morphology, and the syntax of second position. *Language* 69:68–98

BELLETTI, ADRIANA. 1990. *Generalized verb movement: aspects of verb syntax.* Torino: Rosenberg & Sellier

CHOMSKY, NOAM. 1995. *The minimalist program.* Cambridge MA: MIT Press.

GRAFFI, GIORGIO. 1996. Alcune riflessioni sugli imperativi italiani. In *Italiano e dialetti nel tempo. Saggi di grammatica per Giulio C. Lepschy*, ed. by Paola Benincà, Guglielmo Cinque, Tullio de Mauro & Vincent Nigel, 133–48. Roma: Bulzoni Editore.

GUILLAUME, GUSTAVE. 1929. *Temps et verbe. Théorie des aspects, des modes et des temps*. Paris: Champion.

HEIDEGGER, MARTIN. 1952. *Introduction à la métaphysique.* Paris: Gallimard.

KAYNE, RICHARD. 1992. Italian negative infinitival imperatives and clitic climbing. In *De la musique à la linguistique. Hommages à Nicolas Ruwet*, ed. by Liliane Tasmowsky & Anne Zribi-Hertz, 300–12. Ghent: Blandijnberg.

LAUSBERG, HEINRICH. 1971. *Linguistica Romanza. II. Morfologia.* Milano: Feltrinelli. (Italian translation of *Romanische Sprachwissenschaft*. Berlin, 1956)

LEVI, PRIMO. 1958. *Se questo è un uomo.* Torino: Einaudi.

MORAVIA, ALBERTO. 1988. *La Romana.* Milano: Bompiani

MUSSAFIA, ADOLFO. 1886. Una particolarità sintattica della lingua italiana dei primi secoli. *Miscellanea di filologia e linguistica in memoria di Napoleone Caix e Ugo Angelo Canello*, 255–61. Firenze: Le Monnier.

NESPOR, MARINA. 1993. *Fonologia.* Bologna: Il Mulino.

—— & Irene Vogel. 1989. On clashes and lapses. *Phonology* 6:69–116.

PEŠKOVSKIJ, A. M. 1956. *Russian syntax in a scientific light.* Moscow. (Quoted, but with incomplete citation, in Žirmunskij 1966.)

SERIANNI, LUCA & ALBERTO CASTELVECCHI. 1989. *Grammatica italiana. Italiano comune e lingua letteraria.* Torino: UTET Libreria.

SCHUCHARDT, HUGO. 1874. Phonétique comparée. *Romania* III:1–30.

SELKIRK, ELIZABETH. 1995. Sentence prosody: Intonation, stress, and phrasing. In *The handbook of phonological theory*, ed. by John Goldsmith, 550–69. Oxford: Basil Blackwell.

SKYTTE, GUNVER. 1983. *La sintassi dell'infinito in italiano moderno*. Copenhagen: Munksgaard.

WACKERNAGEL, JACOB. 1892. Über ein Gesetz der indogermanischen Worstellung. *Indogermanische Forschungen* I:333–436

WEIL, HENRI. 1844 (reprint edition 1991). *De l'ordre des mots dans les langues anciennes comparées aux langues modernes. Question de grammaire générale.* Paris: Didier Erudition.

ZANUTTINI, RAFFAELLA. 1991. *Syntactic properties of sentential negation. A comparative study of Romance languages*. Philadelphia: Institute for Research in Cognitive Science, University of Pennsylvania.

ŽIRMUNSKIJ, V. M. 1966. The word and its boundaries. *Linguistics* 27:65–91.

❧

LANGUAGE SHIFT IN PROGRESS: EVIDENCE FROM MANDARIN CHINESE/ENGLISH CODESWITCHING

LIWEI GAO
University of Illinois at Urbana-Champaign

THIS PAPER[1] presents preliminary findings of a research project that investigates the linguistic consequences of the contact between Mandarin Chinese and English in the United States. The paper focuses on the analysis of language shift in the Chinese children who recently came to the United States. By examining the data of codeswitching (henceforth CS)[2] collected from three Chinese children and their parents, the paper demonstrates that CS by the children is subject to fewer structural and functional constraints (e.g., Pandharipande 1990, Singh 1985, Sridhar & Sridhar 1980)[3]. Based on such evidence the paper proposes that the children are undergoing language shift, viz., Mandarin Chinese, the children's mother tongue, is being gradually replaced by English as the primary means of communication and socialization.

The paper first provides the theoretical framework for this research. Then it makes a brief introduction to the demographic background of the community in which the study was conducted, followed by the methodology of data collection. The bulk of the paper, data analysis, is presented next. The paper also includes a brief illustration of the factors that caused the children's language shift. And it concludes by pointing out, on the one hand, the contribution it attempts to make, and on the other, its potential weakness.

1. THE THEORETICAL FRAMEWORK. Since the publication of Fishman's seminal work on language shift in 1964, research on language attrition and shift in the bilingual or multilingual context has mushroomed (e.g., Brenzinger 1992, Paulston 1994). Research on the fate of linguistic minorities in multicultural settings, among which is the attrition and shift of the migrated language, has also been well documented (e.g., Extra and Verhoeven 1999, Paulston 1994).

The mechanism of language shift is multidimensional. CS is among such machinery. As Myers-Scotton (1992) points out, CS, with a shift in the host language, is an evident mechanism for language shift. Similarly, Schjerve (1998) also observes that CS may lead to the change in language in that

> (a) [CS] facilitates the functional-pragmatic switch to the dominant language and (b) [CS] frequently mediates change in the socially non-dominant language, potentially leading to convergence or even language death.

In light of the evidence from CS, this study shows that the Chinese children's command of their native language, Mandarin Chinese, is waning. For one thing, compared with the adults, the children used the mixed code of Chinese and English in more domains (Fishman 1964, 1968). For another, in the children's CS the matrix language, Mandarin Chinese, permits the permeation of broader categories of English items, both lexical and grammatical, to the extent that it loses its host language status.

Although evidence shows that English is replacing Chinese as the children's primary language, it is unlikely that the children will lose their ability to use Chinese completely. In other words, both Mandarin Chinese and English are going to be used by these three subject children, and most probably by their own children. In this sense, the term language shift in this study does not denote *complete* language displacement, or even language death[4].

2. THE DEMOGRAPHIC BACKGROUND. This study was conducted in a community that is located in a college town in the Midwestern United States. Since more than ten percent of the university enrollees are international students, the population in this town is notably diverse in terms of nationality. In other words, the town is culturally multifarious where the community under study finds itself. Even so, the absolute majority of residents in this town are still United States citizens. Consequently, English is the dominant language in most settings of social life. Other languages, including Mandarin Chinese, are linguistic minorities.

The subjects lived with family in one of the university apartment complexes for graduate students. There are approximately forty such complexes in the community under study. Since nearly one third of the residents in these apartment complexes are Chinese, this neighborhood is dubbed 'Little Chinatown'. There were altogether around 1,000 Chinese living in this area at the time of data collection.

Each of the three Chinese families under study comprised two parents and one child. The parents were between thirty-three and thirty-nine years old. The male parents in all these families were graduate students at the university in town. One of the female parents was also a graduate student at the same university, and the other two were part-time students at a local community college. At the time of data collection, the parents had been in the United States for about four years. And the child subjects, two boys and one girl, had been in the United States for approximately three and a half years. Two of the children were nine years old and the third was eleven. They had all been enrolled in a local elementary school for more than two years.

3. THE METHODOLOGY. The data corpus of this study includes one hundred and fifty Chinese-English code-switched sentences that start in Mandarin Chinese. Among them seventy sentences were produced by the adults and eighty by the children.

The data were collected primarily through participant observation, which was carried out from January through May 2000. The children were observed mainly in three different domains: at school, on the playground, and at home. The parents were also observed primarily in three different settings: at school, which covers sports

facilities and recreational centers, in grocery stores, and at home. The choice of these domains helped to ensure that the children and the adults would be observed mostly separately, which, in turn, precludes the possibility that the data are seriously compromised by the parents' potential agenda that encourages the children to speak Chinese or, alternatively, English.

The principal means of data recording was note taking. In order to minimize possible bias associated with note taking, a tape-recorder was also employed for some sessions of the observation, particularly for the recording of conversations among the family at home. If conversations were recorded, the recordings were transcribed on the same day as they were made.

To supplement the participant observation data with information collected in a more systematic, though undoubtedly less natural, manner, a questionnaire (cf. Appendix[5]) was also designed for this study. The questionnaire consists of four sets of questions, totaling 64 questions. The first set of questions requires the subjects to choose the domain(s) in which they have ever code-switched. It also requires them to specify other possible settings of CS that are not given. The second set requires the subjects to express a specified idea, want, or feeling in the manner with which they are the most comfortable. The third set requires the subjects to choose the preferable sentence from pairs of code-switched and non-switched sentences. The last set requires the subjects to judge the acceptability of sentences code-switched in different ways.

4. THE DATA ANALYSIS. The study first compares the code-switched sentences actually uttered by the children with those produced by their parents. It then examines the data elicited by means of the questionnaire, which contain the information about the potential as well as actual employment of CS. The result of the data analysis indicates that although the children's CS behavior resembles that of the adults', there also exist patterns of difference concerning both the functional domain and structural constraints on CS.

4.1 THE COMMONALITY BETWEEN CHILDREN'S AND ADULTS' CS. The children and the adults share a number of features in their CS behavior. Since the focus of this paper is on the discrepancies, not all the observed common characteristics are discussed below.

As seen in examples (1-2), most of the English elements used in both the children's and the adults' CS were nouns or noun phrases. The Chinese equivalents of these expressions are either cumbersome or difficult to find, for example, *to go* (as is used in the restaurant) in (1), which was uttered by the child, and *time-share* in (2), which was used by the adult. For convenience in reading, in all the examples given in this paper Chinese expressions are presented in italicized *pinyin* ('the Chinese phonetic alphabet') instead of in characters. The expressions in English are boldfaced.

(1) *Wǒ bù   xiǎng* **to go**. *Wǒ xiǎng zài   nà'er chī.*
     I   not   want to go   I   want PREP there eat
     'I don't like "to go". I want to eat there [at the restaurant].'

(2) *Shuí   mǎi*   **time-share**? *Nàmo guì*.
    who   buy   time-share   so   expensive
    'Who'd like to buy time-share? So expensive.'

In addition, both the children and the adults usually code-switched to English when
they started to use a proper name that is associated with the United States, for exam-
ple, a US product, a US basketball team, or the name of an American professor. Two
of such CS are given in examples (3)–(4) below.

(3) *Wǒ jīběnshang bù   zěnmo kàn*   **NBA**.
    I   basically   not often   watch   NBA
    'I don't watch NBA games so often.'

(4) *Míngtiān nǐ   qù bù   qù* **Super K**?
    tomorrow   you   go   not   go   Super K
    'Are you going to Super K tomorrow?'

Example (5), which was uttered by the child, and (6), produced by the adult, indicate
that the children and the adults also both switched to English verbs or adjectives in
their CS, although such instances are far less frequent than switching to nouns. In (5)
the child used the verb *ground*, and in (6) the adult used the adjective *competitive*.
Again, the Chinese counterparts of these expressions are either burdensome or dif-
ficult to find.

(5) *Wáng Zhé bèi   tā bà*   **ground** *le*.
    Name   PASSIVE   his   father   ground   PERFECTIVE
    'Wang Zhe was grounded by his father.'

(6) *Tā   lǎoshī   shuō tāde chéngjì háisuàn* **competitive**.
    his   teacher   say   his   score   close to   competitive
    'His teacher said that his score is close to being competitive.'

Examples (1)–(6) together point to another important common feature between the
children's and the adults' CS. That is, in all these instances English elements fit in
well with the syntactic requirements of the matrix language, Mandarin Chinese. This
type of formal cohesion (Kachru 1983) is also reported by An (1985), which seems to
confirm Poplack's (1980) Equivalent Constraint that switching should occur at places
where the syntactic structures of the matrix and the embedded language match.

   In terms of the functional domain of CS, both the children and the adults were
found to have code-switched in all the identified broad functional domains, for exam-
ple, in schools and at home.

4.2. THE DIFFERENCE BETWEEN CHILDREN'S AND ADULTS' CS

4.2.1. THE FEWER STRUCTURAL CONSTRAINTS ON CHILDREN'S CS. Examination of the data demonstrates that, in one way or another, the children's CS is subject to fewer formal constraints than their parents'. Some of the relaxed constraints are exemplified in (7)–(14) below.

Examples (7)–(8) suggest that for the children switching from an English item, be it a verb or noun, to a Chinese particle is quite possible. In example (7) the child switched from an English main verb to the Chinese aspectual marker *le*, which indicates the perfection of an action. In example (8) the child switched from an English noun to the Chinese particle *ne*, which is used in this case to help establish a fact. In contrast, such switching is virtually impossible for the adults.

(7) *Māma   yǐjīng*   **sleep**   *le.*
    mum   already sleep   PERFECTIVE
    'Mum has already gone to sleep.'

(8) *Wǒ   hái   méiyǒu   zuò*   **proofreading**   *ne.*
    I   yet   not   do   proofreading   PARTICLE
    'I haven't yet done the proofreading.'

Examples (9)–(10) show that system morphemes in English—function words and inflections, which in this case mainly involve the complementizers *if* and *that*, also occur in the children's Chinese-English CS. It should be borne in mind, however, that such deep borrowing (Myers-Scotton 1992) in the children's CS is not as extensive as the switching to lexical items. In contrast, switching from Chinese elements to English system morphemes is not observed in the adults' CS.

(9) *Tā   māma   bù   zhīdào* **if** *tā   wánchéng   le   zuòyè.*
    his   mother not know   if   he   finish   PERFECTIVE assignment
    'His mother didn't know if he had finished his assignments.'

(10) *Dàn   tā   māma   xiāngxìn*   **that he's a good boy**.
     but   his mother believe   that he's a good boy
     'But his mother believed that he is a good boy.'

Another related difference between the children's and the adult's CS is that in the adults' CS, no English bound morpheme was switched together with a lexical item. In contrast, the bound morpheme was used together with the lexical item in the children's CS. Examples of this type are in (11)–(12), produced respectively by the adult and the child. Although both types of switching satisfy Poplack's Equivalent Constraint, for the children the syntax within the boldfaced noun phrase is English, while for the adults it is still Chinese, given that English generally marks plurality morphologically,

while Chinese does not. However, this does not mean that it is possible for the children to switch from Chinese directly to an English bound morpheme.

(11) *Zhè   xuéqī nǐ   yào mǎi duōshǎo* **book**?
      this   term   you   need buy how many book
      'How many books do you need to buy this term?'

(12) *Wǒ yǐjīng   zuòwán jīntiān de*   **assignments** *le*.
      I   already   finish today   MODIFIER assignments   PERFECTIVE
      'I've already finished today's assignments.'

Examples (13)–(14) below, where the matrix language in the code-switched sentence is English rather than Chinese, constitute one more piece of evidence that English is replacing Chinese as the children's preeminent language. Myers-Scotton (1992:49) calls such a change, the change from the status of a guest to a host language, the '"outside goes to inside" change'. She further offers a metaphor to describe this situation, recalling 'the Russian fable of the wolf which ate the sleigh-horse and thereupon found itself in harness as a horse-substitute' (citing Denison 1977:21). Yet again, no sentences code-switched in this manner are found to have been produced by the adults.

(13) *Tā   céngjīng* **suffered from pneumonia**.
      he   ever   suffered from pneumonia
      'He's suffered from pneumonia before.'

(14) *Búshì* **everybody likes this teacher**.
      not   everybody like   this teacher
      'Not everybody like this teacher.'

In (13) the only two words in Chinese are the subject of the sentence *tā* 'he' and the sentential adverb *céngjīng* 'ever'. And in (14) the only Chinese element is the sentence-initial negation adverb *búshì* 'not'. In a sentence that features CS, the criteria for determining the host language status are basically twofold: 1) the proportion of words expressed in a language; and 2) the syntactic functions that these words constitute[6]. Based on either of these two criteria, English is definitely the matrix language in sentences (13)–(14).

4.2.2. THE BROADER FUNCTIONAL DOMAINS FOR CHILDREN'S CS. The data analysis also indicates that, compared with the adults, the children code-switched in more functions, even though both the parents and children code-switched in all the identified broad domains. In other words, within these domains the children used CS in more sub-domains. For instance, CS was both reported to have taken place and found in children's squabbles at home or at the playground, but no such linguistic behavior

was either found or reported to have occurred in the adults' quarrels. Examples in this domain are provided in (15)–(16).

(15)   *Nǐ*   *búyào*   **blame** *wǒ*!   **I did nothing wrong!**
       you   may not blame me   I did nothing wrong
       'Don't blame it on me! I did nothing wrong!'

(16)   *Nǐ*   *wèishénmo bù gěi wǒ mǎi*   **roller skates!?**   *Wǒde péngyǒu dōu yǒu*!
       you why   not for me buy roller skates   my   friends all   have
       'Why don't you buy me a pair of roller skates!? My friends all have them!'

In example (15) the child switched to the English word *blame* instead of using its Chinese counterpart *pīpíng* 'criticize'. In example (16) the child substituted the English word *roller skates* for the Chinese equivalent *bīngxié* 'roller skates'. More significantly, in (15) the second sentence 'I did nothing wrong' is completely in English.

Labov (1970) notes that in an interview situation when the subject or topic creates strong emotions in the interviewee, he or she will shift from careful speech towards the vernacular, the type of speech that the interviewee feels most at ease with. Similarly, when the children were engaged in heated arguments or quarrels, they were unlikely to be aware of, not to mention be selective about, their language. Hence, the variety of code that they used in (15)–(16) is likely to be the one that they are the most comfortable with. In this sense, it is clear that the children's mother tongue, Mandarin Chinese, is starting to decay, since the children cannot even produce a complete sentence in Chinese. More significantly, the decline is not confined to the lexicon, as is evidenced by the second sentence in example (15).

One more strong piece of evidence that supports the claim that the three Chinese children are experiencing language shift is that in any transcribed tape-recorded conversation among the children and their parents in the domain of home, CS by the children occurred much more frequently than by the adults, if the adults code-switched at all. Although this study focuses on a qualitative analysis, the quantitative evidence would also be meaningful.

5. THE SOCIAL AND INTERNAL FACTORS FOR LANGUAGE SHIFT. As Gal (1979) points out, language shift is an instance of socially determined linguistic change, which involves the redistribution of communicative forms over functions in everyday interaction. Kulick (1992) also notes that language shift is a reflection of cultural change. In the case of these three Chinese children, then, it is only natural that when they moved from an Oriental to an Occidental culture that is predominantly expressed in English, their primary language would change, especially when their ability to acquire another language is still strong.

More specifically, in the college town under study overall social factors are conducive to the Chinese children's language shift. Most critically, these children attend schools where they receive education in English. Furthermore, many of the children's

friends, both at school and at the playground, are from countries other than China. To communicate with these friends, the children may only use English. Although at home these children mostly speak Chinese with their parents, this fact may not successfully compete against the influence from the predominant use of English elsewhere in the children's language development. Other factors, such as sending the children to the Chinese School, where they study Chinese over weekends, cannot prevent the language shift from taking place, either.

Le Page and Tabouret-Keller (1985: 14) view 'linguistic behavior as a series of *acts of identity* in which people reveal both their personal identity and their search for social roles'. In this sense, if the Chinese children attempt to create a modern identity, for example, an American identity, they will naturally choose to use more English. Wei (1994) also documents the choice of a certain language by the British Chinese to reveal a certain identity. Such an internal motivation, combined with the pressure from the external social context, constitutes a strong determinant for Mandarin Chinese to lose its primary language status to English in the three Chinese children.

6. CONCLUSION. The decay of migrant languages has been well studied. Nevertheless, there is a gap in terms of research on the fate of Mandarin Chinese as a migrating language. This study attempts to help fill this gap. Furthermore, the overall research on the linguistic consequences of the contact between English and Mandarin Chinese, the two languages with the largest number of speakers, seems to be insufficient. This study also attempts to contribute to this field of inquiry[7]. Given the fact that this study is in nature an apparent time study (Chambers 1995), the children's attrition of Chinese and shift toward English might be difficult to perceive or describe. If the children's CS behavior is studied longitudinally, their language shift can be more easily appreciated.

---

[1] I would like to express my gratitude to Rajeshwari Pandharipande and Rakesh Bhatt for their helpful comments and guidance in the course of my preparing this paper. I am also grateful to those who offered helpful comments and suggestions at the LACUS conference.

[2] Precisely speaking, intrasentential codeswitching is referred to as code-mixing. In this paper codeswitching is used as a cover term for both inter- and intra-sentential codeswitching.

[3] Bokamba (1989) demonstrates that most proposed syntactic constraints on codeswitching and code-mixing are cross-linguistically invalid. Even so, there do exist syntactic constraints on codeswitching and code-mixing. The constraints proposed provide helpful references for the study of codeswitching involving different languages.

[4] The term language shift is sometimes used with two slightly different denotations. Fishman (1964) defines it as the replacement of one language by another in certain domains, i.e., the substitution of one language for another as the chief means of communication (Mesthrie et al. 2000). According to some other researchers (e.g., Fasold 1984), language shift means that a community completely gives up a socially dominated language to a dominating one. In this paper, language shift is meant to have the interpretation as in Fishman (1964).

[5] The sample questionnaire in the appendix does not represent the actual form of the questionnairre given to informants. It is instead an adaptation for this paper, with some

of the questions (e.g., the first question) somewhat abstracted. The aim in providing this appendix is to show what sort of data were being solicited.

6   For a detailed discussion of the definition for the matrix and the embedded language, see Myers-Scotton (1993). Kamwangamalu and Lee (1991) is a discussion of the matrix language assignment specifically associated with Chinese-English codeswitching.

7   Hsu (1994) is a comprehensive study of the influence upon the Chinese from the English language. Zhou and Feng (1987) includes a good discussion of the influence of Chinese language and culture upon English used in China.

APPENDIX: SAMPLE QUESTIONNAIRE

1.   In which of the following domains have you ever code-switched between Chinese and English?
    1)   at home
    2)   at school
    3)   in other settings (please specify)

2.   What would you say to express the following ideas, wants, or feelings?
    1)   This is a beautiful apartment.
    2)   Please tell me the meaning of this idiom.
    3)   There are *so* many interesting books here!
    4)   Anyway, in-class participation will also be taken into account to decide your final score.
    5)   Like father, like son.

3.   There are at least two ways in which the following ideas or questions can be expressed. Which one do you prefer? (Expressions in italics are Chinese *pinyin*; English expressions in the codeswitched sentences are in bold.)
    1)   That professor is very nice.
        a.   *Nage jiaoshou ren hen hao.*
        b.   *Nage jiaoshou hen* **nice**.
    2)   Is this dish good?
        a.   *Zhedao cai haochi ma?*
        b.   *Zhege* **dish** *haochi ma?*
    3)   Anyway, let's eat.
        a.   *Buguan zenmeshuo, xian chifan ba.*
        b.   **Anyway**, *xian chifan ba.*
    4)   Are we going shopping tomorrow?
        a.   *Mingtian women qushangdian maidongxi ma?*
        b.   *Mingtian women* **go shopping** *ma?*
    5)   Two heads are better than one.
        a.   *Liangren zhihui sheng yiren.*
        b.   *Two heads are better than one.*

4.   Are the following expressions acceptable to you? (Expressions in italics are Chinese *pinyin*; English expressions in the codeswitched sentences are in bold.)
    1)   *Ta*   *tiantian*   *shuo*   **tired**.
        he   every day   say   tired
        'Every day he says that he's tired.'

2) *Ba*, *wo* **sleepy** *le*!
   dad I   sleepy Particle
   'I'm sleepy, Dad.'

3) **Swimming** *dangran   bucuo*.
   swimming certainly   good
   'Swimming is, of course, good.'

4) *Suoyou de*   **performance is considered in deciding your final grade**.
   all   PARTICLE performance is considered in deciding your final grade
   'All kinds of performance are considered in deciding your final grade.'

5) *Wo bu xiangxing* **that he'll be elected President**.
   I   not believe   that he'll be elected President
   'I don't believe that he'll be elected President'.

REFERENCES

AN, WU-YI. 1985. Code-mixing by English-Chinese bilingual teachers of the People's Republic of China. *World Englishes* 4:303–17.

BOKAMBA, EYAMBA. 1989. Are there syntactic constraints on code-mixing? *World Englishes* 8:277–92.

BRENZINGER, MATTHIAS. (ed.) 1992. *Language death: factual and theoretical explorations with special reference to East Africa*. New York: Mouton de Gruyter.

CHAMBERS, JACK. 1995. *Sociolinguistic theory*. Oxford: Blackwell Publishers.

DENISON, NORMAN. 1977. Language death or language suicide? *International journal of the sociology of language* 12:13–22

EXTRA, GUUS & LUDO VERHOEVEN. (eds.) 1999. *Bilingualism and migration*. New York: Mouton de Gruyter.

FASOLD, RALPH. 1984. *The sociolinguistics of society*. Oxford: Basil Blackwell.

FISHMAN, JOSHUA. 1964. Language maintenance and language shift as fields of inquiry: a definition of the field and suggestions for its further development. *Linguistics* 9:32–70.

———. 1968. Sociolinguistic perspective on the study of bilingualism. *Linguistics* 39:21–49.

GAL, SUSAN. 1979. *Language shift: social determinants of linguistic change in bilingual Australia*. New York: Academic Press.

HSU, JIA-LING. 1994. *Language contact and convergence: Englishization of Mandarin Chinese*. Urbana IL: University of Illinois at Urbana-Champaign dissertation.

KACHRU, BRAJ. 1983. *The Indianization of English: the English language in India*. New Delhi: Oxford University Press.

KAMWANGAMALU, NKONKO M. & CHER-LENG LEE. 1991. Chinese-English code-mixing: a case of matrix language assignment. *World Englishes* 10:247–61.

KULICK, DON. 1992. *Language shift and cultural reproduction: socialization, self, and syncretism in a Papua New Guinean village*. Cambridge: Cambridge University Press.

Labov, William. 1970. The study of language in its social context. *Studium generale* 23:30–87.

Le Page, R.B. *&* Andree Tabouret-Keller. 1985. *Acts of identity: Creole-based approaches to language and ethnicity*. Cambridge, England: Cambridge University Press.

Mesthrie, Rajend, Joan Swann, Andrea Deumert *&* William Leap. 2000. *Introducing sociolinguistics*. Philadelphia: John Benjamins.

Myers-Scotton, Carol. 1992. Codeswitching as a mechanism of deep borrowing, language shift, and language deaths. In Brenzinger 1992:31–58.

———. 1993. *Social motivations for codeswitching: evidence from Africa*. Oxford: Clarendon Press.

Pandharipande, Rajeshwari. 1990. Formal and functional constraints on code-mixing. In *Codeswitching as a worldwide phenomenon*, ed. by Rodolfo Jacobson, 15–32. New York: Peter Lang.

Paulston, Christina B. 1994. *Linguistic minorities in multicultural settings*. Amsterdam: John Benjamins.

Poplack, Shana. 1980. Sometimes I'll start a sentence in Spanish y termino en español: toward a typology of code-switching. *Linguistics* 18:581–618.

Schjerve, Rosita R. 1998. Codeswitching as an indicator for language shift?: evidence from Sardinian-Italian bilingualism. In *Codeswitching worldwide*, ed. by Rodolfo Jacobson, 221–47. New York: Mouton de Gruyter.

Singh, R. 1985. Grammatical constraints on code-mixing: evidence from Hindi-English. *Canadian journal of linguistics* 30:33–45.

Sridhar, S.N. *&* Kamal Sridhar. 1980. The syntax and psycholinguistics of bilingual code mixing. *Canadian journal of psychology* 34:407–16.

Wei, Li. 1994. *Three generations, two languages, one family: language choice and language shift in a Chinese community in Britain*. Philadelphia: Multilingual Matters Ltd.

Zhou, Zhipei *&* Wenchi Feng. 1987. The two faces of English in China: Englishization of Chinese and nativization of English. *World Englishes* 6:111–25.

❧

RECONSTRUCTING OGAM *P

Toby D. Griffen
Southern Illinois University Edwardsville

THE PHONOLOGY OF THE CELTIC LANGUAGES is marked by the disappearance of Indo-European /p/, as in Old Irish *athir* 'father' corresponding to Latin *pater*. This proceeded through stages first with the change from /p/, possibly through /ɸ/ (McCone 1996:44), to /h/ or /χ/ (Lewis and Pedersen 1973:27), and then finally to null. Within Celtic, some dialects developed a new /p/ from /kʷ/, as seen in such correspondences as Middle Welsh *pymp* 'five' and Old Irish *cóic* (compare Latin *quinque*).

1. OGAM. The earliest attested form of the ogam (or ogham) writing system consisted of strikes across or emanating from the vertical edges of stone monuments in the British Isles beginning in the fifth century. Typically, it was inscribed from bottom to top, leading to the traditional rendition of the signary in Table 1 (overleaf).

The conventional Roman representation is given to the right of the sign with a capital letter. The broad phonetic values are largely those determined by McManus (1991:36–39, with one adjustment, Z, reflecting ongoing research).

There are three central points of general agreement within the profession on the ogam writing system that will be of importance here. These regard the origin of the system, the phonetic basis of the signs, and the dialect variation of Celtic.

The signary was quite transparently derived from some sort of tally system (compare, for example, Gerschel 1962 and McManus 1991:14–15), and such tally systems composed of strikes across a line are in evidence in the British Isles from the Upper Palaeolithic (as illustrated in Barham et al. 1999:80, 102). Given recent DNA evidence for the stability of the population from the Palaeolithic (Barham et al. 1999), the fact that this type of tally system appears and reappears is significant for the development of ogam, for it implies a consistent, persistent culture.

One of the most strongly held tenets in the study of ogam is that the various columns were arranged in accordance with phonetic principles. Of course, these principles would not necessarily have adhered to our current view of feature or componential phonetics; but they would have proceeded within the system along rational sound-related parameters. Within this vein, there is also concurrence that the motivation for setting the tally system down as a form of writing was probably influenced by contact with the Roman alphabet. Certainly, ogam's appearance on stone monuments has been seen as an imitation of Roman grave markers. Although once somewhat strongly held, the idea of a direct influence by such Roman grammarians as Donatus on the phonetic array of the system is now doubtful both from the array itself (McManus 1991:28) and from considerations of dating (Stevenson 1990:165). We must

▤	N	/n/	⪧	Q	/kʷ/	▩	R	/r/	▦	I	/i/
▤	S	/s/	⪧	C	/k/	▩	Z	/tˢ/	▦	E	/e/
▤	F	/w/	⪧	T	/t/	▩	GG	/gʷ/	▦	U	/u/
⊧	L	/l/	⊣	D	/d/	✶	G	/g/	✚	O	/o/
⊢	B	/b/	⊣	H	/h/	✛	M	/m/	✛	A	/a/

Table 1. *The ogam alphabetic signary.*

also bear in mind that while our earliest physical evidence of ogam is found on these stone monuments, the signs had already been incised for some undetermined length of time on wood, which has long since decayed.

Finally, there is growing agreement on the dialect diversity of Celtic throughout the entire region of Western Europe and in the British Isles themselves. The neat progressions of /p/ to null overall and of /kʷ/ to /p/ in precise regions have been severely contradicted by such evidence as place names. Indeed, evidence of original /p/ surviving relatively late in some dialects identified as Celtiberian (Rankin 1987:24) and a mixture of /kʷ/ and derived /p/ on the Coligny Calendar and throughout Gaul and Spain (see Rankin 1987:14, 23) indicate that the situation was far more fluid than we may have previously believed.

2. RECONSTRUCTING *P. With the ordering of the ogam signary in conjunction with the tally system, it should be rather clear that the phonetic value of each sign would have already been in use as a mnemonic device for identifying the numerical value. That is, a reference to ✛ would have been made by using a word beginning with A, and a reference to ✛ by using a word beginning with M, and so forth. In fact, there are sets of words and kennings that were used in just this manner. Such sets would have been quite necessary for those changing the tally system into an alphabetic signary, so that others would readily understand which sound was being signaled.

While it is generally assumed that the ogam system of writing developed in Ireland, no stone inscription there contains the sign ⊣ or H (nor Z—Macalister 1945:v; and some contend that GG occurs too infrequently for precise phonetic determination—Gippert 1990:291). The word usually associated with the sign H is the Old Irish *hÚath* 'hawthorn' in which the initial /h/ had already been reduced to null. In all such cases as that of the mnemonic word, the reflex of /h/ would have been a silent grammatical entity and would not have been represented in the writing. Certainly as regards this sign, the phonetic value associated with the tally mark necessarily predates Irish.

One word associated as a kenning with H is Old Irish *úath* 'fear, horror' (compare also Merony 1949:28) corresponding to Latin *pavēre* 'to be terrified', a point made by Peter Schrijver and reported by McManus (1991:37). McManus, however, dismissed the speculation that H might have been derived from /p/, as that would have represented a linguistic situation much too early for the monumental inscriptions. On

the other hand, the monumental stone inscriptions followed a tradition of unknown duration in which the ogam inscriptions were carved on now-perished wood.

That ⊣ originally represented the sound /p/ is precisely what is argued here both from the internal structure of the ogam signary itself and from comparative evidence of Irish and Pictish.

2.1. INTERNAL EVIDENCE. Let us begin with an examination of the sounds in the ogam signary's array. Starting with the vowels, we note that the base value of the fourth column is A. The next two signs proceed quite rationally up in the back of the oral cavity from O to U. We then change to the front of the oral cavity and proceed in precisely the same manner from E to I. The idea that the first two values 'above' the base value progressed in one manner and that the next two progressed in another, but related manner is by no means new, having been pointed out, for example, by Carney (1975:54–61). Moreover, the A represents an extreme and unique position of articulation and the only point at which we may enter the vocalic triangle in the oral cavity in such a way as to effect the pattern in which two values follow two values.

The consonantal base values in the first and third columns are B and M, which are produced at the labial position of articulation. Parallel with the A in the vocalic column, these signs thus provide an articulatory base position that may be considered as a logical starting point to the oral cavity. This is one reason for reconstructing the value of ⊣ as *P—now all of the base values are phonetically consistent in a means that could have occurred to reasonably intelligent people at the time, as they represent readily identifiable positions.

Proceeding up the columns, we find that the *P is indeed a valid fit. Very briefly, the consonants appear to be grouped by a perception of 'hardness' and 'complexity'. The B column starts with the softest, most liquid pair and ends with somewhat harder continuants (with each pair marked or 'complicated' by a retraction of the tongue for the second member). The M column starts where the B column leaves off, with the nasal continuant relating to the voiced stop by complexity, which is made more complex in the continuant off-glide; and likewise, the single affricate is complicated in the continuant trill. The *P column also starts where the B column leaves off with a voiced stop which is hardened to the homorganic voiceless stop. Within the column itself, the same pattern continues with a voiced stop hardened to its homorganic voiceless counterpart; and the next voiceless stop is complicated with the off-glide.

Now, the soft labial B introduces the soft column, the hard labial *P introduces the hard column, and the complex labial M introduces the complex column, just as the base vowel A introduces the vowel column. This reconstructed *P thus fits the phonetic valuation of the system quite precisely and in a highly consistent pattern; the H or null value, on the other hand, clearly does not. Indeed, *P provides a consistent base value for its column, as it serves as a basis for the progression up the column in modern terminology, it shares 'distinctive features' both within its 'order' and within its 'series'.

2.2. COMPARATIVE EVIDENCE. Not only is the signary better served with the *P than with the H, but the reconstruction of *P fits into comparative evidence from Pictish, a Brythonic Celtic dialect grouping in which /kʷ/ had already changed to /p/ (see Forsyth 1997). We must recall, though, that the dialect situation among the Celts was not subject to clear geographical divisions reflecting a limited set of variations. Rather, it was a hodgepodge of diversity, in which one dialect might be more conservative than its neighbor in one respect and more innovative in another.

While Pictish ogam is frequently problematic, one of the names that stands out very clearly and that is corroborated by the king lists is traditionally rendered as NEHT- or NEHHT-, the designation for *Nechtan* (variously spelled). Here the traditional ogam sign ⊣ (or ⊣⊣)—H (or H H)—was definitely used and represented a spirant /χ/ related with the aspirate /h/ both in sound and evidently in the perception of the speakers.

As it were, NEHT- is cognate with the root NET- 'grandson, nephew, descendant' found in Irish ogam inscriptions. Of course, both terms call to mind Latin *nepos* 'grandson, nephew' with its root *nepot-* (compare McManus 1991:100). Indeed, this was also used by Latinized Celts as a name—compare Cornelius Nepos of Cisalpine Gaul (Rankin 1987:106). (The name may possibly be related with NETTAS in Gaulish—Evans 1967:369–70.)

In these three forms, then, we see the historical progression noted at the beginning of this paper in which Indo-European /p/ (as in Latin) changed first to a spirant that could ultimately be realized as /h/ or /χ/ (as in Pictish) and then to null (as in Irish). The realization of /χ/ in Pictish is quite in accord with more general changes from the Indo-European /p/ before /t/ (compare Lewis and Pedersen 1973:27); and indeed, where the /t/ followed directly, the /χ/ was retained in Irish, as in *nechta* 'granddaughter' (compare Latin *neptis*). In keeping with the diverse nature of Celtic dialects, Pictish had long retained the reflex spirant but had already changed the /kʷ/ to /p/, there being no longer any competing sound.

Insofar as the rendering of the ogam signary is concerned, then, Pictish H would indeed be historically appropriate for the reconstructed *P. The Irish ⊣ had lost its pronunciation and the sound /χ/ was represented in appropriate environments as ≣ or C (single or doubled), as we find, for example, in the name CARRTTACC *Carthach* (McManus 1991:124). On the other hand, Pictish ⊣ still represented /h/ or /χ/. Thus, the comparative evidence verifies the status of H as a reflex of *P in the Pictish ogam signary. This opens the door for *P in the original tally system upon which the ogam alphabetic signary was imposed, if not on the original alphabetic signary itself.

3. IMPLICATIONS. The Celtic cultures were to one degree or another marked by the synthesis of the pre-Indo-European cultures of the Atlantic region—those that would culminate in the megalithic cultures—and the cultures of the Indo-Europeans. Certainly the most prominent of these latter peoples were those we now identify as the Beaker Folk. In the introduction of the Beaker Culture, however, there was a ratio of population impact to cultural influence that gradually tilted in favor of the latter in

the progression westward into the British Isles—in much the same manner as had occurred (verified by archaeological and biological evidence) in the earlier extension of Neolithic farming techniques from Anatolia (compare Sherratt 1997:22). For example, the significant changes to the grave goods included in beakers in the North Atlantic basin (ibid 387) imply that the Beaker Culture was only partially adopted. In particular, the evidence from what was to become Pictland strongly suggests the adaptation of Beaker burial practices with major modifications in keeping with traditional local practice (see Ashmore 1996).

When we consider the Celts (particularly the Insular Celts), we are thus faced with an amalgam of cultures in which the conservative, pre-Indo-European elements were particularly prominent. One thing that the presence of arrow heads and the absence of javelin heads does not tell us, however, is how the indigenous languages of the region differed from those of the Indo-European Beaker Folk. Indeed, we could take the extreme position of Colin Renfrew (1987) and suggest that the Indo-European languages had spread during the Neolithic agricultural revolution, millennia before the arrival of the Beaker Folk with their somewhat related language.

Moreover, while we can trace the progress of various strains of DNA across Europe in a number of different patterns and can use these patterns to help determine movements of peoples, this evidence tells us nothing about the languages these peoples spoke (compare Cavalli-Sforza 2000). One of the most enlightened observations made by modern linguists is that there is no necessary connection between language groupings and genetics.

Within this framework of uncertainty enters the problem of ogam *P. The fundamental distinction between Celtic and the other Indo-European families is the absence of /p/ in Celtic. Yet, both the structural evidence of the ogam signary and the comparative evidence of Irish and Pictish inscriptions inexorably point to ⊣ as in fact deriving from an original *P /p/. Furthermore, the very nature of the tally system from which ogam was derived indicates an indigenous, pre-Indo-European origin.

How, then, are we to classify ogam? In particular, just what was the language that provided the mnemonic phonetic values for the ogam signary? Was it pre-Indo-European; was it pre-Celtic; or both? Or shall we say that Celtic itself—in language and/or in culture—stretched back much further into the past than we may feel comfortable with? Indeed, as recently proposed by Simon James (1999), should we be examining the 'Atlantic Celts' with their ogam signary as a separate group altogether—a group with many diversities of its own? Moreover, just how much of that diverse cultural group might we consider to be Indo-European or pre-Indo-European, however we may now wish to interpret that distinction?

In any case, it is time for linguists to address the evidence of the past quarter-century in archaeology and in biology and to start asking such difficult questions. In the wake of the evidence, we need to make some major reassessments. And somewhere within these reassessments lurks the problem of ogam *P.

REFERENCES

ASHMORE, P. J. 1996. *Neolithic and Bronze Age Scotland*. London: Batsford.

BARHAM, LARRY, PHILIP PRIESTLY & ADRIAN TARGETT. 1999. *In search of Cheddar Man*. Stroud, Gloucestershire: Tempus.

CARNEY, JAMES. 1975. The invention of the ogam cipher. *Ériu* 26:53–65.

CAVALLI-SFORZA, LUIGI LUCA. 2000. *Genes, peoples, and languages*. New York: North Point.

EVANS, D. ELLIS. 1967. *Gaulish personal names: a study of some continental Celtic formations*. Oxford: Clarendon.

FORSYTH, KATHERINE. 1997. *Language in Pictland: the case against 'non-Indo-European Pictish (Studia Hameliana 2)*. Utrecht: de Keltische Draat.

GERSCHEL, LUCIEN. 1962. L'Ogam et le nombre. *Études celtiques* 10:127–66.

GIPPERT, JOST. 1990. Präliminarien zu einer Neuausgabe der Ogaminschriften. In *Britain 400-600: Language and history*, ed. by Alfred Bammesberger & Alfred Wollmann, 291–304. Heidelberg: Carl Winter Universitätsverlag.

JAMES, SIMON. 1999. *The Atlantic Celts: ancient people or modern invention?* Madison: University of Wisconsin Press.

LEWIS, HENRY & HOLGER PEDERSEN. 1974. *A concise comparative Celtic grammar*, 3rd ed. Göttingen: Vandenhoek & Ruprecht.

MACALISTER, R. A. S. 1945. *Corpus inscriptionum insularum celticarum*. Dublin: Stationery Office [Reprint 1996. Dublin: Four Courts Press].

MCCONE, KIM. 1996. *Towards a relative chronology of ancient and medieval Celtic sound change (Maynooth studies in Celtic linguistics 1)*. Maynooth: St Patricks College.

MCMANUS, DAMIAN. 1991. *A guide to ogam (Maynooth monographs 4)*. Maynooth: An Sagart.

MERONEY, H. 1949. Early Irish letter-names. *Speculum* 24:19–43.

RANKIN, H.D. 1987. *Celts and the classical world*. London: Areopagitica Press.

RENFREW, COLIN. 1987. *Archaeology and language: the puzzle of Indo-European origins*. Cambridge: Cambridge University Press.

SHERRATT, ANDREW. 1997. *Economy and society in prehistoric Europe: changing perspectives*. Princeton: Princeton University Press.

STEVENSON, JANE. 1990. The beginnings of literacy in Ireland. *Proceedings of the Royal Irish Academy* 89C:127–65.

DURATIONAL PROPERTIES OF SYLLABLES AS POTENTIAL EVIDENCE FOR RHYTHMIC PATTERN IN L2 ACQUISITION

CHRISTIAN GUILBAULT
Department of Linguistics, University of Alberta

RECENTLY, MUCH TIME AND EFFORT has been devoted to understanding the phenomenon of foreign accent in adult L2 acquisition. As a result, it is now possible for researchers to predict assimilation patterns of new phonemic contrasts (Flege 1995, Best 1995). However, experimental studies on the nature of a possible prosodic accent are still too few and far between. This is somewhat paradoxical, since the effect of non-native prosody is considered by many to be of crucial importance for accurate production of individual phones, for the expression of emotions and for proper parsing and processing of the speech signal by listeners. This study presents the results of two experiments which examined the rhythmic temporal pattern of French as produced by native speakers of English learning French.

1. THE PROBLEM. Rhythm is central to the prosodic structure of speech. The work of Pike (1945) and Abercrombie (1967) has led to the traditional distinction between *stress-timed* and *syllable-timed*. Languages that belong to the former category exhibit equal temporal distance between stresses and languages that belong to the latter category exhibit equal temporal distance between syllables. A third category was later proposed for Japanese and Tamil, which exhibit moraic rhythm. The most important consequence of stress-timed languages like English, according to this proposal, is the necessity to either compress or lengthen syllables in order to make stress groups approximately equal in duration. By definition, syllable-timed languages, like French, exhibit constant syllable duration (except for group-final syllables, see Wenk & Wioland 1982). Measurement studies have failed to provide empirical evidence of strict acoustical isochrony (Dauer 1983, Roach 1982, Wenk & Wioland 1982 among others), and most authors now consider these labels as tendencies rather than categories. These studies also led to the proposal that the two opposing rhythmic types be replaced by a continuum with prototypical stress-timed and syllable-timed languages located at opposing ends. Other languages would be located on this continuum according to their approximate resemblance to either type (Dauer 1987). Despite the interest of such a proposal, the role played by temporal patterns in a language and their importance in locating a specific language on this continuum is still unclear.

2. GOAL OF THE CURRENT EXPERIMENT. The primary goal of the current research is to investigate the use of duration as a primary property of the syllable and its ability to account for rhythmic properties of languages. More specifically, I expect to shed

French sentences	English sentences
Luc a mangé et s'est endormi.	Luke has been gone and has not been seen.
Luc qui a mangé s'est endormi.	Luke who has been gone has not been seen.
Luc qui aurait mangé se serait endormi.	Luke who would have been gone would have been seen.
Pierre qui a peint travaille beaucoup.	Claire who has painted travels a lot.
Pierre qui nous a peint travaille beaucoup.	Claire who should have painted travels a lot.
Pierre qui nous a bien peint travaille beaucoup.	Claire who should have been painting works a lot.
Claude n'a pas vu Marie à la fenêtre.	Jim has not seen Mary at the window.
Claude n'aurait pas vu Marie à la fenêtre.	Jim would not have seen Mary at the window.
Claude ne l'aurait pas vu à la fenêtre.	Jimmy would not have seen Mary at the window.

Table 1. *Sample of English and French corpora.*

some light on the phenomenon of L2 acquisition of the temporal rhythmic structure of French by native speakers of English. A secondary goal of this research is to gather empirical evidence related to the acquisition of suprasegmental features of a language. The acquisition of new phonemic segmental contrasts is fairly well documented, but experiments investigating the acquisition of suprasegmental features are still infrequent. In this paper, I report on two experiments which examined the rhythmic temporal pattern of French as produced by native speakers of English learning French.

3. METHODOLOGY. This study involved twelve participants. Among these were six learners of French and six native speakers of French who were recruited from the student and staff populations at the University of Alberta. The learners of French were divided into two subgroups according to their oral proficiency and their overall experience in French[1]. Less experienced learners (EL1) had between 7 and 13 years of exposure to formal instruction in high school or university. More experienced learners had in general spent extensive immersion periods in French-speaking environments either in Montreal, Canada, or in Paris, France. All native speakers of English but one were native speakers of Canadian English. Native speakers of French were subdivided into two smaller groups, based on their dialect: 3 native speakers of Canadian French (CF), 3 native speakers of European French (EF)[2]. Both groups were considered representative of their respective dialect.

Speech samples consisted of recalled single-sentence utterances. During the recording session, participants were presented with a single sentence on a computer screen. They were instructed to read the sentence and then to repeat it while facing a blank page. Due to the relatively high number of sentences to be uttered, no distractors were used. The second utterance of every sentence was used for measurements. All speakers read the same list of sentences presented in a different random order.

The stimuli were single sentences in French which included one relative clause varying in length. Given that French has a group-final primary stress, it was expected that this clause would have only one primary stress mark and, hence, would constitute a single rhythmic group[3]. Six sentence groups were conceived involving words with different syllable structures (see Table 1 for a sample list of the analyzed sentences). A similar corpus was formed in English with sentences that have a similar number of syllables, similar syntactic structure, and, in some cases, comparable syllable structures.

Syllable durations were measured within the embedded clause of each sentence. Measures were taken from the onset of the first segment—the nucleus or the first C in onset position—to the beginning of the next syllable at the zero-crossing point. The very rare hesitations and silent pauses were removed from the measurements. The segmentation process of the French corpus into syllables was done according to the principles proposed by Delattre (1940). Most syllables in the French corpus had a CV or CVC structure. The syllabification of the English corpus was also relatively straightforward because of the nature of the corpus. Intervocalic consonants were assigned to a syllable following the maximal onset principle. Most syllable structures in the English corpus were also CV or CVC.

4. EXPERIMENT 1.

4.1 VARIABILITY INDEX. The goal of this first experiment was to determine if English learners of French exhibit more syllabic durational variability in a production task than native speakers of French. In order to measure this variability, the index used by Deterding (2001) was chosen. This index measures variations in syllable duration compared to an average syllable duration. All syllables within the relative clause were measured. Following Deterding, it was decided not to include the final syllable of this clause in the measurements. The motivation for this decision is that measuring group-final syllables, which usually bear primary stress in French, would introduce an important amount of variation in the calculations if produced differently across groups of participants. Among all 30 sentences recorded for each participant, only the last three sentences of each sentence group had the three syllables minimally required for the computation of the index (total of 18 sentences for each participant). Normalized durations were used in the computation in order to neutralize variations in tempo. The formula in (1) shows the computation of the VarIndex:

(1) $VarIndex = \left(\sum_{k=1}^{n-2} \left| d_{k-1} - d_k \right| \right) / (n-1)$

d_k = normalized duration (duration of a syllable divided by average
             duration of all measured durations of a phrase) of the kth syllable,
             and

n   = number of syllables

Speaker Groups	VarIndex French Corpus	Standard Deviation
English	0.3601	0.1995
EL1	0.4056*	0.2486
EL2	0.4298*	0.2399
CF	0.3508	0.2042
EF	0.2866*	0.1547

Table 2. *VarIndex for all speaker groups. (*EL1 and EL2 are statistically significant when paired with EF.)*

According to this VarIndex, perfect syllable isochrony would allow for no durational variability, hence leading to a VarIndex of 0. The greater the variability in syllable duration, the greater the index. Digitization (20 kHz) and measurements were done using Computer Speech Laboratory by Kay Elemetrics Corporation model 4300.

Earlier studies have considered English and French as prototypical examples of stress-timed and syllable-timed languages. Therefore, it is expected that syllabic durational variability as measured with the VarIndex will be greater in English than in French. Previous research in L2 acquisition has found evidence which suggests that L2 learners gradually acquire the rhythmic properties of an L2 (Wenk 1986). Hence, English learners of French are expected to exhibit more variability in syllabic duration in French than native speakers of French. Moreover, the amount of inter-syllabic variability should decrease as English speakers become more proficient in French. Finally, one more hypothesis can be made based on the differences reported between CF and EF (Armstrong 1999, Paradis & Deshaies 1990, Ouellet & Tardif 1996 among others): CF speakers will exhibit more inter-syllabic variability than EF speakers.

4.2. RESULTS. The VarIndexes for all groups of speakers are given in Table 2 and Figure 1. The results partially validate the first hypothesis, as native speakers of English produced a greater VarIndex than native speakers of European French (EF). However, the index displayed by native speakers of English in their mother tongue, 0.3601, is noticeably lower than the ones reported by Deterding (2001). In his study, indexes of 0.448 and 0.543 were reported for British and Singaporean speakers respectively, with standard deviations of 0.164 and 0.172. This discrepancy between the results of the present research and Deterding's are attributed mainly to the different experimental tasks. It has been claimed that read speech in French (which resembles the recall speech used in the current experiment) is characterized by a tendency to regularize inter-syllabic intervals, whereas spontaneous speech favors the production of breath groups of equal length (Vaissière 1991). In addition, the unusually low number of syllables with complex onsets or codas in our corpus of English (most syllables are CV or CVC) may have contributed to lower inter-syllabic variability. Contrary to the first hypothesis, however, durational variability displayed by CF speakers is noticeably greater than the index displayed by EF
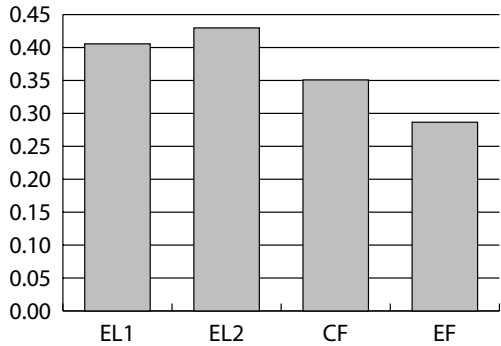
Figure 1. VarIndex for all speaker groups: Intermediate English learners of French (EL1), Advanced English learners of French (EL2), native speakers of Canadian French (CF), and native speakers of European French (EF).
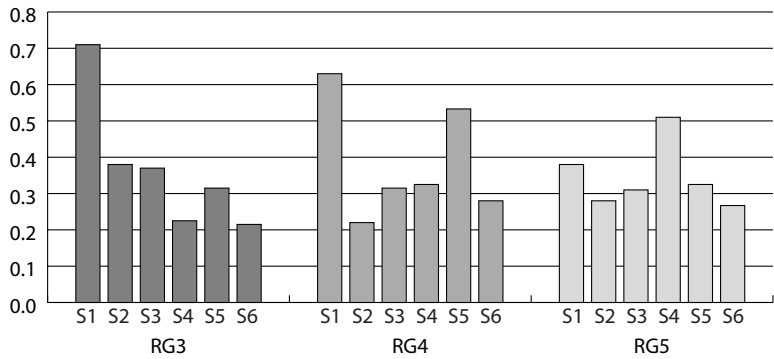
Figure 2. VarIndex for each sentence within each rhythmic group.

speakers, and similar to the one of English speakers. There is no immediate explanation for this surprising result. It is possible that the index used in this study did not capture the more subtle differences between the three groups of speakers, or that Canadian French is becoming a quantity-sensitive language (Armstrong 1999), thereby resembling to English more than to European French. This hypothesis will have to be investigated more in depth in further research.

The second hypothesis for this experiment is confirmed. In general, English L2 learners of French produced greater VarIndex and standard deviations in the target language than native speakers of French. Results from a two-way ANOVA revealed a significant Speaker Group effect (p<.05). A Post-hoc (Tukey's HSD) analysis indicated that EL1 and EL2 are significantly different when paired with EF but not when paired with CF. The third hypothesis, which assigned significantly lower indexes to more advanced learners of French, was not confirmed by the post-hoc analysis. This unexpected result suggests that L2 learners of French did not make significant progress after several years of exposure to French. It appears that despite their greater

experience, the EL2 speakers have not acquired a native-like temporal rhythmic structure in French.

A closer look at the results of the ANOVA revealed a second interesting effect. The analysis characterizes the main effect of Sentences nested within Rhythmic Group as significant (p < .05). This result indicates that some sentences were produced across all groups of speakers with a significantly greater index than other sentences. Among these sentences, RG3S1, RG4S1, and RG5S1 may have a higher index because they include a short vowel immediately followed by a syllable with a long French nasal vowel (*Luc a man*gé). Similarly, the presence of a complex onset like /br/ or /bj/ when preceded by a syllable which has no onset nor coda, found in RG4S5 (*Rome qui a brûlé serait détruite*) and RG5S4 (*Paul avait bien aimé la chasse*), generated a high VarIndex.

These results suggest that duration provides consistent information regarding the temporal structure associated with French rhythm. It is still undetermined if the absence of significant difference between both groups of learners should be attributed to the limits of the index or to the possibility that EL2 speakers may display fossilization. The results also strongly suggest that specific phonemic factors must be considered in an account of syllable variability.

5. EXPERIMENT 2: LANGUAGE-SPECIFIC PHONEMIC PROPERTIES. The previous experiment rested on the assumption that all speakers, including L2 learners of French, followed similar rules for segmenting the signal into syllables. Previous research (Beaudoin 1996) has shown that English learners of French, especially in the earlier stages of acquisition, exhibit mixed syllable structures. A proper account of L2 rhythm must consider this important variable. The goal of this second experiment is to confirm the tendencies identified in experiment 1 when syllable structure is not considered.

Measurements for this experiment are similar to the one proposed by Ramus, Nespor & Mehler (1999), who proposed a simple measure of the duration of vocalic and consonantal intervals and their respective standard deviations. These intervals consist of the unaltered total duration of the segments in a given sentence. In their paper, the authors argued that this basic phonetic account of the temporal structure of a sentence reflects the phonemic properties of a language, such as syllable structure and vowel reduction, for instance. They further explain, following a proposal made for the first time by Dasher and Bolinger (1982), that these language-specific phonemic properties are responsible for the perception of different distinct rhythmic classes.

The following experiment attempts to show that language-specific phonemic properties are responsible for different rhythmic temporal structures. English and French differ in a number of phonemic properties, among which the most noticeable are the syllable structures (greater use of CVC in English, CV in French), the existence of vowel reduction in English, and the presence of lexical stress in English (Dauer 1983). These phonemic properties should be reflected in the phonetic structure of the utterances, which will be measured in the vocalic and consonantal intervals.

The specific hypotheses for this experiment are: a) L2 learners of French will produce a greater percentage of vocalic intervals in English than in French; b) L2 learners

of French will produce smaller vocalic intervals in French than native speakers; c) standard deviations for consonants and vowels will be greater for learners of French than for native speakers of French; and d) more proficient learners will exhibit intervals closer to native speakers'.

5.1. METHODOLOGICAL CONSIDERATIONS. The data used for this analysis were taken from the same corpus used in the first experiment. Measurements were taken only on the last sentence of each sentence group, however. Contrary to the first experiment, the entire sentence was used for the analysis. Once more, the last syllable of the sentence had to be excluded from the calculations mainly because it was on many occasions almost inaudible and impossible to measure with accuracy.

Following Ramus, Nespor and Mehler (1999), vocalic intervals of the recalled sentences are defined as all vowels located in between two consonants, and consonantal intervals are formed by all consonants located between two vowels. The sentences were segmented as in (2).

(2)   *Rome qui aurait brûlé serait détruite.*
       /r-ɔ-mk-i-ɔ-r-ɛ-br-y-l-e-s-ə-r-ɛ-d-e-(tr- ɥi-t)/

In these examples, intervals are separated by "-" and the omitted sentence-final syllable is in parentheses. The authors provided three variables in their study:

1.   Percentage of vocalic intervals in the entire sentence (%V), computed by dividing the sum of all vocalic intervals by the total duration of the sentence and multiplying it by 100;
2.   Standard deviations of vocalic intervals within each sentence (ΔV), and
3.   Standard deviations of consonantal intervals within each sentence (ΔC).

The sum of all consonantal and vocalic intervals should be identical to the duration of the entire sentence.

5.2. RESULTS. Table 3 and Figure 3 (overleaf) present the results across all groups of speakers. As predicted by the first hypothesis, English speakers exhibit noticeably greater %V in English than in French, thereby supporting the first hypothesis. The ratios in this second experiment are in general greater than the ones reported by Ramus et al., who found proportions of vocalic intervals for English and French of 40.1% and 43.6% respectively. These discrepancies are, once again, attributed to the constitution of the English corpus, which is not considered similar to free speech. As mentioned before, there is an insufficient amount of complex onsets and codas compared to CVs and CVCs, and this would give a higher vowel interval percentage.

The analysis of the French corpus produced by all groups of speakers partially confirmed the experimental hypotheses. Contrary to the second hypothesis, the proportions of vocalic intervals produced by EL1 is noticeably greater than those of EL2

	# of V Intervals	# of C Intervals	%V	ΔC	ΔV
EL1	94	93	57.81	0.064	0.069*
EL2	93	93	53.09	0.057	0.047
CF	95	95	53.83	0.058	0.045
EF	99	98	54.98	0.058	0.038*
total:	381	379	—	—	—
English	225	231	47.21	0.057	0.046

Table 3. *Number of consonantal and vocalic intervals measured, vocalic intervals and standard deviations across all speaker groups. (*Significant difference as determined by a Tukey (HSD) post-hoc analysis.)*
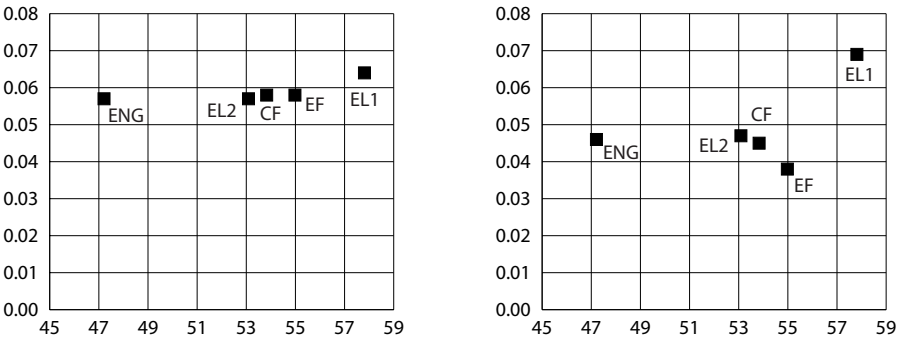
Figure 3. *Distribution of speaker groups over the %V, ΔC (left) and (right) ΔV planes.*

and both groups of native speakers of French. Unfortunately, the two-way ANOVA did not reveal any significant difference between these ratios. Standard deviations for consonants (ΔC) and vowels (ΔV), however, did confirm the experimental hypothesis, as greater values were displayed by both groups of learners of French (EL1, EL2). Contrary to the results reported by Ramus et al., and as predicted by hypotheses (c) and (d), the standard deviation of vocalic intervals (ΔV) proved useful in discriminating between speaker groups. Speakers who belong to EL1 exhibited the greatest standard deviation (0.069), EF displayed the smallest deviations (0.038) and EL2 and CF presented intermediate variations (0.047, 0.045). This difference between EL1 and EF was declared significant, in a one-way ANOVA ($p < .05$). The standard deviations associated with consonantal intervals were not significant in this study.

6. DISCUSSION. This study examined the hypothesis that duration as a fundamental property of the syllable provides an account of English and French rhythm. It differs from previous studies, since it compares data about adult L2 learners and native speakers of French. The first experiment used a variability index to determine

if variations in syllabic duration are consistent with previous accounts of English and French rhythm. Moreover, it was predicted that English L2 learners of French would display more syllabic variability in French than native speakers. The confirmation of these hypotheses strongly suggests that duration as a fundamental property of the syllable must be part of a proper account of rhythm. Even though these results are in agreement with the controversial notion of isochrony, the analysis also suggests that a more detailed investigation is required in order to explain the effects related to the quality of the segments and syllable structure.

An attempt was made with the second experiment to examine more accurately the effect of phonemic properties of both languages and their role in the account of rhythm. The tendency for learners and native speakers of French to produce identical relative clauses with different syllabic temporal properties measured in the first experiment was confirmed. These different acoustic properties displayed by learners of French strongly suggest that, at least in the early stages of acquisition, learners have not fully acquired the phonological properties of the target language. In the acquisition of French by native speakers of English, the phonetic characteristics of vowels seem to be more challenging than consonants. These experiments raise many interesting questions regarding the nature of linguistic rhythm and its acquisition. For instance, what exactly are the language-specific phonemic properties which contribute to the perception of rhythm? Which of the phonemic properties are acquired during the acquisition of an L2? Are some languages more difficult to acquire by L2 learners than others because of the complexity or nature of their rhythmic properties, and if so, why?

The results obtained in the study presented here certainly highlight the importance of duration as a fundamental property of French rhythm. However, more empirical evidence from cross-linguistic studies is needed to confirm the importance of phonemic properties of vowels in L2 acquisition as observed in this research. To confirm this will require a larger sampling of the population and a broader sampling of languages and speech material. Regardless, the results of this study does not support the strong position which attributes the perception of rhythmic differences between languages solely to isochrony. Instead, the results presented here suggest that the phenomenon of rhythm would be better understood if analyses of languages' segmental properties and phonetic aspects are included.

---

[1]   This initial classification has been confirmed by a perceptual experiment. In this experiment, native speakers of French subjectively classified these learners into two distinct categories. The intraclass correlation (Shrout 1995) between all listeners was high at 0.895.

[2]   The term 'European French' refers to speakers who speak a variety of French with no traces of regional accent perceived by the main experimenter.

[3]   A *rhythmic group*—or *groupe accentuel*—in French is a series of unstressed syllables followed by and including one syllable bearing primary stress (Lacheret-Dujour & Beaugendre 1999:45).

REFERENCES

ABERCROMBIE, DAVID. 1967. *Elements of general phonetics*. Edinburgh: Edinburgh University Press.

ARMSTRONG, SUSAN. 1999. Stress and weight in Quebec French. *ACOL*, Banff (Alberta).

BEAUDOIN, MARTIN. 1996. *The syllable structure of French as perceived by first and second-language speakers*. Unpublished Ph.D. thesis, University of Alberta.

BEST, CATHERINE T. 1995. A direct realistic view of cross-language speech perception. In *Speech perception and linguistic experience: Issues in cross-language research*, ed. by Winifred Strange, 171–206. Baltimore:York Press.

DASHER, RICHARD & DWIGHT BOLINGER. 1982. On pre-accentual lengthening. *Journal of the International Phonetic Association* 12:58–69.

DAUER, REBECCA. M. 1983. Stress-timing and syllable-timing revisited. *Journal of phonetics* 11:51–62.

———. 1987. Phonetic and phonological components of language rhythm. *11th International Congress of Phonetic Sciences* 5:447–50.

DELATTRE, PIERRE. 1940. Tendances de coupe syllabique en français. *Publications of the Modern Language Association* 55(2):579–95.

DETERDING, DAVID. 2001 The measurement of rhythm: a comparison of Singapore and British English. *Journal of phonetics* 29:217–30.

FLEGE, JAMES E. 1995. Second language speech learning: Theory, findings, and problems. In *Speech perception and linguistic experience: Issues in cross-language research*, ed. by Winifred Strange, 207–32. Baltimore: York Press.

GUAÏTELLA, ISABELLE. 1999. Rhythm in speech: What rhythmic organizations reveal about cognitive processes in spontaneous speech production versus reading aloud. *Journal of pragmatics* 31:509–23.

LACHERET-DUJOUR, ANNE & FRÉDÉRIC BEAUGENDRE. 1999. *La prosodie du français*, Paris: CNRS Éditions.

OUELLET, MARISE & BENOIT TARDIF. 1996. From segmental duration properties to rhythmic structure: a study of interactions between high and low level constraints. *Proceedings of the International Conference on Spoken Language Processing* 2:1177–80.

PARADIS, CLAUDE & DENISE DESHAIES. 1990. Rules of stress assignment in Quebec French. *Language variation and change* 2:135–54.

PIKE, KENNETH L. 1945. *Intonation of American English*, Michigan: University of Michigan Publications.

RAMUS, FRANCK, MARINA NESPOR & JACQUES MEHLER. 1999. Correlates of linguistic rhythm in the speech signal. *Cognition* 73:265–92.

ROACH, PETER. 1982. On the distinction between 'stress-timed' and 'syllable-timed' languages. In *Linguistic controversies*, ed. by David Crystal, 73-79. London: Edward Arnold.

SHROUT, PATRICK E. 1995. Measuring the degree of consensus in personality judgements. In *Personality research, methods, and theory*, ed. by Patrick E. Shrout, *&* Susan T. Fiske, 79–92. Hillsdale NJ: Erlbaum.

VAISSIÈRE, JACQUELINE. 1991. Rhythm, accentuation and final lengthening in French. in *Music, language, brain and speech: Proceedings of an international symposium at the Wenner-Gren Center, Stockholm, 5–8 September 1990*, ed. by Johan Sundberg, Lennart Nord *&* Rolf Carlson, 108–20. Houndmills UK: Macmillan

WENK, BRIAN. 1986. Crosslinguistic influence in second language phonology: Speech rhythms. *Crosslinguistic influence in second language acquisition,* ed. by Eric Kellerman *&* Michael Sharwood Smith, 120–33. Elmsford NY: Pergamon Institute of English.

———— *&* FRANÇOIS WIOLAND. 1982. Is French really syllable-timed? *Journal of phonetics* 10:193–216.

ON THE SUPERIORITY OF TONETIC OVER SEGMENTAL PHONETIC EVIDENCE: AN ORIGINAL REANALYSIS OF KHOEKHOE TONE

ROY S. HAGMAN
Trent University

OF ALL FORMS OF LINGUISTIC EVIDENCE, phonetic evidence has always had the reputation of the being the 'hardest' and most objective. The whole scientific edifice of structural linguistics was founded upon the corpus of phonetically transcribed linguistic data to which the techniques of analysis could be applied. The core of the phoneticians' methodology was a system of alphabetic representation then already 3,000 years old, newly reinvented and universalized on a solid basis of articulatory observation. Though, on a theoretical level, linguists gradually weaned themselves from the alphabetic form of representation, on the practical and didactic levels it is still very much alive and used.

Through a combination of historical accidents, however, the representation of the non-segmental aspects of linguistic sound, and tone in particular, had been sadly neglected from the beginning and was slow to benefit from the new articulatory science. The languages for which alphabets were invented tended to be non-tonal, and the Asian languages for most of their history used only ideographic systems. If it were not for the efforts of Byzantine Greeks attempting to represent the pitch-accent of ancient Greek with marks above the vowels, even accent marks would likely never have been invented. These, however, gradually lost their association with pitch properties, an association which had to be rediscovered by modern scholars (Stanford 1967).

Because available phonetic alphabets had no reliable way to indicate tone, early modern investigators of tone languages had to improvise, inventing their own idiosyncratic methods for observing and recording the tones of the languages they were studying. Their efforts ranged from impressionistic drawings of tonal contours to the detailed mapping of the pitch of the voice against a grid of frequencies or musical pitches. The articulatory science developed during the 19th and early 20th centuries was of little help to them for the simple reason that the glottal tightening and thickening responsible for changes in the fundamental pitch of the voice was not for them an observable phenomenon. They were, in effect, forced to do an early, non-instrumental form of acoustic phonetics.

When properly done, tonetic transcription had the distinction among all forms of phonetic transcription of having been done by means of an entirely extra-linguistic form of measurement and so was not subject to the limits imposed by an available inventory of phonetic symbols. A transcriber with a good ear for pitch could record the continuous rise and fall of the fundamental pitch of the voice with reliable accuracy. When the pitch meter first made its appearance at the mid-century, the tracing

1.	high-rising	c'	>	d'
1a.	less high-rising	a#	>	c'
2.	mid-rising	g	>	b
3.	low-rising	f	>	f#
4.	high-falling	c'	>	b
5.	mid-falling	a#	>	g#
6.	low-mid level	g#	>	g#

Table 1. *Khoekhoe tonal contours.*

of pitch became mechanized, but the complex nature of vocal tone made it only a moderately reliable device. The most modern phonetic software now makes it possible to do entirely reliable tracings of vocal pitch by following the entire overtone series of the vocal buzz and using this information to compute fundamental pitch. The pitch tracings that such modern devices produce are, however, very similar to what can be done with the human ear, whose neural machinery no doubt computes fundamental pitch in very much the same way.

An example of good early tonetic recording and its advantages that I would like to examine is the work of D.M. Beach, who in the 1930's published his classic work *The Phonetics of the Hottentot Language* (Beach 1938), which included a careful tonemic analysis of what we now refer to as the 'Khoekhoe' language, a major indigenous language of Namibia best known for its extensive inventory of click consonants. Beach's work on Khoekhoe tonetics was inspired by Karlgren's work on Chinese and included careful tracings of continuous pitch fluctuations against a grid defined by the tones of the western musical scale. He was, of course, measuring absolute pitch, not the relative pitch that is the essence of tone languages, and he chose to simplify things by limiting his tracings to the pitch range of one person, his primary linguistic informant. The utterances he recorded ranged all the way from individual roots to entire folk tales, where overall intonation contours could be seen to carry the tonal fluctuations of the individual roots over an extensive absolute pitch range. Beach actually recorded and published much more tonetic material than he himself could analyze, recognizing that the interplay of tone perturbations, intonation, and syntactic factors were all producing complexities that were beyond his powers of analysis with the primitive phonemic theory then available to him. He admittedly includes much of this extra information for the benefit of future generations of linguists to unravel with techniques yet to be invented and theories yet to be proposed. These complexities have finally been done justice in the recently published work of Haacke (1999).

Beach's examination of his tonetic data led him to propose that the tonemic system of Khoekhoe consisted of six contour tones which he mapped according to the tones of the musical scale. Presenting Beach's contours according to the names of the note values at the end of the contours, we get the patterns in Table 1.

Note that the contours all fell within the following sequence of pitches: f-f#-g-g#-a-a#-b-c'-c#'-d', comprising the comfortable speaking-pitch range of his primary

informant. The domain of these contours was not the syllable, as was the case with other known tone languages, but the root. The root in Khoekhoe was quite a well defined unit phonologically, and roots fell into five classes according to canonical form: C, CV, CVV, CVN, and CVCV. The classes were numbered sequentially I through V, and only II through V could carry tonal contours.

Kenneth Pike in his book *Tone Languages* (Pike 1948) was the first to express scepticism regarding Beach's assigning of contours to roots rather than syllables, and he suspected that there was an alternate solution. The other logical possibility was that the contours were the result of the sequencing of two register tones, but the fact that Beach assigned them to roots of class II with the form CV meant that two register tones would have to be regularly found on single vowels—not impossible, but unlikely.

As a result of my own analysis of the language (Hagman 1977), I found that many of the roots which Beach had classified in Class II (CV) were actually roots of Class III (CVV) where the two vowels happened to be identical. Beach had not thought of this because he had unfortunately rejected the length marks that had been placed on just these roots by earlier investigators, since he had tried his best to measure absolute vowel length and found he could not do so reliably. Once these roots are removed from Class II (CV), all that remains in this class are particles and suffixes, so that all true roots with tone in Khoekhoe then only have one of three forms, CVV, CVN, and CVCV, all three with two nuclei, each of which can carry a register tone.

However, even knowing that all the roots are bipartite, it is still hard to see how there could be register tones when one looks at Beach's tonetic tracings of the six contours. Anyway, if there were two registers there should be four contours, not six, and if there were three registers, there should be nine. Still, how the contours might be resolved into register tone sequences is not immediately apparent.

Part of the solution is a reanalysis that becomes possible upon a close study of Beach's methodology, which he scrupulously describes at every step. In his later work on the Korana language, a close relative of Khoekhoe, Beach describes a new elicitation technique where he devised frames to include the word whose tone is being examined. His intent was to prevent the distortion caused by utterance-final contours (Beach 1938:243). It turns out that Khoekhoe, like Korana, has an intonation pattern where the pitch of the voice falls at the end of an utterance, a contour which automatically applies when words are uttered in isolation. The possibility then arises that perhaps register levels are obscured in the Khoekhoe tracings because the contours are distorted by this effect.

When one goes back to Beach's contours and raises each of the final pitches in the contours which do not already end in a lower tone, there appears a suggestion of three pitch levels between which the contours are moving: a low level on the note f, a mid level on the note g#, and a high level on the note c'. One need only extend some of the contours at each end to get the underlying tonal contours in Table 2 (overleaf).

Tone number 1, high rising, is an allotone conditioned by a diphthong ending in a high vowel, so we will take 1a, less high rising, as basic. After making all the necessary alterations, it becomes clearly apparent that the contours are moving from one

1. high-rising	c'	>	d'				
1a. less high-rising	a#	>	c'	becomes	g#	>	c'
2. mid-rising	g	>	b	becomes	f	>	c'
3. low-rising	f	>	f#	becomes	f	>	g#
4. high-falling	c'	>	b	becomes	c'	>	c'
5. mid-falling	a#	>	g#	becomes	c'	>	g#
6. low-mid level	g#	>	g#	becomes	g#	>	g#

Table 2. *Derivation of underlying tonal contours.*

of three pitch levels in the first syllable to one of two in the second, resulting in the six combinations found by Beach. We will return later to the question of why the contours may appear to be shortened in Beach's data.

The presence of different numbers of registers in each syllable is still, however, a problem. Languages with register tones tend to have the same number of registers for all syllables. Interestingly, Beach himself solves this problem for us with the historical-comparative work he did using Khoekhoe and its sister language Korana (Beach 1938:247–53). Words beginning with the lowest tone in Khoekhoe tend to correspond with words beginning with voiced consonants in Korana, distinctive voicing not being present in Khoekhoe. He suggests that the lowest tone in Khoekhoe is the result of a relatively recent loss of voicing in initial consonants. Since medial consonants are always voiced in both languages, the same did not happen in the second syllable. Reanalysing Beach's contours into register sequences, we can say that the lowest tone of the first syllable, an extra-low tone, was likely a replacement for a lost voicing distinction. In acoustic terms, we would be looking at the replacement of the downward bending of the first formant produced by a preceding voiced consonant with a lowering of fundamental vocal pitch, which would have a roughly similar perceptual effect.

Beach never did a tonemic analysis of Khoekhoe's particles and suffixes, but he knew they didn't have contour tones. In my own work, I found three register tones to be present in these—not surprising since Korana has a voicing distinction in the consonants of these items too, which Khoekhoe has also apparently lost. It turns out that it is only the second syllable of a root where just two of the three possible register tones are found. It is thus now very simple to transcribe the language with markings for three tonal levels: acute accent for high, nothing for mid, and grave accent for low.

Even if one accepts that the evidence accumulated since Beach's time weighs heavily in favour of a register-tone interpretation of Khoekhoe tonology, one cannot help but remember that Beach diagrammed his contours as smooth curves; nowhere do we see the jumping between registers from syllable to syllable which we expect to find in a register tone language. However, when we read Beach closely, we begin to realize that his tracings and generalizations about contours apply primarily to the roots of Classes II through IV (CV, CVV, and CVN), and much less so to the less numerous roots of Class V (CVCV). These roots, which he always discusses last, are the only roots with clearly disyllabic structure. When we closely examine his tracings of longer texts we see

movements between more clearly defined pitch levels and not the smooth contours of the other roots. It now looks like the contouring effect found on most Khoekhoe roots may be explainable as a sort of tonetic 'diphthongization', analogous to the purely phonetic form of diphthongization which occurs in Khoekhoe roots with vowel clusters, as I described in an earlier paper (Hagman 1995). A concomitant effect is the shortening of tonal glides in these roots as a side effect of a rapid articulation, an effect which we reversed earlier in our reanalysis when we extended the glides.

In conclusion, the preceding reanalysis was only possible because of Beach's extremely careful tonetic transcription and his scrupulous detailing of methodology. The transcriptions are observations of physical acoustic facts, not categorizations according to a given inventory of symbols, nor extrapolations of a preexisting theory of tone, nor impositions of any traditional view of language. We can be sure of this because at that time there were no symbols for tones, there was no general theory of tone, and there was no traditional view on the subject of tone to impose. By virtue of working in an area that linguistics had neglected, Beach had found himself doing some investigative work which can stand up to anyone's definition of good science.

REFERENCES

Beach, D. M. 1938. *The phonetics of the Hottentot language*. Cambridge: Steffer and Sons.

Haacke, Wilfrid H. G. 1999. *The tonology of Khoekhoe (Nama/Damara)*. Köln: Rüdiger Köppe.

Hagman, Roy. 1977. *Nama Hottentot grammar (Indiana University publications language science monographs 15)*. Bloomington: Indiana University.

———. 1995. The disyllabic diphthongs of Nama Hottentot. *lacus forum* 21:100–109.

Pike, Kenneth. 1948. *Tone languages*. Ann Arbor: University of Michigan Press.

Stanford, W. B. 1967. *The sound of Greek: studies in the Greek theory and practice of euphony*. Berkeley: University of California Press.

ONOMATOPOEIA MARKERS IN JAPANESE

KEN-ICHI KADOOKA
Ryukoku University, Kyoto, Japan

ONOMATOPOEIA in the lexicon of Japanese is both abundant and systematic. Japanese has a far richer and more systematic onomatopoeic stock than either Chinese or English, though not as great as Korean. The quantity is shown in the 1634 examples in Kakehi et al. (1996). I use the entries in this dictionary as a database for the following discussion.

An inspection of the database reveals five morphological classes of onomatopoeia, as in (1):

(1)	bare stem	26	*hisi*	(to hug firmly)
	altered reduplication	41	*gasa-goso*	(to look for something)
	doubled base	45	*butu-kusa*	(voice/manner of grumbling)
	reduplication	716	*koro-koro*	(something rolling)
	others	806	*guiQ*	(< base *gu*, jerking action)

Only 26 entries, or 1.59% of the total, are bare stems. This suggests that actual onomatopoetic usages are realized as a base with some morphological suffix(es). For example, altered reduplication includes a change of phoneme(s); in some examples, such as *dota-bata* '(state of extremely busy with something)', the whole first syllable is different. In *gasa-goso*, the /a/ vowels in the base are replaced by /o/ in the reduplicant. No example of consonant alternation alone is found in the database. Doubled bases, on the other hand, are fairly common: e.g. *suQteN-korori*, 'manner of falling down while walking or running' > *suQteN* 'stumbling' and *korori* 'rolling'.

Reduplication is the most characteristic onomatopoetic morphological class. 'Others' include various forms, amounting to 806 items in the database. The major subdivisions are shown in section 3.

In the following sections, we that entries in the Japanese onomatopoeia lexicon are characterized by morphological or phonological markers. We call those morphemes/phonemes/features which are peculiar to Japanese onomatopoeia Onomatopoeia Markers (hereafter OMs), and describe the sound symbolic system of Japanese onomatopoeia in more general terms in section 3.4.

1. SYSTEMATICITY OF JAPANESE ONOMATOPOEIA. In this section, we briefly review the organization of Japanese onomatopoetic lexical items. We assume that the systematicity originates in the derivational structure of the lexeme (base + suffix[es]) and that

this derivation is synchronic rather than historical. The most representative onomatopoetic base is disyllabic with canonic form as in (2).

(2) $/C_1V_1 C_2V_2/$

Typically the first consonant is voiceless. These base forms are not themselves onomatopoetic without OMs, but they can be reduplicated to become attested forms.

The concept of OMs was suggested in Waida (1984). These are affixed to the base, forming actual onomatopoetic lexemes, as in (3).

(3)   base + OM = onomatopoetic lexeme

OMs are usually suffixed to the base, symbolizing various aspects of the action or sound depicted by the base. Because bare stems are rare in the database, we can conclude that OMs are generally necessary for the onomatopoeia lexicon of Japanese.

OMs can be divided into two categories, moraic and segmental. Moraic OMs are more conspicuous because they add at least one mora to the base. The labels are tentative, and segmental OMs have never been studied systematically.

The five types of moraic OM's are given in (4):

(4)   moraic nasal N
       moraic consonant Q: duplication of the following consonant
              [p t k s h] or a glottal stop
       vowel prolongation R
       ri
       Reduplication

These OMs differ from one another with regard to phonological duration or length; N, Q, and R add only one mora to the base and do not constitute independent syllables; conversely *ri* adds one syllable to the base and Reduplication multiplies the number of syllables in the base.

Segmental OMs modify a distinctive feature of some phoneme of the base. The three resulting modifications of this category are less visible morphologically than the modifications of moraic OMs. The classes of segmental OMs are summarized in (5).

(5)   Voicing   433   /koro/ → /goro/   (rolling)
       Palatalization   56   /kata/ → /katya/   (clitter-clatter)
       Spirantization   37   /kutya/ → /kusya/   (messy)

Moreover, there may be more than one segmental OM. For example, voicing may be affixed to the spirantized product of the original base: /kutya/ → /kusya/ → /gusya/ 'messy'. It is also possible to have three segmental OMs with Voicing, then Palatalization then Spirantization: /heta/ → /peta/ → /petya/ → /pesya/ 'flat'. I discuss these

OMs, including their relative order and the reason for calling h → p voicing, in section 3.

2. MORAIC OMS. In this section, we survey moraic OMs without attempting a detailed study of individual OMs. Each OM is attested in previous studies (e.g. Waida 1984 Kadooka 1993), hence detailed investigation is omitted here. We mention only that there are hundreds of examples of each of the 5 OMs.

Some of the forms on the base /koro/ are given in (6). Each depicts a manner of rolling.

(6) *koro*   (the base does not occur alone)
     koroQ   something rolls only once; the rolling action does not continue for a long time
     koro-koro the thing rolls more than once
     koroN   implies the completion of the rolling
     koroRQ   the rolling object is large, suggested by the prolongation of the vowel R
     korori   the thing rolls and then stops

The notion of synchronic derivation is exemplified by the set of lexemes which have koro as a base. They all communicate something to do with rolling, but the manner of rolling differs from one to another, depending on the OM. To give a concrete example, a moraic stop Q signifies a short period of time, whereas the long vowel produced by R suggests either that the activity is extended in time or that the object is large.

The non-occurrence of the bare stem *koro* is typical of the lack of productivity of onomatopoetic roots without OMs. However, this stem is understood by native speakers as referring to the action of rolling. The form is regarded as unfinished without OMs.

The form *koroQ* is often followed by the citation article to; hence the moraic consonant is realized as [t]. As mentioned above, when only this marker follows the base, a short action is suggested.

Contrary to the short action expressed by *koroQ*, the reduplication *koro-koro* communicates rolling repeated more than once. The base can be reduplicated more than once, resulting in a three- or four-fold OM: *koro-koro-koro* or *koro-koro-koro-koro*. Such improvisational repetitions are not listed in dictionaries. Then remind us of the vivid narrative style used in telling nursery tales to children. Naturally the length of the rolling action is proportionate to number of reduplications. Reduplication can be combined fairly freely with other OMs.

The moraic nasal N contrasts with Q in communicating extended duration or large size. A larger object is pictured with N than with Q. At the same time, the completion of the action is also implied with N. At least part of the communicative contrast between N and Q can be ascribed to the phonetics. The greater sonority of N relative to Q reflects the greater vs. lesser duration or size iconically.

 Two OMs, i.e. R followed by Q, appears in *koroRQ*. Of the two markers, the vowel prolongation R is predominant in mentioning the manner of rolling in that the longer duration is hinted as well as the slowness of rolling. The second marker Q is rather phonetic than symbolic in that case; a form without Q, i.e. **koroR-to* is unstable.

The OM *ri* is an individual syllable. Its origin may be related to the Middle Japanese auxiliary *keri*. No etymological commitment is made here. *Ri* communicates the termination of the action. In *korori*, something rolled and stopped.

These five forms do not exhaust the derivations from the base *koro*. Though there are cooccurrence restrictions on some combinations of OMs (Q followed by R is phonetically impossible), moraic OMs can be combined rather freely, sometimes producing nonce formations. For example, *kororiN-kororiN* '(manner of something rolling intermittently)' combines *ri*, N, and reduplication. This kind of productivity is unique to Japanese onomatopoeia.

3. SEGMENTAL OMS. In this section, segmental OMs are discussed. These OMs differ from moraic OMs both in how they are applied and how the bases appear with them. These OMs are phonological in nature. Some segmental feature is different. This shift communicates the change of status of the thing/action/sound under description.

3.1. VOICING. We begin with Voicing. The voiceless obstruents of the bases /p t k s/ are voiced, producing /b d g z/, respectively. We return to the triad /h p b/ below and explain why the laryngeal fricative /h/ is paired with the labial stops /p b/. Take the base /koro/. The first stop of the base /k/ is voiced to /g/, and the converted base /goro/ implies that larger things are rolling than in /koro/.

Voiced obstruents are generally regarded as marked, relative to their unvoiced counterparts. Beside the iconicity between semantic and phonological markedness, however, there are other reasons for considering the voiced base marked. Take /koro/ again. This base portrays the manner of things rolling, ranging from smaller to relatively large things such as apples and oranges. The voiced counterpart /goro/, on the other hand, can only refer to large things. In this sense, the bases with voiced obstruents are marked, relative to semantically related bases with voiceless counterparts to those obstruents. Voiceless obstruents in Japanese are considered unmarked for other reasons than those adduced in our examination of onomatopoeia. One of these is in *rendaku* 'sequential voicing' (cf. Vance 1987, chapter 10). This voiceless-voiced contrast would be always judged voiceless unmarked not only in onomatopoeia in Japanese but also other lexicon in many of the languages of the world. Only one example from the various phenomena to consider voiceless [unmarked] is *rendaku* (sequential voicing) in Japanese (Vance 1987, Chapter 10).

Voicing occurs in 433 instances in the database (cf. [5] in section 1). This nearly equals the number of occurrences of moraic OM's. If we include the triad /h p b/ in this group, about 900 entries are involved voice OMs, i.e. the 433 with their unvoiced form and once again in their voiced form. This accumulation of data suggests the legitimacy of Voicing OMs.

It is generally the first consonant of a $/C_1V_1C_2V_1/$ that is affected by voicing. But in a few entries, e.g. /doteN/ → /dodeN/ 'manner of falling down', the second consonant is voiced. C_1 is, of course, in a more prominent position and a shift in voicing is more obvious here. In this regard, C_2 plays only a subsidiary role. But in the case of /doteN/ C_1 is already voiced. Voicing can only affect C_2.

There is also another type of exception to limiting Voicing to C_1: Double Voicing. In some lexemes, voiceless C_1 and C_2 are voiced. Consider the base *tyapo* 'splashing sound'. Moraic N is suffixed to /tyapo/, giving /tyapoN/. This form can be double-voiced, giving /dyaboN/. Double Voicing is presumably an Obligatory Contour Principle (OCP) violation. OCP prohibits two or more segments sharing a single feature within a given phonological domain. William J. Sullivan (personal communication) points out that the OCP is an artifact of a two-dimensional theory of phonology that does not permit a full exploitation of hierarchical (non-linear) relations. There is no question that /dyabo/ and forms like it occur naturally, so the implications for OCP need further consideration.

Now consider the triad /h/-/p/-/b/. Articulatorily, /h/ is a laryngeal fricative and /p/ and /b/ are bilabial stops, hence there is a more than one-feature gap between /h/ and /p b/. Yet the three consonants are treated as related in modern Japanese orthography, reflecting the historical change /p/ → /f/ → /h/, such that /h/ is unmarked, while /b/ is marked and /p/ most marked. Orthographic markedness is signaled in the kana syllabary: those syllables having /b/ onset are indicated with two dots superscript and those having /p/ onset with a superscript circle. The dots are used in other voiced obstruents /d g z/, but the circle is only for /p/ onsets. This also suggests a markedness order of /h/ > /b/ > /p/.

But a different markedness order is given by the onomatopoeia series as /h/ > /p/ > /b/, as in (7).

(7)   /hata-hata/:   the sound of something like flag fluttering in the wind
       /pata-pata/:   the wind is stronger than in /hata-hata/
       /bata-bata/:   the wind is stronger than in /pata-pata/

Dozens of the entries with the onset triad /h p b/ can be found in both sound and manner mimesis, proving the appropriateness of the derivational relationship among the consonants.

3.2. PALATALIZATION. The second segmental OM is Palatalization, which converts each of the consonants /p b t d k g s z h n/ into a palatalized counterpart. It is impossible to find a common articulatory feature defining these sounds as a natural class. Worse, the [coronal] /r/ and the [labial] /m/ are excluded from the list. At the moment it seems impossible to give this set of consnants a neat phonetic description. Palatalization applies to 56 items, versus 433 for Voicing. We return to this gap in section 3.4.

Palatalization occurs before the back vowels /u o a/, since consonants are automatically palatalized before the front vowels /i e/. Under palatalization, for example,

/beto-beto/ 'sticky' becomes /betyo-betyo/ [bečo-bečo] with palatalization of the /t/. *Beto-beto* communicates the idea of stickiness, such as with oil on the hand. *Betyo-betyo* adds a touch of discomfort. Palatalization does not necessarily entail a bad connotation. For example, /suru/ → /syuru/, both mimicking the rustling sound of cloth, implies nothing wrong.

Needless to say, the plain root is unmarked and the palatalized counterpart is marked. This parallels the relative markedness of plain to palatalized consonants. Palatalization is marked relative to voicing, because it has a broader area of application. That is, both unvoiced and voiced consonants can be palatalized, as can consonants like nasals, for which voice is not phonemic. In the onomatopoeia base $/C_1V_1C_2V_2/$, C_2 is more likely to be palatalized, but this is far from general. Many of the palatalized C_2's communicate a splashing or dripping sound, but other possibilities are included in (8).

(8)   /kata/   →   /katya/   clitter-clatter
      /nu/   →   /nyu/   to appear suddenly
      /pota/   →   /potya/   dripping sound of water
      /tapo/   →   /tyapo/   splashing sound of water
      /zyori/ →   /zori/   roughness of the surface of human skin

The entries in (8) are typical of palatalized items. But it is difficult to give a unified semantic description for explanation for all of them. At best, something like 'roughness' or 'discontinuity' is the most general semantic feature for these palatalized entries.

In contrast to Double Voicing, double palatalization is impossible. Schourup and Tamori (1992:123) suggest that this is due to articulatory difficulty: if both C_1 and C_2 are palatalized, it is tongue twisting for a native speaker of Japanese, though there is no such restriction on Slavic languages. Yet this is one more piece of evidence to assume that voicing is articulatorily easier than palatalization in Japanese onomatopoeia phonology.

There is one interesting pair of bases belonging to the group of dripping sound of water: /pota/ and /tapo/. The latter is interchangeable with /tapu/ by alternation of the second vowel. Thus /tapo/ and /tapu/ may both be distinct entries, giving the same meaning with two different syllables. This suggests that /p/ and /t/ are equivalent in symbolizing the dripping sound. If C_1 is predominant in the Japanese onomatopoeia lexicon, this alternation is a rare case.

3.3. SPIRANTIZATION. The last alternation is spirantization or sibilantization. Here a stop /t/ becomes a sibilant /s/, often in palatalized syllables. Here /t/ is regarded phonologically unmarked because the plosive phonemes (/p t k b d g/) outnumber those of fricatives (/s z h/) in the Japanese consonant system. Hence it would be more natural to postulate that a sibilant /s/ derives from /t/ rather than the opposite.

The next question is why Spirantization applies only to the voiceless stop /t/ and not its voiced counterpart /d/. There are phonological reasons for this. First, Spirantization

generally follows palatalization. The contrast between the voiceless /sy/ and /ty/ is maintained and can be neutralized in spirantization. But the contrast between the voiced /dy/ and /zy/ is neutralized. There is nothing left for spirantization to neutralize.

Spirantization is the most marked of the three Segmental OM's. It has even fewer occurrences still than Palatalization: only 37 examples are attested in the database.

Regarding combinations of segmental onomatopoeia, there is an interesting example concerned with the sound of splashing: the palatalized C_2 of /patya/ is spirantized to /pasya/; each of these can get a voicing OM on C_1, giving /batya/ and /basya/, respectively.

Spirantization is restricted almost exclusively to C_2 of the onomatopoeia root. The sole exception is /syaN/ (see below). Its range of application is limited to /t/ in C_2 position. This tells us why spirantization is so rare.

As mentioned above, the only case of the Spirantization OM not applied to C_2 is the monosyllabic base /syaN/, derived from /tyaN/ 'neatly, consciously'. There are no instances of Spirantization on C_1 of a /$C_1V_1C_2V_2$/ base.

Among the 37 entries with the Spirantization OM, only three lack the Palatalization OM as defined in in 3.2. above. Examples of these are given in (9).

(9)　/mutuQ/　→　/musuQ/　　in a bad mood
　　　/gaQtiri/　→　/gaQSiri/　　the body strongly built
　　　/gatiQ/　　→　/gasiQ/　　the body strongly built

Of these three forms, the /si/ and /ti/ sounds are automatically palatalized phonetically, and they can't be further palatalized. Hence, the pure exception to Palatalization as an OM is only /musuQ/. If palatalized, the spirantized form would be */musyuQ/ which would be derived from */mutyuQ/. This would lead to the conclusion that Spirantization should obligatorily cooccur with Palatalization.

Other instances of Spirantization are given in (10):

(10)　/kutya/　　→　/kusya/　　　　　　　　　　messy
　　　/gutya/　　→　/gusya/　　　　　　　　　　cloth or paper crumpled
　　　/bityo/　　→　/bisyo/　　　　　　　　　　wet, soaked to the skin
　　　/dota/　　→　(/dosya/)　　→　/dotya/　　sound of falling down
　　　/petaN-ko/ → /petyaN-ko/ → /pesyaN-ko/　flat

It seems to me that those forms with non-spirantized /t/ in the bases entail a somewhat sticking tone while the spirantized counterparts with /s/ do not necessarily, with the latter sounding a little noisy. This may come from the acoustic property that the sibilant /s/ involves greater energy than /t/, and that it can last longer than the plosive /t/.

Some of the examples in (10) can be traced back to non-spirantized non-palatalized bases. So /pesyaN-ko/ goes back to non-spirantized /petyaN-ko/, which in turn comes from non-palatalized /petaN-ko/. All of these three forms are in the database. These derivational relations illustrate the relative order of Palatalization and Spirantization.

Another instance is /dosya/ derived from /dota/, but with an unattested intermediate form /dotya/. The others don't have non-palatalized forms. Non-palatalized /kuta/ exists, but it has the meaning 'exhausted'. The same is true of /guta/ and bita/. A full investigation of the interaction of Palatalization and Spirantization is still needed.

3.4. SUMMARY. The three segmental OMs are arranged with regard to the frequency of occurrence in (11), together with the inventory of those consonant phonemes which are subject to each process.

(11)   Voicing (433)   Palatalization (56)   Spirantization (37)
       /p t k s h/   >   /p t k b d g s z h n/   >   /t/

The order in (11) reflects the relative importance of each OM. Voicing is clearly indispensable for the onomatopoeia lexicon in Japanese, both because of the pairs of voiceless-voiced consonants in the sound symbolic system and the proportion of obstruents in the realization of morphemes. Palatalization and Spirantization are much less prominent in the system of segmental OMs.

Another view of the segmental OM system is given by the numbers of the phonemes that participate in each process. Five phonemes are subject to Voicing, ten to Palatalization, but only one to Spirantization. This suggests that Palatalization is a more important OM than Voicing. Yet Voicing occurs about 7.5 times more than Palatalization. If we include the voiced obstruents /b d g z/ as participating in Voicing, the number of the phonemes related to Voicing is nine, which approaches that of Palatalization. But the fact that Voicing OMs occur 7.5 times as often as Palatalization OMs in spite of the fact that there are fewer potential targert for Voicing underlines the significance of the Voicing process in sound symbolism. By either measurement, however, Spirantization is the least significant of the three.

4. CONCLUSION. The present study, though still preliminary, has a number of advantages. First, by noting the various OMs, we can provide insight to large numbers of semantically parallel but otherwise unrelated entries. Second, It also provides regularity to the phonology, resulting in a certain amount of phonosymbolic iconicity. Third, it suggests that the onomatopoeia lexicon of Japanese should be organized on the basis of the root morpheme.

Similar approaches to onomatopoetic vocabulary may be taken with languages that have a much less systematic set of morphological patterns than Japanese, like English (Kadooka 1995) or Beijing Chinese (Kadooka 2001), and with languages that have a much more systematic set of morphological patterns, like Korean (Noma 2001). The typology of onomatopoetic vocabulary in many more languages must be studied before we can make general statements about the phonological patterns used.

REFERENCES

KADOOKA, KEN-ICHI. 1993. The derivation structure in Japanese onomatopoeia. *Proceedings of Kansai Linguistic Society* 13:106–16.

———. 1995. Sound symbolism in English onomatopoeia word forms. Ryukoku Journal of Humanities and Sciences 17.39-47.

———. 2000. Morphological patterns in Japanese and Chinese onomatopoeia. *Ryukoku journal of humanities and sciences* 22(1):31–42.

KAKEHI, HISAO, LAWRENCE SCHOURUP & IKUHIRO TAMORI. 1996. Dictionary of iconic expressions in Japanese. Berlin: Mouton de Gruyter.

NOMA, HIDEKI. 2001. Onomatope to on-syoutyou (Onomatopoeia and sound symbolism). *Gengo* 30(8):12–18.

SCHOURUP, LAWRENCE & IKUHIRO TAMORI. 1992. Palatalization of Japanese *r* in relation to theories of restricted underspecification. *Gengo Kenkyuu* 101.

VANCE, TIMOTHY. 1987. *An introduction to Japanese phonology*. Albany NY: State University of New York Press.

WAIDA, TOSHIKO. 1984. English and Japanese onomatopoeic *structures. Bulletin of Osaka Women's University Studies in English* 36.

DISCOURSE STRUCTURE OF TWO PARABLES

Shin Ja J. Hwang & Jonathan Lathers
SIL International
Graduate Institute of Applied Linguistics

THIS PAPER PRESENTS A DISCOURSE ANALYSIS of two parables in the New Testament: the parable of the workers in the vineyard (Matthew 20:1–16) and the wedding feast parable (Matthew 22:1–14)[1].

A parable is a small, embedded discourse that is part of a much larger discourse. Parables contain some of the most recognized stories in the Bible, but they are also some of the most misunderstood and misinterpreted stories[2]. Each parable has a specific audience and a set of points by which the audience is drawn into the parable and enticed to action. A hearer is to understand different points of reference in a parable, identify himself with one point, and respond to the story's unexpected turn. Subgroups in the audience may assume different points of reference and react differently to the parable. Fee and Stuart (1982:126) suggest that Jesus' main purpose in telling a parable was 'calling forth a response on the part of the hearer'. The key to understanding the parables is to 'hear' them in the same way that the original audience would have heard them. Thus it is important to ascertain who the audience was for the parable and then to figure out what the points of reference are that would 'catch' that audience and cause them to respond in some way.

The two parables are each analyzed at the discourse level and divided into macrosegments of an aperture (a formulaic opening), prepeak and peak episodes, and a closure. A peak episode is 'a zone of turbulence in regard to the flow of the discourse in its preceding and following parts' (Longacre 1996:38). The parables are also analyzed at the paragraph level, with each macrosegment divided into sentences and embedded paragraphs. The two parables, although different in internal paragraph structures, are similar in overall discourse structure reflecting a narrative schema, but with hortatory intent as related to the larger discourse. The audience of each parable is identified, and the subgroups are related to points of reference. The analysis of the parables tries to capture the Koine Greek structure, but literal translations in English are used for ease of presentation.

1. THE PARABLE OF THE WORKERS IN THE VINEYARD (MATT. 20:1–16). The parable starts with the formulaic expression, 'The kingdom of heaven is like', in v. 1a, following the well-established pattern of introducing a series of parables Jesus used to enhance our understanding of the kingdom of heaven. Verse 16, 'Thus will be the last ones first and the first last', is outside the story proper and is considered the closure containing the moral. The rest of the material in the body of the story can be divided into

two episodes at the discourse level: the hiring process spanning almost the whole day (vv. 1b–7), and the paying process (vv. 8–15) initiated by a conjunction (*de*) and a participial clause '[when] evening having come'. The first episode recounts hiring five groups of workers at different times: early in the morning and the third, sixth, ninth, and eleventh hours (or 9 a.m., noon, 3 and 5 p.m.). The second episode provides the paying process, starting from those hired last going on to the first. It is here that something unexpected occurs: they are all paid the same wage of a denarius—forming the climax of the story. On the surface level of the morphosyntax, unusual features occur, marking the episode as peak. The discourse structure of the parable is presented in (1), with notional narrative schema slots in parentheses (the aperture is considered only a surface slot with no notional correlate):

(1)   Aperture: 20:1a
       Prepeak Episode   (Inciting Incident):   20:1b–7
       Peak Episode   (Climax):   20:8–15
       Closure   (Moral):   20:16

The prepeak episode is expounded by a sequence paragraph, as shown in the indentation diagram in (2)[3]. The first Sequential Thesis (ST) is filled by a simple paragraph with Setting in v. 1b, with an introduction of the main participant, the landowner, and the first action of the parable—in a relative clause—that the landowner goes out to hire workers. The Thesis in v. 2 begins with a participial clause, in which the landowner makes the agreement for payment with the workers, and the main clause verb 'sent' is in aorist, the first grammatically marked mainline action.

(2)   Prepeak Episode: Sequence ¶ (Matt. 20:1b–7)
        ST1: Simple ¶
          Setting: 1b a landowner who **went out** early in the morning to hire
                       workers for his vineyard.
          Thesis: 2 And having agreed with the workers for a denarius for the
                       day, (he) **sent** them into his vineyard.
        ST2: Simple Dialogue ¶
          Lead-In: 3 And having gone out around the third hour (he) **saw**
                       others having stood in the marketplace idle.
          IU (Proposal): 4 And to them (he) **said**, 'Go also you into the vine-
                       yard, and whatever may be considered right (I) will
                       give to you.'
          RU (Nonverbal Response): 5a And they **left**.
        ST3: 5b And again having gone out around the sixth and the ninth hour
                   (he) **did** likewise.
        ST4: Compound Dialogue ¶
          Lead-In: 6a And around the eleventh hour having gone out, (he)
                       found others having stood

> *Exchange 1:* Simple Dialogue ¶
>> *IU (Question):* 6b and (he) SAYS to them: 'Why have (you) been standing here all day idle?'
>> *RU (Answer):* 7a (They) SAY to him, 'Because no one hired us.'
> *Exchange 2:* Unresolved Simple Dialogue ¶
>> *IU (Proposal):* 7b (He) SAYS to them, 'Go also you into the vineyard.'

Each ST corresponds to a group of workers hired at a different hour, except that ST3 includes two groups in v. 5. The temporal progression corresponds to the numbers of ST's, from ST1 to ST4. The amount of detail given for each group shows the importance of each group to the plot. Each time the landowner goes out, the same event takes place. ST1 and ST2 contain quite a bit of information so that the listeners would know what was going on. ST3, however, contains very little information; these workers are not as important to the story. It summarily handles two different groups of workers by the use of 'again' and 'likewise'. Then in ST4, there is a lot of detail and a dialogue with the last group of workers who were hired at the eleventh hour. Note also after the stream of aorists in narration of ST1–3, the historical present is used here for the speech verb (*legei* 'says.he') in all three occurrences of vv. 6–7 (cf. Levinsohn 2000; Longacre 1999). These contrast with the *eipen* 'said.he' used in v. 4. All these features—the detail, dialogue, and unusual tense—draw more attention to the workers sent out at the eleventh hour because of their importance to the parable. Within the prepeak episode, ST4 certainly is climactic.

The peak episode (vv. 8–15) recounts the paying process. Verse 8 provides the Setting for the two ST's (see diagram 3), with a direct speech by the owner to the foreman to give the wage. Immediately, the audience is drawn in and the tension mounts as they try to figure out how much each group will get paid. The last group receives a denarius each in ST1, and the middle groups are not mentioned. Then ST2 (vv. 10–15) deals with the interaction between the owner and the first group of workers. They get the same amount! They are quite upset. It is here that we have reached the very zone of turbulence within the peak episode.

(3) Peak Episode: Sequence ¶ (Matt. 20:8–15)
> **Setting:** 8 And evening having come, SAYS the owner of the vineyard to his foreman, 'Call the workers and give them the wage, beginning with the last ones until the first ones.'
> **ST1:** 9 And those having come around the eleventh hour **received** each a denarius.
> **ST2: Complex Dialogue** ¶
>> *Lead-In:* 10 And the ones having come first **thought** that a larger sum (they) would receive, but (they) **received** each a denarius also themselves.
>> *IU (Remark):* Quote ¶

Quote Formula: 11 And having received (it), (they) *were complaining* against the landowner saying,

Quote: 12 'These last **worked** (only) one hour, and you **made** them equal to us who have endured the burden of the day and the heat.'

CU (CounterRemark): Quote ❡

Quote Formula: 13a But he, having answered one of them, **said,**

Quote: Contrast ❡

   Thesis: Reason ❡

      Reason: Reason ❡

         Thesis: 13b 'Friend, (I) am not treating you unjustly.

         Reason: 13c Didn't (you) agree with me for a denarius?

      Thesis: 14a Take what is yours and go.

   Antithesis: Comment ❡

      Thesis: 14b But (I) wish to give to this last the same as (I gave) you.

      Comment: Alternative ❡

         Alternative Thesis 1: 15a Or am I not allowed to do what (I) wish with what is mine?

         Alternative Thesis 2: 15b Or are you envious (lit.: is your eye evil) because I am good?"

There are several features in vv. 8–15 that help to mark the peak: participant reference, tense change, long relative clause, rhetorical question, and crowded stage. Throughout most of the parable, the subject is usually only marked in the verb, except when the participant is first introduced or when an explicit reference is needed for disambiguation, e.g., a switch of subject. Thus the landowner is introduced after the verb in v. 1b but is not overtly referred to again in the prepeak episode. At the beginning of the peak episode in v. 8, when evening came, he is called 'the owner of the vineyard'. This explicit reference in a noun phrase, even when there is no change of subject (between v. 7 and v. 8), helps to mark the episode boundary. Later in v. 13a, an explicit (articular) pronoun *ho* 'he' is used in addition to the usual agreement in the verb 'said.he'. This use of the pronoun with *de* 'but' is for a switch of speakers in dialogue and adds an emphasis to the landowner.

    Tense is mostly aorist throughout the parable, but the verb *legei* 'says.he' in v. 8 is in historical present (as in vv. 6–7, as noted above). In addition, there occurs one verb in imperfect in v. 11, where the first group grumbles about the pay (they do it continuously). It is the only imperfect in an independent clause in the parable. Their complaint in the direct quotation in v. 12 also shows turbulence at peak by the long modification in a relative clause at the end: 'to us who have endured the burden of the day and the heat'. Note that it does not give any new information but serves the function of rhetorical underlining. The long speech by the owner in vv. 13-15 is a marked

feature as well, including three rhetorical questions. This section also contains a crowded stage; the landowner, his foreman, and all the workers are there on stage.

While the grammatical structure of the parable is presented in (1)–(3), there is a clear chronological frame in chiastic structure according to the general content and lexical items. The workers that are hired at different times are paid in reverse order. Even the order of the first and the last in the thematic statements is reversed between the sentences in 19:30 and 20:16[4]. Both grammatical and chiastic principles of unity and cohesion of the text are at work here. The two principles are often quite independent of each other, but contribute to the unity of the whole each in its own way.

(4)   19:30 Many who are first will be last, the last first.
             20:1–2 Hiring workers early in the morning
                 20:3–5a Hiring at third hour
                     20:5b Hiring at sixth and ninth hours
                         20:6–7 Hiring at eleventh hour
                             20:8 Instruction for payment
                         20:9 Paying those hired eleventh hour
             (no mention: Paying those hired ninth, sixth, and third hours)
                 20:10-15 Paying those hired first
         20:16 The last ones will be first, and the first ones last.

The use of tense is somewhat chiastic as well: the historical present with speech verbs in the innermost structure (vv. 6-7 and v. 8), the usual aorist for mainline verbs in the rest of the body (vv. 1-5 and vv. 9-13), and the future for moral statements at the outermost (19:30 and 20:16).

As for the audience of the parable of the workers, the larger context shows that Jesus has been speaking to the disciples earlier (Matt. 19:23), and that is probably what is happening here. Matthew 20:1 has the conjunction *gar* 'for', making an explicit connection to the previous section, which ends with the moral in Matt. 19:30.

The points of reference in the parable of the workers would be the landowner, the first group of workers, and the last group. The other groups of workers are not given as much detail and do not appear at peak. Grammatically, the landowner is given direct participant tracking as the subject. The workers hired last engage in dialogue exchanges with the landowner at the hiring stage; it is the only group to have such prominence in prepeak episode. At peak, however, they only serve as a foil to the highlighted full-day workers, which is now the only group to have a dialogue exchange with the landowner at the paying stage. Anyone listening who would identify with the first group of workers in assuming unfairness on the part of the landowner would find themselves embarrassed by the landowner's reply to their objections. The intended audience is thus those who would 'identify with the full-day laborers, since they are the focus at the end' (Fee & Stuart 1982:130). We are likely to identify with the first group, but the message shows that God's mercy is equal for all and that we are all to be equally grateful[5].

2. THE PARABLE OF THE WEDDING FEAST (MATT. 22:1–14). This parable opens with a speech quote formula where Jesus is speaking to a multitude and some Pharisees: 'And having answered, Jesus again spoke to them in parables, saying' (v. 1). This is basically the introduction that a parable is coming and has a function only in the larger discourse. The parable itself is the quote, which is expounded by an embedded narrative discourse. It starts with almost the same aperture as in the parable of the workers: 'the kingdom of the heavens is like'. The main body of the parable is similarly divided into two episodes, closing with the same kind of moral statement. The prepeak episode itself is expounded by an embedded narrative discourse with its own two episodes, prepeak(em) and peak(em)[6]. The overall structure of Matthew 22:1–14 appears in (5).

(5)   Quote Formula: 22:1
         Quote: Narrative Discourse
             Aperture: 22:2a
             Prepeak Episode (Inciting Incident):   Narrative Discourse
                 Prepeak(em) Episode: 22:2b–7
                 Peak(em) Episode: 22:8–10
             Peak Episode (Climax):   22:11–13
             Closure (Moral):   22:14

The prepeak(em) episode (see diagram 6) is expounded by a sequence paragraph in which the main participant is introduced in Setting with a relative clause: 'a king who prepared a wedding feast for his son'. ST1 contains the first eventline verb where the king sends out his servants to call the ones who were invited to the feast. But they do not want to come. ST2 begins with the word *palin* 'again', which is an introducer that shows a repeated action. More details are given this time, with direct speech. Once more, the king sends out his servants to invite the guests. What the king wants them to say is given in a direct quotation. The responses this time are more detailed and are broken into three groups. Instead of coming to the feast, the first two went off—one to his own field and the other to his business. The others seized the king's servants, insulting and killing them.

   The king's response to their deeds is found in ST3 (v. 7). The king becomes angry, sends out his army, destroys the murderers, and burns their town. The emphasis is on the actions of the king. The sending of the armies is in a preposed participial clause. The verb 'destroyed' is in third person singular as if the king was doing the destroying. The armies are the instrument by which the king performs the action of destroying and burning. The last two actions are stated in contrastive VO *kai* OV orders: 'he.destroyed those murderers and their city he.burned'.

   At this point in the story, the king is in a quandary: What is he to do for guests for the wedding? It is here the peak(em) starts. The king instructs his servants to go out into the streets and invite everyone. The servants go out and bring both good and bad people for the feast, filling the wedding chamber with guests. Thus peak(em) is expounded by an execution paragraph: the king's plan is carried out (executed) by his servants.

(6)   Prepeak Episode: (Embedded) Narrative Discourse (Matt. 22:2b–10)
    Prepeak(em) Episode: Sequence ⸤
        Setting: 2b a king who **prepared** a wedding feast for his son.
        ST1: 3 And (he) **sent out** his servants to call those invited to the
            wedding feast, but (they) *wished* not to come.
        ST2: **Counterexpectation/Frustration ⸤**
         Thesis: 4 Again, (he) **sent out** other servants saying, 'Tell those
             invited: "Behold, my banquet (I) have prepared, my oxen
             and fat calves having been killed; and all things (are)
             ready, come to the wedding feast."'
         Surrogate Thesis 1: 5 But they, having paid no attention, **left**, one to
             his field, another to his business.
         Surrogate Thesis 2: 6 And the others, seizing his servants, **mis-**
             treated and **killed** (them).
        ST3: 7 And the king **was angry**, and having sent his army, (he)
            destroyed those murderers, and their city (he) **burned**.
    Peak(em) Episode: Execution ⸤
        Plan: 8 Then (he) SAYS to his servants, 'Indeed the feast is ready, but
            those invited were not worthy. 9 Go therefore to the main
            streets, and as many as (you) find there, invite to the feast.'
        Execution: 10 And having gone out into the streets, his servants
            gathered all the people (they) **found**, both bad and
            good, and the wedding chamber **was filled** with
            guests.

The seam of the two episodes within the embedded narrative is marked slightly differently from that of the parable of the workers. Here, while *tote* 'then' occurs initially in the sentence and episode, there is no explicit reference to the king as there is to the landowner in Matt. 20:8. The king is the continuing agent-subject from v. 7, although he interacts with different groups, first with the murderers and then with his servants. In the whole parable, there are four explicit references to the king in a noun phrase, the first at the first introduction (v. 2), and the rest when there is a switch of subject across sentences (vv. 7, 11, 13). All these occurrences are required to make the participant reference clear.

The peak episode (vv. 11–13, in diagram 7) centers around the man with no wedding garment, throwing an interesting twist to the story. It is unexpected enough that the king becomes so generous as to invite even bad people from the streets to the wedding feast. But now that these people from the streets are at the feast, the king comes in and notices that one of them is not dressed properly. The king has him thrown out into the outer darkness[7].

This episode constitutes a climax reflecting the highest tension. It is filled by a compound dialogue paragraph. After the Lead-In sentence, Exchanges between the king and two separate groups follow. The first exchange consists of his (rhetorical?)

Question to the man who has no answer. The second occurs with his servants: his instruction to throw the man out (Proposal), to be followed by the unstated Response, yielding an unresolved simple dialogue paragraph.

(7) Peak Episode: Compound Dialogue ¶ (Matt. 22:11-13)
      Lead-In: 11 But the king, having come to see the guests, **saw** a man not wearing wedding clothes.
      Exchange 1: Complex Dialogue ¶
          IU (Question): 12a And (he) SAYS to him, 'Friend, how did (you) enter here not having wedding clothes?'
          CU (Nonverbal reaction): 12b But he (the man) **was speechless**.
      Exchange 2: Unresolved Simple Dialogue ¶
          IU (Proposal): 13 Then the king **said** to the servants, 'Having bound his feet and hands, throw him into the outer darkness; there will be wailing and gnashing of teeth.'

The peak of this story is not as well marked in the surface structure as the other parable. There is, for example, no long stretch of direct speech with rhetorical questions, but there are a few things that provide clues. First of all, the speech verb *legei* 'say' in this episode (v. 12a) is in the present tense, while the final one in v. 13 is in the usual aorist. The use of the historical present makes this section more vivid. An articular pronoun 'he' is used with *de* 'but' in v. 12b for a switch of reference, adding an emphasis to the man and his reaction. In some sense, this peak episode represents a crowded stage as well, having all the participants on stage in the background but focusing on the king and the man.

    The final verse (v. 14) constitutes the moral: 'For many are called but few chosen.' This statement is much better understood after having heard the parable than if heard on its own. Notice that the didactic statement of both parables is a kind of commentary or evaluation by Jesus. Within the parables, Jesus does not interject commentaries or explanations as to meaning. He merely tells the story and then gives the main teaching point that is expected to bring about action by the listeners.

    The audience of the wedding feast parable is fairly obvious. Jesus is speaking to the chief priests and the Pharisees (Matt. 21:45), with a large multitude listening as well. The king, the group who rejected their invitation, the group invited from the streets, and the man not dressed properly seem to be the points of reference. The king does most of the eventline action in the story, while those invited from the streets and the man play a very important role. Those who rejected the invitation play a crucial role in building the tension of the story, but are not referred to again after v. 7. The Pharisees might relate to the point of reference of those who are originally invited but do not come. In listening to the story the Pharisees would get caught when the king destroys them and then invites both good and bad people into the wedding feast. But this is not the place where the multitude who are also listening are drawn in. They are

probably rejoicing that the king has invited all the average people. But they get caught when one of these people gets thrown out because he is not dressed appropriately.

3. CONCLUSION. This paper has analyzed the discourse structure of two parables to see how discourse features are grammatically marked. While the embedded paragraph structures within the episodes differ considerably, from compound and complex dialogue paragraphs to sequence and execution paragraphs, there is an overall similarity in discourse structure. Both stories start with an aperture and close with a moral, with prepeak and peak episodes in the body. The differences tend to be in the details at the paragraph level as to how sentences and embedded paragraphs relate to each other. The differences in points of reference are a result of Jesus directing his parables to specific audiences through points at which they can relate to the story.

   Below is a comparison of the surface structures from the two parables as we analyzed them.

(8)   **Matt. 20:1–16**   **Matt. 22:2–14**

   Aperture: 1a   2a
   Prepeak Ep: Sequence ⸮   Narrative Discourse
                  ST1:   1b–2   Prepeak(em) Ep: Sequence ⸮
                  ST2:   3–5a   Setting: 2b
                  ST3:   5b   ST1:   3
                  ST4:   6–7   ST2:   4–6
                                           ST3:   7
                                       Peak(em) Ep: Execution ⸮
                                           Plan: 8-9
                                           Execution: 10
   Peak Ep: Sequence ⸮   Compound Dialogue ⸮
                  Setting: 8   Lead-In: 11
                  ST1:   9   Exchange 1:   12
                  ST2:   10–15   Exchange 2:   13
   Closure (Moral):   16   14

To briefly refer to the conference theme, what counts as legitimate evidence in linguistics, we argue for the use of recurring patterns in naturally occurring data. The data-based approach is fruitful, especially when we analyze a unit larger than the sentence. Native speakers are rarely conscious of discourse-level features. Notice, for example, the use of explicit reference ('the owner of the vineyard') at the beginning of a new episode although there is no subject switch. Note also the shift in tense and the use of rhetorical questions at peak. Such variation is likely to be 'intuitively' labeled just 'optional' at the sentence level, but each variety has a unique function in discourse. When we analyze a language such as Koine Greek with only written data and no native speakers, the use of textual data may be the only approach available.

1   We express our gratitude to Marlin Leaders, David Robinson, and Bruce Turnbull for their comments on the earlier versions of the paper.

2   Fee and Stuart (1982:123) point out that 'the parables have suffered a fate of misinterpretation in the church second only to the Revelation'.

3   The readers are referred to Longacre (1996, chapters 4 and 5) for a detailed discussion of paragraph and dialogue types. The abbreviations used here follow his convention: ST=Sequential Thesis, IU=Initiating Utterance, RU=Resolving Utterance, CU=Continuing Utterance. The data are given in literal translations in English to reflect the Koine Greek structure. The aorist tense/aspect, which usually marks the eventline of narrative, is indicated in boldface, the present in capital letters, and the imperfect in italics. The tense is indicated for main clause verbs and those in relative clauses. The implied information needed in English is shown in parentheses.

4   Matthew 19:30, 'But many who are first will be last, and the last first', is the final verse of the previous chapter, and 20:16 is the closure (moral) of the parable as outlined in (1). Both 19:30 and 20:16 are the thematic statements for the parable, bracketing the story.

5   The closure further points out that those who were first may be last since they are not as grateful as those who were originally last but are now first, being filled with gratitude for God's abundant grace.

6   To avoid the confusion, the embedded discourse episodes are marked by (em), e.g., peak(em).

7   We believe that our analysis as shown in diagrams 5-7 is 'plausible'. We have combined vv. 2b–7 and vv. 8–10 as two episodes in the same embedded discourse dealing with God's invitation for all of us to his kingdom. A short peak episode follows in vv. 11–13, with the theme of judgment, that we need to wear special garments (i.e., we need to be changed and reborn) to respond to his invitation. Alternative analyses, however, may be possible, e.g., combining vv. 8–10 and vv. 11–13 together as a sequence of events occurring on the same day. This, we believe, is a less preferred approach to text analysis since chronological time is in general secondary to grammatical criteria. Often the author chooses to bundle up events in a sentence or paragraph in which a considerable time lapse is involved. See, for example, v. 7, which states in one sentence a series of events that would have taken time, for the king to send his army, kill the murderers, and burn their city.

REFERENCES

FEE, GORDON D. & DOUGLAS STUART. 1982. *How to read the Bible for all its worth*. Grand Rapids, MI: Zondervan.

LEVINSOHN, STEPHEN H. 2000. *Discourse features of New Testament Greek*, 2nd ed. Dallas: SIL International.

LONGACRE, ROBERT E. 1996. *The grammar of discourse*, 2nd ed. New York: Plenum.

———. 1999. Mark 5.1–43: Generating the complexity of a narrative from its most basic elements. In *Discourse analysis and the New Testament*, ed. by Stanley E. Porter & Jeffrey Reed, 169–96. Sheffield, England: Sheffield Academic Press.

WHAT CONSTITUTES ACOUSTIC EVIDENCE OF PROSODY? THE USE OF LINEAR PREDICTIVE CODING RESIDUAL SIGNAL IN PERCEPTUAL LANGUAGE IDENTIFICATION

Masahiko Komatsu
University of Alberta & Sophia University, Japan

INTRODUCTION. *Language Identification* (LID) is a process for identifying a language used in speech[1]. The cues for identifying languages are classified into two types: segmental and prosodic. In the field of *automatic* LID by computers, much of the research so far has focused on utilizing segmental features contained in the speech signal (Muthusamy et al. 1994), although some research also suggests the importance of incorporating prosodic information into the system (Thymé-Gobbel & Hutchins 1996; Itahashi et al. 1999; etc.). In contrast to this engineering research scene, most of the research on *perceptual* LID by humans has focused on prosodic information.

Humans' capacity for LID, or perceptual LID, has drawn the attention of researchers from engineering, linguistics, and psychology. The typical method of research is to conduct perceptual experiments with stimulus signals that are supposed to contain prosodic information of certain languages but not contain segmental information. In other words, the signals are used as 'acoustic evidence of prosody' in the argument. The stimuli used in the experiments have been various and not consistent across researchers. The critical question here is whether the signals used really represent the prosody of language, or more specifically what represents prosody acoustically.

In this paper, I argue for the use of the Linear Predictive Coding (LPC) residual signal as the stimulus in perceptual LID. LPC is a basic technique used for analyzing and resynthesizing speech. It is based on the source-filter theory of speech production and separates the speech signal into a residual signal representing the source part and LPC coefficients representing the filter part. I argue that the LPC residual signal, if its intensity is adjusted, represents the prosodic information in the speech signal, and thus it is feasible to use it to test for the role of prosody in perceptual LID. I discuss in particular the factors relevant to syllable structure, citing data from the research recently carried out by the LID research group at Sophia University.

1. STIMULI IN PERCEPTUAL LID RESEARCH. A variety of signals have been used as stimuli in perceptual experiments. Most studies have used signals that were presumed to represent the prosody of speech. In the experiments, researchers played a stimulus and asked their subjects to choose one from a given set of a small number of languages or dialects, or adopted some other method of observing the subjects' responses. The stimuli to represent prosody used in previous experiments include lowpass-filtered speech (Atkinson 1968; Moftah & Roach 1988; Mugitani et al. 2000), laryngograph

output (Maidment 1976, 1983; Moftah & Roach 1988), triangular pulse trains or sinusoidal signals (Ohala & Gilbert 1979; Barkat et al. 1999), LPC-resynthesized or residual signals (Foil 1986; Mori et al. 1999), white-noise driven signals (Mori et al. 1999), and resynthesized signals preserving or degrading broad phonotactics, syllabic rhythm, or intonation (Ramus & Mehler 1999). Although all of these experiments showed that 'prosody' plays a role in LID, the stimuli used differ from each other in the amount of information they carry; that is, the acoustic definitions of prosody are not coherent across studies. An appropriate selection of stimuli is needed for further research.

It is clear that none of the stimulus signals used in the preceding studies, except for the LPC signals and Ramus and Mehler's signals (1999), properly represent prosody in speech. In lowpass-filtered speech, some segmental information is preserved under the cutoff frequency, pitch sometimes rises higher than the cutoff, and intensity is not preserved (ibid). The laryngograph output is an indication of short-term variations of glottal electrical resistance and virtually uninfluenced by supraglottal resonance and noise source (Moftah & Roach 1988). This means that it is not representative of outputted speech, which we actually hear in usual situations. Due to the loss of resonance and noise source, it does not contain sonority information, the importance of which I argue for in this paper. In the simulation of prosody with pulse or sinusoidal trains, the noise source is not taken into account, either. The white-noise driven signal keeps the intensity contour of the original speech but does not have any other information, e.g., pitch.

Signals made by LPC technique are new in the history of research on perceptual LID. The idea can be traced back to Foil's (1986) experiment, but it was simply a preparatory test for developing an automatic LID system. Foil resynthesized speech by LPC with its filter coefficients constant, resulting in the speech signal that had a constant spectrum all the time. Mori et al. (1999) was the first to apply the LPC technique in the research on perceptual LID. In the LID test of English and Japanese, they used a residual signal with its intensity adjusted so that it had the same intensity contour as the original speech. Theoretically, the LPC technique can remove most of the spectral contour information, and it creates a signal that sounds like muffled speech and is unintelligible enough for a perceptual LID experiment. This signal is the topic of the discussion in this paper.

The set of signals made by Ramus and Mehler (1999) should be noted, too. They conducted perceptual experiments on English and Japanese, controlling broad phonotactics, syllabic rhythm, and intonation. They segmented the original English and Japanese speech into phonemes and replaced them by French phonemes to exclude the segmental cues to LID. They created four types of stimulus signals differing in the information they contain: 'saltanaj', 'sasasa', 'aaaa', and 'flat sasasa.' In 'saltanaj', all fricatives were replaced by /s/, stops by /t/, liquids by /l/, nasals by /n/, glides by /j/, and vowels by /a/. In 'sasasa', all consonants were replaced by /s/, and vowels by /a/. In 'aaaa', all segments were replaced by /a/. 'Flat sasasa' was the same as 'sasasa' but its fundamental frequency was made constant. The information that each stimulus contained and the results of LID tests are summarized in Table 1. Ramus and Mehler concluded that syllabic rhythm is a necessary and sufficient cue.

	Intonation	Syllabic rhythm	Broad phonotactics	Result of LID
saltanaj	+	+	+	successful
sasasa	+	+	−	successful
aaaa	+	−	−	unsuccessful
flat sasasa	−	+	−	successful

Table 1. Stimuli and LID results of Ramus and Mehler (1999). '+' indicates presence of cue, and '-' indicates absence of cue.

All of the studies cited so far have argued for the importance of prosody, but the stimuli used differ from each other in terms of which acoustic features they contain, as discussed above, and thus the evidence that their arguments are based on, is not coherent. Further, the consideration of the acoustic properties of prosody through the comparison of stimuli has not been seen except for Ramus and Mehler (1999) and Mori et al. (1999).

2. PROSODY IN LINGUISTICS. Linguistic features that constitute a prosodic typology include accent (stress, pitch, and tone), intonation, and rhythm. Their acoustic correlates are, basically, fundamental frequency, intensity, and length. However, the notion of rhythm is quite controversial, and therefore the determination of its acoustic correlates needs further consideration.

Traditionally, the rhythm types of languages are classified into 'stress-timed', 'syllable-timed', and 'mora-timed.' However, the manifestation of these different rhythms has not been clear in the light of experimental phonetics. For example, Japanese is said to be mora-timed, but researchers do not agree on how this is defined (Warner & Arai 2001a). The claims that have been made so far may be classified into timing hypotheses, rhythm hypotheses, and other alternatives. The timing hypotheses assume that the mora is an isochronic unit or that the length of the higher level structure such as a word is predictable from the number of morae in it. The rhythm hypotheses claim that the rhythmic difference is the reflection of structural factors, such as syllable structures, phonotactics, etc., rather than timing specifically. The experiments by Ramus and Mehler (1999), discussed in Section 1, support this claim: They define 'syllabic rhythm' as the temporal alignment of consonants and vowels, which is the reflection of syllable structures, and show that it is essential to the perceptual discrimination of languages. Besides these hypotheses, there are alternative claims, such as Tajima's (1998) focusing on the competence of coordinating units in speech production, or Cutler and Otake's (1997) discussion of the role of the unit in perception. In the present paper, I adopt the rhythm hypothesis, following Ramus and Mehler (1999), Ramus et al. (1999), Warnar and Arai (2001b) among others.

Assuming that syllable structure is a contributor to rhythm, I claim that the sonority feature is prosodic linguistically because the syllable structure can be identified by
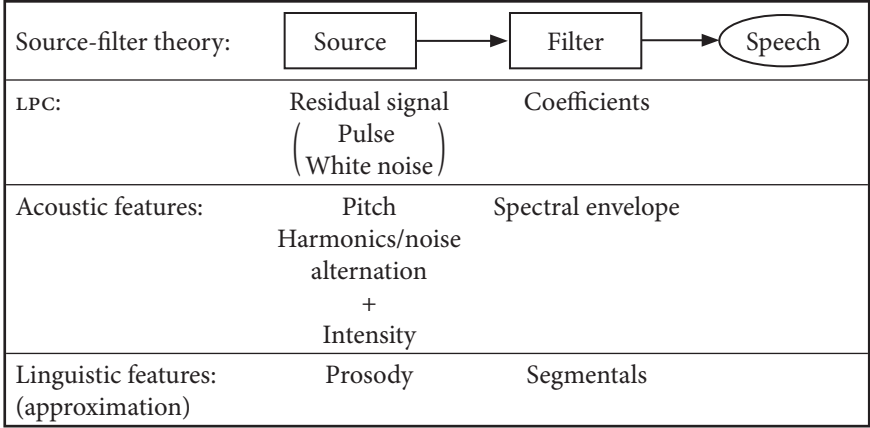
Source-filter theory:	Source →	Filter →	Speech
LPC:	Residual signal $\begin{pmatrix} \text{Pulse} \\ \text{White noise} \end{pmatrix}$	Coefficients	
Acoustic features:	Pitch / Harmonics/noise alternation + Intensity	Spectral envelope	
Linguistic features: (approximation)	Prosody	Segmentals	

Figure 1. *Correspondence of source-filter theory, LPC, acoustic features, and linguistic features.*

sonority contours with the Sonority Sequency Principle (cf. Kenstowicz 1994) phonologically. However, the sonority feature is also segmental by nature because it is closely related to the articulatory manner of segments. It also acts to introduce some broad phonotactic information. My claim is that the sonority feature is ambivalently prosodic and segmental, and that it is an important cue to LID because it constitutes rhythm.

3. LINEAR PREDICTIVE CODING (LPC).

3.1. MODEL. LPC analysis is based on the source-filter theory, and it separates the speech signal into the residual signal and coefficients, representing the source and filter respectively. The residual signal has a flattened spectrum, being similar to pseudo-periodic pulses for vowels and white noise for consonants. Acoustically, the residual has information on the fundamental frequency and the alternating pattern of harmonic structures and noise, while the coefficients have information on the spectral envelope. The intensity information is not carried by the residual signal, so it must be added later for use in perceptual LID experiments. The residual signal, after the addition of intensity, can be regarded as roughly corresponding to prosodic features at the linguistic level, and the coefficients to segmental features. See Figure 1.

Note that LPC is a mathematical operation dividing a signal into the source function and the transfer function, which do not exactly each correspond to the glottis and the vocal tract's activities in speech production. The residual signal is not representative of the glottal activity, as is the laryngograph output, but it rather represents the prosodic features of the speech emitted from the mouth.

It is also important to note that the simple dichotomy at the level of linguistic features pictured in Figure 1 is just an approximation. It is impossible to relate prosodic and segmental features to non-overlapping sets of acoustic features. First, the sonority feature at the linguistic level is both prosodic and segmental by nature, as already

discussed in Section 2. Consequently, the acoustic features representing sonority contribute to both prosodic and segmental features. Second, while acoustic features such as pitch, intensity, and length are essential to prosody, they also contribute to the perception of segments. (Length is not explicitly mentioned in Figure 1; it is represented as the temporal change of any feature.) These facts raise a problem when we try to extract acoustic correlates of prosody for perceptual LID experiments. Theoretically, it is impossible to completely separate prosody from segmental properties at the acoustic level. The signal representing prosody must contain the information on pitch, intensity, length, and sonority, but if it does, it then also contains some segmental information. There can be no acoustic signal that carries only prosodic features and does not carry any segmental features. Therefore, it is important to seek the signal that contains enough prosodic information but reasonably little segmental information as a practical approximation.

3.2. RESIDUAL SIGNAL. If intensity information is added, the LPC residual signal acoustically retains the fundamental frequency contour, the intensity contour, and the alternating pattern of harmonic structures and noise components. The fundamental frequency contour and the intensity contour are the perceptual cues to prosodic features in general. The presence/absence of harmonic structures and noise components together with intensity information may serve as the perceptual cues to vowel/consonant distinction and major classes of consonants, which in turn define the sonority of segments that indicates syllable structures.

The LPC residual is expected to represent sufficient prosodic features, including sonority, while it effectively suppresses segmental features.

4. PERCEPTUAL TESTS WITH LPC RESIDUAL. Perceptual tests support the idea that LPC residuals represent prosodic features including sonority, or syllable structures, while effectively suppressing segmental features. The result of an experiment on Japanese consonant perception showed the identification rate of major classes, which define sonority, was high, while that of phonemes was low (Komatsu, Tokuma, et al. 2000). Another experiment using Japanese, English, and Spanish consonant clusters also yielded the result that the identification rate of major class features was much higher than that of individual phonemes (Komatsu, Shinya, et al. 2000). In an LID experiment, the LPC residuals made from whole speech achieved much higher scores than the LPC residuals whose consonantal sections were muted (Komatsu et al. 2001), supporting the importance of sonority.

4.1. JAPANESE CONSONANTS. Komatsu, Tokuma, et al. (2000) conducted experiments on Japanese consonant perception with several signals including the LPC residual signal. They created the residual signal from 17 Japanese /C/+/a/ syllables (/ka/, /ga/, /sa/, /za/, /ʃa/, /dʒa/, /ta/, /da/, /tʃa/, /na/, /ha/, /ba/, /pa/, /ma/, /ja/, /ɾa/, /wa/) by LPC (sampling rate: 16 kHz; order of LPC: 22) and adjusted the intensity of the residual signals to match their original samples. The spectrum of the residual was tilted at -6 dB/oct

	[consonantal]	[approximant]	[sonorant]	Sonority
Obstruents	+	−	−	Low
Nasals	+	−	+	:
Liquids	+	+	+	:
Glides	−	+	+	High

Table 2. *Major class features (Komatsu, Tokuma, et al. 2000).*

Phonemes	20.0%
Major classes	66.4%

Table 3. *Identification rates by Japanese speakers in Komatsu, Tokuma, et al. (2000).*

to make it sound like speech rather than noise. They presented these signals in per-
ceptual experiments to 15 native speakers of Japanese and 4 native speakers of English
learning Japanese as L2.

They obtained the subjects' correct identification rates of phonemes and major
classes (see Table 2 for their definitions). The rates are calculated in such a way that,
if /ka/ is perceived as /ta/, it is counted as wrong for the identification rate for pho-
nemes, but counted as correct for the identification rate for major classes because
both are obstruents.

The results from Japanese native speakers are as shown in Table 3. The identification
rate of major classes is high, while that of phonemes is quite low. Considering the fact
that the identification rate of phonemes cannot be as low as $\frac{1}{17}$ (5.9 %) because the
sonority is the cue to the manner of articulation, the results indicate that segmental
information is effectively suppressed.

Another noticeable observation found in these results is that there was a mishear-
ing tendency for sonority to be perceived lower. However, we cannot discuss the
identification rates of individual major classes or major class features because the
number of samples was not balanced for major classes or major class features but was
balanced for phonemes.

In their experimental results, English native speakers generally showed better
identification rates than Japanese native speakers. This suggests that the sensitivity to
certain acoustic properties may be different depending on the speaker's first language,
though there was too small a number of English native speakers as subjects to make
a definite statement.

4.2. CONSONANT CLUSTERS OF JAPANESE, ENGLISH, AND SPANISH. Komatsu, Shinya,
et al. (2000) conducted perception tests on consonants in consonant clusters to see
the effects of phonotactic constraints. They created the residual signal by the same
signal processing method as Komatsu, Tokuma, et al. (2000). They used words that
begin with consonant clusters as shown in Table 4. Note that Japanese stimuli include

	Word-initial clusters	Number of types	Word length
Japanese	C	14	2 or 3 morae
	Cʲ	11	
	CV̥C	17	
English	C	21	1 or 2 syllables
	CC	39	
	CCC	9	
Spanish	C	18	2 syllables
	CC	11	

Table 4. *Stimulus words (Komatsu, Shinya, et al. 2000).*

palatalized consonants and consonant-vowel clusters where the vowel is devoiced because Japanese does not have consonant clusters at the word onset. They provided these samples to the native speakers of the language of each stimulus: 15 Japanese speakers, 4 English speakers, and 1 Spanish speaker.

The results showed that the identification rates of major class features were much higher than those of phonemes across all languages and cluster types.

The results also showed cross-linguistic differences. English native speakers showed better identification rates than Japanese native speakers, as in the experiments by Komatsu, Tokuma, et al. (2000). The results were also affected by phonotactic constraints, which are language dependent.

4.3. LID WITH/WITHOUT CONSONANT SECTIONS. Komatsu et al. (2001) investigated the effects of the information that consonant sections have on perceptual LID using the residual signal. They created the residual signal from 10-second chunks of English and Japanese spontaneous speech. They made the residual signal by LPC (sampling rate: 8 kHz; order of LPC: 16), lowpass-filtered the signals at 1 kHz to ensure spectral removal, and adjusted the intensity of the signals to match their original samples. They made two types of residual signals: the signals that consist of entire speech chunks and the signals in which the consonant sections were suppressed to silence. They conducted the perceptual LID tests with 32 Japanese monolinguals and 10 Japanese-English bilinguals for each type of signal. The subjects were asked to choose one from 'English', 'Probably English', 'Probably Japanese', and 'Japanese' after listening to each stimulus.

The results are shown in Table 5 (overleaf). The index of discriminability (D index) was calculated in such a way that 'English' and 'Japanese' were scored as ±2 while 'Probably English' and 'Probably Japanese' were ±1. Positive values indicate correct responses; and negative, incorrect ones. The averaged D index ranges from -2 to +2, where 0 indicates random responses. It is evident that consonant sections play an important role in LID. The loss of the sonority features of consonants drastically reduces the accuracy of LID.

	Japanese subjects	Bilingual subjects
Stimulus with consonant sections	1.17	1.24
Stimulus without consonant sections	0.35	0.23

Table 5. *D indices in Komatsu et al. (2001).*

These results also suggested variation due to the properties of languages and subjects' linguistic knowledge.

5. CONCLUSIONS. This paper has argued that the LPC residual signal, if its intensity is adjusted, represents the prosodic features in speech signal and that it is usable to test for the role of prosody in perceptual LID. It has paid special attention to the factors relevant to sonority or syllable structure.

The sonority feature is both prosodic and segmental by nature. Because it indicates syllable structures, and hence constitutes rhythm, it is important for perceptual LID. It was shown that the LPC residual signal with its intensity adjusted contains prosodic features including sonority while suppressing segmental features (see sections 4.1 and 4.2). The importance of sonority features was attested (see section 4.3). Therefore, the LPC residual signal properly represents prosody, and it provides the 'acoustic evidence of prosody' in perceptual LID research.

Comparing the LPC residual to other stimuli used in preceding studies, it may be regarded as the addition of noise source to Ohala and Gilbert's signal (1979), which represents the prosody of only voiced sections.

Some of Ramus and Mehler's signals (1999) have information similar to that in the LPC residual. However, their approach to the issue of how to extract prosody is fundamentally different from the method using LPC. They first divide speech into segments such as phonemes, and then substitute those segments by others to exclude the segmental effects of the original speech. Here, they assume some phonological unit into which they can segment speech and operate on the sonority of this unit. On the other hand, the LPC analysis does not assume any segmental unit, and it directly operates on the continuous change of acoustics in speech signal. Operations by Ramus and Mehler's method and LPC may make a difference when the acoustic properties of segments are overlapped temporally, or when phonological units should not be assumed for some research purpose. Ramus and Mehler's method is more sophisticated than the LPC method, but they are not pursuing the same thing. Ramus and Mehler's approach is more 'phonological', and the approach using the LPC residual is more 'phonetic'.

The LPC residual thus can provide another experimental paradigm for perceptual LID research, or the perceptual study on prosodic typology.

and those who joined the discussion at the meeting of Tsukuba Circle of Phonologists (Tsukuba, Japan, 29 Oct. 2000).

REFERENCES

ATKINSON, KAY. 1968. Language identification from nonsegmental cues [Abstract]. *Journal of the Acoustical Society of America* 44:378.

BARKAT, MELISSA, JOHN OHALA & FRANÇOIS PELLEGRINO. 1999. Prosody as a distinctive feature for the discrimination of Arabic dialects. *Proceedings of Eurospeech 99*, 395–98.

CUTLER, ANNE & TAKASHI OTAKE. 1997. Contrastive studies of spoken-language perception. *Journal of the Phonetic Society of Japan* 1.3:4–13.

FOIL, JERRY T. 1986. Language identification using noisy speech. *Proceedings of the International Conference on Acoustics, Speech, and Signal Processing 86*, 861–64.

ITAHASHI, SHUICHI, TOSHIKAZU KIUCHI & MIKIO YAMAMOTO. 1999. Spoken language identification utilizing fundamental frequency and cepstra. *Proceedings of Eurospeech 99*, 383-86.

KENSTOWICZ, MICHAEL. 1994. *Phonology in generative grammar*. Cambridge MA: Blackwell.

KOMATSU, MASAHIKO, TAKAHITO SHINYA, MIYUKI TAKASAWA & TAKAYUKI ARAI. 2000. LPC zansa-shingo ni yoru shiin-ninshiki to onso-hairetsu [Consonant perception in LPC residuals and phonotactics]. *Proceedings of the the 14th general meeting of the Phonetic Society of Japan*, 49-54.

———, WON TOKUMA, SHINICHI TOKUMA & TAKAYUKI ARAI. 2000. The effect of reduced spectral information on Japanese consonant perception: Comparison between L1 and L2 listeners. *Proceedings of the International Conference on Spoken Language Processing 2000*, vol. 3, 750–53.

———, KAZUYA MORI, TAKAYUKI ARAI & YUJI MURAHARA. 2001. Human language identification with reduced segmental information: Comparison between monolinguals and bilinguals. *Proceedings of Eurospeech 2001*, 149–52.

MAIDMENT, JOHN A. 1976. Voice fundamental frequency characteristics as language differentiators. *Speech and hearing: Work in progress* 2:74–93. University College London.

———. 1983. Language recognition and prosody: Further evidence. *Speech, hearing and language: Work in progress* 1:133–41. University College London.

MOFTAH, ABDELFATTAH & PETER ROACH. 1988. Language recognition from distorted speech: Comparison of techniques. *Journal of the International Phonetic Association* 18:50–52.

MORI, KAZUYA, NORIAKI TOBA, TOMOYUKI HARADA, TAKAYUKI ARAI, MASAHIKO KOMATSU, MAKIKO AOYAGI & YUJI MURAHARA. 1999. Human language identification with reduced spectral information. *Proceedings of Eurospeech 99*, 391–94.

MUGITANI, RYOKO, AKIKO HAYASHI & SHIGERU KIRITANI. 2000. Yoiku-kankyo ni aru hogen e no senko-hanno no hattatsu [Developmental change of 5 to 8-month-old infants' preferential listening response for the native dialect]. *Journal of the Phonetic Society of Japan* 4.2:62–71.

MUTHUSAMY, YESHWANT K., ETIENNE BARNARD & RONALD A. COLE. 1994. Reviewing automatic language identification. *IEEE signal processing magazine* 11.4:33–41.

OHALA, JOHN J. & JUDY B. GILBERT. 1979. Listeners' ability to identify languages by their prosody. In *Problèmes de prosodie, vol. 2, Expérimentations, modèles et fonctions* (*Studia phonetica 18*), ed. by Pierre Léon & Mario Rossi, 123–31. Paris: Didier.

RAMUS, FRANCK & JACQUES MEHLER. 1999. Language identification with suprasegmental cues: A study based on speech resynthesis. *Journal of the Acoustical Society of America* 105: 512–21.

———, Marina Nespor & Jacques Mehler. 1999. Correlates of linguistic rhythm in the speech signal. *Cognition* 73: 265-92.

TAJIMA, KEIICHI. 1998. *Speech rhythm in English and Japanese: Experiments in speech cycling*. Indiana University Bloomington Ph.D. dissertation.

THYMÉ-GOBBEL, ANN E. & SANDRA E. HUTCHINS. 1996. On using prosodic cues in automatic language identification. *Proceedings of the International Conference on Spoken Language Processing 96*, 1768–71.

WARNER, NATASHA & TAKAYUKI ARAI. 2001a. Japanese mora-timing: A review. *Phonetica* 58:1–25.

———. 2001b. The role of the mora in the timing of spontaneous Japanese speech. *Journal of the Acoustical Society of America* 109:1144–56.

THE *LAH* PARTICLE IN SINGAPORE ENGLISH:
A RELEVANCE-THEORETIC APPROACH

Vivien Soon Lay Ler
National University of Singapore

Discourse particles in singapore english, such as *lah, lor*, *hah*, and *hor*, which give Singapore Colloquial English (sce) its special flavour, belong to the most frequent words used in spontaneous dialogues. They fulfill many pragmatic functions with respect to a number of linguistic and interactional domains. The meanings and functions of these particles have been the subject of previous discussion (e.g., Kwan-Terry 1978, Platt 1987, Gupta 1992, Pakir 1992).

In the present paper, I concentrate on the use of the most frequently occurring discourse particle in sce, the particle *lah*. In the top-five list of particles in the spoken categories in ice-sin[1], *lah* ranks first with 1,742 occurrences. The particle that ranks second is *ah* with 1,242 occurrences, followed by *hah* with 256 occurrences.

People use *lah* to convey the mood and attitude of the speaker (oed 2000). For example, 'Go to Chinatown *lah*' is used as a suggestion. In Brown (2000:127), *lah* is used with a request or command to indicate impatience (e.g., 'Finish your food *lah*') or to turn the utterance into a plea (e.g., 'Give me more time *lah*'). *Lah* has many functions (cf. Section 1). Is there a single meaning of *lah* compatible with the varying uses of *lah* in actual discourse? That is, what kind of cognitive information does *lah* encode? The answers to these questions will unlock one of the most interesting mysteries in intercultural pragmatics.

I argue that we need a unified meaning of *lah* to understand how it is interpreted in discourse. This can be done by a study of the cognitive processes involved in utterances concerning the meaning of *lah* in terms of three fundamental issues: is the meaning of *lah* conceptual or procedural; is it truth-conditional or non-truth-conditional; and how does it constrain the utterance. The data in my study are from two main sources. The first is the recently completed ice-sin, which is by far the most comprehensive collection of Singapore English. The second is personal conversations or statements overheard at churches or canteens and recorded either at the time or immediately thereafter.

1. previous accounts. *Lah* has been discussed by many writers, but there is considerable disagreement as to its use and functions. Previous descriptions of *lah* are based on a few examples; only Richards and Tay (1977) included a telephone conversation in their study, and they used only a fragment of the whole.

The earliest account of *lah* is by Tongue—depending on the way it is pronounced, *lah* can function as an 'intensifying particle, as a marker of informal style, as a signal

of intimacy, for persuading, deriding, wheedling, rejecting and a host of other purposes' (1974:114). Tongue's account of *lah* is the first to treat it as characteristic of SCE.

Lah is treated as a marker of rapport or solidarity (Tongue 1974; Richards & Tay 1977; Kwan-Terry 1978; Bell & Ser 1983; Pakir 1992) and as a marker of emphasis (Richards & Tay 1977; Platt et al. 1983; Loke & Low 1988). It has also been said to communicate a range of attitudes, such as obviousness, persuasion and impatience. Other functions adduced include friendliness, hostility, and annoyance. It is also described as an indicator of enthusiasm and assertion or as a word communicating objection. This list is not exhaustive. The main functions, as described by past researchers, are shown in the following examples. (Unless indicated otherwise, the examples are from my personal collection or from ICE-SIN).

1.1. SOLIDARITY. *Lah* is first described as a marker of rapport or solidarity, comparable to the English 'filler' *you know*:

(1)   Don't be shy *lah.* [We are friends]
(2)   No use trying to hide our roots *lah.* [We are Singaporeans]

However, this classification cannot accommodate data such as (3), where no element of rapport or solidarity can be detected:

(3)   Context: A mother (A) and her daughter (B) have a disagreement on
       who is to buy Mandarin oranges. (It is customary for the Chinese to
       exchange Mandarin oranges when visiting during the Chinese New Year).
       A: Then after that it's the Lunar New Year special *lah.*
       B: So?
       A: Ya *lah,* then during that period we can go what?
       B: Cannot *lah.* Aiyah, when I wash my hair, I don't want to go out.
            Dirty my hair *lah.*
       A: You bring one of them *lah.* (ICE-SIN-S1A-007)

1.2. EMPHASIS. According to some researchers, *lah* contributes an element of emphasis to sentences such as (4) and (5):

(4)   Do you want to go? I'm not going *lah.* [Emphasis]
       (Kwan-Terry 1992:69)
(5)   Normal doctors *lah* who are on our medical panel. [not specialists]
       (ICE-SIN-S1B-073)

But emphasis does not explain (3).

1.3. OBVIOUSNESS. *Lah* is often said to convey the speaker's attitude of 'obviousness'. There is also a note of impatience or annoyance in these cases:

(6) They generally don't take beef *lah*. [It's obvious; everybody knows that.]
     (ICE-SIN-S1A-023)

(7) I mean <u>of course</u> it changes *lah*. (ICE-SIN-S1A-065)

Obviousness might explain (3), but it does not explain (1) and (2).

1.4. PERSUASION. *Lah* can also be used with a certain tone to persuade or to suggest:

(8) Come with us *lah*. [Won't you?] (OED 2000)

(9) Go to Chinatown *lah*. [Why don't you?] (ICE-SIN-S1A-007)

Again, persuasion does not explain (3).

1.5. FRIENDLINESS. *Lah* is sometimes used when the speaker wants to be friendly:

(10) Okay, doesn't matter *lah*. [It's all right; we're friends.] (ICE-SIN-S1A-091)

(11) Quite nice *lah*. [I'm your friend; consider my opinion.]
      (ICE-SIN-S1A-023)

Friendliness does not explain (3) either.

1.6. HOSTILITY. Sometimes, *lah* is described as conveying a sense of 'hostility':

(12) If you want then it should be after this week *lah*. [Not earlier!]
      (ICE-SIN-S1A-091)

(13) I don't want to eat *lah*. [Don't force me!]

The list of functions ascribed to *lah* in (1)–(13) is not exhaustive. But simply calling it polysemous or multifunctional is inadequate. This can be seen by comparing the same utterance with and without *lah*:

(14) a. I mean of course it changes *lah*! (ICE-SIN-S1A-065)
     b. I mean of course it changes!

The 'obviousness' in (14) is evident even without the particle. It is in the semantics of the utterances, as indicated by *of course* in (14). Hence, 'obviousness' cannot be characterised as an inherent part of *lah*. Similarly 'persuasion' in (8) and (9), 'friendliness' in (10) and (11), and 'hostility' in (12) and (13), may have other sources, such as intonation or tone. These meanings (in (8)–(14) are preserved even when *lah* is omitted. Thus these functions are not inherent in the particle itself.

What is most striking about the above descriptions is that they tell us different things about the particle. Some of the descriptions contradict others (e.g., friendliness vs. hostility). The reason for the varied descriptions is that none of them give a complete

picture of *lah*. Thus, while such findings shed some light on what *lah* does, a general description does not yet exist. Specifically, how does *lah* operate in discourse? Nor have its contradictory functions in imperatives (a softening effect) and declaratives (a strengthening effect) been explained. In my account, we shall see why this is so.

2. A RELEVANCE-THEORETIC ACCOUNT.

2.1. THE *LAH* PARTICLE. *Lah* can be attached to declaratives in (15), imperatives in (16), and some interrogatives, e.g., (17). The range of *lah* is shown in (15)–(18). It was first shown by Bell and Ser (1983) that *lah* need not necessarily occur in sentence- or clause-final position, as shown in (18)a–c (Bell & Ser 1983) and in (18)d.

(15) a. She's quite playful *lah*. (ICE-SIN-S1A-091)
     b. No *lah*! This way cannot! Miss turn already. (*The Straits Times* 6 Apr 2001)
(16) a. Come on *lah*. (ICE-SIN-S1A-065)
     b. Bring one of them *lah*. (ICE-SIN-S1A-007)
(17)   What's in fashion *lah*? (ICE-SIN-S1A-003)
(18) a. Must *lah* have been cooking.
     b. Must have been *lah* cooking.
     c. That great hawker *lah* from Newton Circus.
     d. Normal doctors *lah* which are on our medical panel. (ICE-SIN-S1B-073)

However, there are constructions that do not allow *lah*. Consider (19-21):

(19)   *Are you going home *lah*?
(20)   *He's asleep *lah*?
(21)   *Where are you going *lah*? [seeking information]

Examples (19)–(21) illustrate the unacceptability of using *lah* in yes/no interrogatives (19), declarative interrogatives (20), and wh-interrogatives seeking factual information (21). These restrictions are not in the literature, and an explanation should be part of the full description of *lah*[2].

    I suggest that *lah* is a procedural, non-truth-conditional particle that contributes to the explicature of an utterance. It encodes procedural information about the speaker's desire for the hearer to recognise the shared assumption(s) behind the utterance. To explain this I turn to those ideas of relevance theory that are pertinent to my account, specifically to the notion of constraints on relevance (Sperber & Wilson 1995; Blakemore 1987).

2.2. THEORETICAL FRAMEWORK. A recently developed pragmatic framework, Relevance Theory (Sperber & Wilson 1995), is a general theory of communication based on cognitive principles. It looks at utterances as inputs to inferential processes which affect the cognitive environment of the hearer. In this account of communication, interpretation of utterances is not merely a matter of linguistic decoding but relies

heavily on inference. The process of utterance interpretation is governed by the principle of relevance, that is, 'every act of ostensive communication communicates a presumption of its optimal relevance' (Sperber & Wilson 1995:176).

Relevance is seen as a combination of the effects gained and the cost of processing the utterance (the greater the contextual effects, the greater the relevance; the smaller the processing effort to arrive at the intended interpretation of an utterance, the greater the relevance). In utterance interpretation, the hearer begins by decoding the utterance linguistically, which often involves reference assignment, disambiguation and enrichment[3]. If the hearer does not find the explicit context optimally relevant, he will search for other contextual assumptions (by calling to mind accessible premises) which will enable him to reach the intended conclusion. Consider (22):

(22)   The coach was cold.

In (22), the sentence is ambiguous: 'coach' can refer to someone who coaches a sport or to a vehicle, and 'cold' can be a certain physical condition or a certain attitude. In the right context, (22) is no longer ambiguous. Consider (23)a–b:

(23) a. We started basketball lessons last week.
     b. The game was fun but the coach was cold.

Another important postulate of Relevance Theory is that expressions in language can be seen to encode not only concepts but also procedures. Such expressions guide the hearer in the process of utterance interpretation and contribute to relevance by reducing the processing effort needed to reach the intended interpretation (Blakemore 1987). *Lah* encodes procedural meaning.

The processing effort is reduced by the effect of constraints on relevance (Sperber & Wilson 1995), i.e., by making the hearer's context set smaller. The analysis of (24) and (25) provides a better understanding of constraints on relevance.

(24)   Benjamin Bratt likes to please Julia Roberts.
(25)   He loves Julia Roberts.

(24) and (25) can be construed as being in a variety of relations. For example, they could be just two facts or beliefs, or one could be construed as giving evidence for the truth of the other. In that case, one is the conclusion, while the other supports it. Because either can be the conclusion, there is the possibility of misinterpretation. In such cases, Blakemore (1987) argues that constraints on relevance play a vital role. Consider (26) and (27):

(26) a. Benjamin Bratt likes to please Julia Roberts.
     b. *After all*, he loves Julia Roberts.

(27) a. Benjamin Bratt likes to please Julia Roberts.
    b. *So*, he loves Julia Roberts.

Although the two utterances are in the same order in (26) and (27), they are related in different ways. In (26), (26)a is the conclusion with (26)b providing the evidence. In (27), (27)b is the conclusion with (27)a giving the evidence. In both cases the speaker expects the hearer to have further contextual assumptions available, and they are not the same for (26) as they are for (27). Thus, in (26), the speaker expects the hearer to access assumption (26'):

(26') If X loves someone then X likes to please this person.

In (27), however, the assumption is (27'):

(27') If X likes to please someone then X loves this person.

Thus *after all* and *so* constrain the processing of the two utterances in different ways.

Blakemore argues, on the basis of examples such as (26) and (27), that words such as *so* and *after all* do not contribute to the truth-conditional content of the utterance in which they occur and that they do not encode conceptual meaning. Their role is to help 'constrain the hearer's choice of context for its interpretation' (1987:141).

3. ANALYSIS AND DISCUSSION. It is now possible to interpret the particle *lah* using a Relevance Theory approach. Consider (28):

(28)   Context: A and B are discussing how the economic downturn has affected
       business and as a consequence organisations have to be prudent to
       protect the interests of shareholders.
       A: So you know we are not spared *lah*. Okay we are not spared *lah*.
       B: Uhm nice to know that I am not alone in all this.
       A: You are not spared okay. (ICE-SIN-S1B-077)
(29)   A's contextual assumptions include 5 premises:
       Premise 1. The economic downturn has affected A's business.
       Premise 2. A knows that other businesses have been also affected by the
                     downturn.
       Premise 3. A wants B to know that his business has been affected by the
                     downturn.
       Premise 4. A wants to assure B that he has his sympathy.
       Premise 5. A knows that he has to be prudent.

A's intention in the utterance is not only to inform B that they are not spared the consequences of the economic downturn but also to indicate the speaker's desire for the hearer to recognise the shared assumption Premise 4. This is effected by *lah*.

The shared assumption is recognised when B says that it is nice to know that he is not alone in all this. B feels reassured. Let us now look at a *lah*-appended utterance involving an interrogative:

(30)   Context: A and B are discussing the latest fashion.
         B: What is in thing?
         A: What's in fashion *lah*?
         B: Couldn't really identify any fashion. (ICE-SIN-S1A-003)
(31)   A's contextual assumptions include 5 premises:
         Premise 1: B has not given A the right response.
         Premise 2: A is not happy that B does not know what is the 'in thing'.
         Premise 3: A feels that B should know what she means.
         Premise 4: A wants B to know that she disapproves of B's ignorance.
         Premise 5: A feels that she does not have to spell out what 'in thing' is.

Lah leads the hearer to access and consider assumptions implicitly communicated by an utterance, assumptions that may or may not be retrieved in the absence of the particle. The assumption made accessible by A's comment in (30) is something like Premise 4 in (31). The hearer would choose the first interpretation coherent with the principle of relevance. B knows that A is reproachful of her (B's) ignorance and is showing it indirectly. In attaching *lah* to her utterance, A shows that she expects more from B than just an answer to her question. B's reply 'Couldn't really identify any fashion' appears to answer the question as part of the 'repair' to her recognition of A's reproach. B's reply would most probably be accompanied by non-verbal linguistic expressions such as a soft tone, an apologetic look, thus indicating that she knows what A meant. *Lah* helps convey the speaker's desire for the hearer to recognise the shared assumption made manifest in the context (Premise 4, which is similar to what B has in her cognitive environment). In other words, the speaker desires that her (informative) intention to make manifest the shared/common assumption be fully recognised by the hearer.

Our analysis that *lah* signals the speaker's desire that the hearer recognise the shared assumption behind the utterance can explain all the communicative effects ascribed to *lah* in the literature (cf. Section 1). It accounts for the solidarity and rapport felt between communicators. If I make known to you that there are shared assumptions between us, I am treating you as someone I can relate to, as a member of a certain community which is also mine. In so doing, I create an impression of rapport between us.

The present account also explains why (8), 'Come with us *lah*', is seen as more polite than 'Come with us'. That is, *lah* makes imperatives more polite ('weakening' imperatives). In (8), *lah* invites the hearer to recognise the speaker's desire to have the same shared assumption behind the utterance. To explain this, it is necessary to look at what mood a sentence encodes, and how this affects the way relevance is achieved. According to Sperber and Wilson (1988), the indicative mood shows that the thought

communicated by the utterance is entertained as a true description of an actual state of affairs. The imperative mood, on the other hand, indicates that the thought communicated is entertained as a true description of a potential and desirable state of affairs. In a command or request, *lah* has the effect of asking the hearer to find and accept how relevant it is for the *speaker* to achieve some potential and desirable state of affairs. In a suggestion or persuasion, *lah* has the effect of asking the hearer to find and accept how relevant it is for the *hearer* to achieve some potential and desirable state of affairs.

The present account also explains why *lah* added to an imperative can appear to be persuading or pleading. In (8), *lah* instructs the hearer about the speaker's desire to have the hearer recognise the shared assumption behind the utterance. In a context where the hearer appears unable to recognise the speaker's intention to draw on a shared assumption (e.g., it will be good for the hearer to go along with them), *lah* can be interpreted as an attempt to persuade the hearer to accept the speaker's point of view.

That *lah* appended to utterances adds an element of annoyance or impatience can also be accounted for in our explanation of the particle. If the hearer appears not to recognise the shared assumption as desired by the speaker, then the speaker's insistence that he do so may have a touch of annoyance or impatience. This does not mean that *lah* contains annoyance or impatience in its semantic make-up.

The various functions of *lah* as described by previous researchers, such as obviousness, friendliness, consultativeness, can be subsumed under our description of the particle. *Lah* encodes the speaker's desire that the hearer recognise a shared assumption behind the utterance, which in turn, functions as an explicit guarantee of relevance. As a consequence of such an explicit guarantee of relevance, the hearer is encouraged to expand the contextual assumptions in order to obtain the intended contextual effects.

I have shown so far that *lah* is used when the speaker intends to draw on the shared assumptions behind the utterance. There are two ways in which linguistic meaning contributes to the interpretation of utterances. It may encode conceptual meaning[4], on the one hand, or it may contain procedural information, that is, instructions for processing propositions (Blakemore 1996:151). In the case of *lah*, the particle appears to belong to the group of linguistic entities that encode procedural meaning. In my analysis, *lah* does so by signalling the speaker's intention that the shared assumption be fully recognised by the hearer. In turn, this functions as a guarantee of relevance.

The distinction between truth-conditional meaning and non-truth-conditional does not equal the distinction between conceptual meaning and procedural meaning. For example, sentence adverbials such as 'frankly' and 'seriously' are treated as non-truth-conditional, but they encode conceptual meaning (Wilson and Sperber 1993; Blakemore 1992). In the case of *lah* in sce, the particle can be omitted without affecting the truth-conditions of the host utterance. Consider (6), reproduced here as (32)a:

(32) a. They generally don't take beef *lah*. (ICE-SIN-S1A-023)
     b. They generally don't take beef.

For both (32)a and (32)b, the proposition is the same, namely, that there is a group of people who generally do not eat beef. If the particle is omitted, as in (32)b, there is no loss in propositional meaning. *Lah* instructs the hearer about the speaker's desire for the hearer to recognise the shared assumption behind the utterance, that is, what the speaker wants to say is not 'They generally don't take beef' but that the speaker need not spell this out; the hearer should be able to gather that from the context. In this way, the presence of the particle allows the hearer to process the utterance in the smallest context, thereby making for optimal relevance.

4. CONCLUSION. The present account of *lah* has several advantages. First, it explains the difference between an utterance with *lah* and one without *lah*, which has not been adequately dealt with in the previous studies. A relevance-theoretic account of *lah* explains the difference in the following manner. By providing an overt guarantee of relevance, *lah* can guide the hearer to explore assumptions implicitly communicated by an utterance with *lah*, including contextual assumptions intended by the speaker. An utterance without *lah* does not have this encouragement.

Next, I argue that *lah* is procedurally used by the speaker and the various uses of this particle can justifiably be subsumed under a single description of a code marker instructing the hearer to recognise the shared assumption behind the utterance. With this unified meaning, glosses of the type presented by the previous studies become redundant.

---

[1]   The lexical corpus of ICE-SIN (International Corpus of English, Singapore component) was completed at the Department of English Language and Literature, the National University of Singapore, in April, 2000. Its completion was achieved thanks to the NUS-funded project *A Study of Definite Noun Phrases in Singaporean and British Discourse* (RP3982058). It is a one million-word corpus, consisting of 500 texts (200 written and 300 spoken) of approximately 2000 words each. The data used in this study is taken mainly from the spoken texts.

[2]   Space considerations preclude a discussion of the restrictions on *lah*.

[3]   Semantic representations often have to be enriched. For example, in. 'The bat is too grey', the adverb *too* is semantically incomplete. The bat is too grey *for something*. (For further details, please refer to Sperber & Wilson 1995:188–89).

[4]   The distinction between conceptual and procedural meaning is a useful one, as it expresses the intuition that there are different types of linguistically encoded information. It is also useful in explaining expressions that do not affect the propositional content of an utterance. Due to constraints of space in this paper, a detailed account is not possible. I refer the reader to the literature (Blakemore 1987, 1992; Wilson & Sperber 1993).

REFERENCES

BELL, ROGER & LARRY PENG QUEE SER. 1983. 'Today la!' 'Tomorrow *lah*!', the *la* particle in Singapore English. *RELC journal* 14/2:1–18.

BLAKEMORE, DIANE. 1987. *Semantic constraints on relevance.* Oxford: Blackwell.

———. 1992. *Understanding utterances.* Oxford: Blackwell.

BROWN, ADAM. 2000. *Singapore English in a nutshell: an alphabetical description of its features.* Singapore: Federal Publications.

GUPTA, ANTHEA F. 1992. The pragmatic particles of Singapore Colloquial English. *Journal of pragmatics* 18:31–57.

KWAN-TERRY, ANNA. 1978. The meaning and source of the *la* and *what* particles in Singapore English. *RELC journal* 9/2:22–36.

LOKE, KIT-KEN & JOHNA MEI-YIN LOW. 1988. A proposed descriptive framework for the pragmatic meanings of the particle 'la' in colloquial Singaporean English. In *Asian-Pacific papers: regional papers presented at the 8ᵗʰ World Congress of Applied Linguistics, University of Sydney, 16-21 August 1987*, ed. by Brian McCarthy, 150–61. Australia: Applied Linguistics Association of Australia.

SIMPSON, JOHN. 2000. *Oxford English Dictionary*, INTERNET edition. Oxford University Press. http://oed.com. (Referred to in the body of the article as OED 2000)

PAKIR, ANNE. 1992. Dictionary entries for discourse particles. In *Words in a cultural context*, ed. by Anne Pakir, 143–52. Singapore: UniPress.

PLATT, JOHN. 1987. Communicative functions of particles in Singapore English. In *Language topics: essays in honour of Michael Halliday,* vol. 1, ed. by Ross Stelle & Terry Threadgold, 391–401. Amsterdam: John Benjamins.

———, HEIDI WEBER & MIAN LIAN HO. 1983. *Singapore and Malaysia: varieties of English around the Wo*rld. Amsterdam: John Benjamins.

RICHARDS, JACK & MARY TAY. 1977. The 'la' particle in Singapore English. In *The English language in Singapore*, ed. by William J. Crewe, 14-56. Singapore: Eastern Universities Press.

SPERBER, DAN & DEIRDRE WILSON. 1995. *Relevance: communication and cognition.* Oxford: Basil Blackwell. (Originally published in 1986.)

TONGUE, RAY. 1974. *The English of Singapore and Malaysia*. Singapore: Eastern Universities Press.

WILSON, DEIRDRE. 1999. Relevance and Relevance Theory. In MIT *encyclopedia of cognitive sciences*, 719–20. Cambridge MA: MIT Press.

——— & DAN SPERBER. 1988. Mood and the analysis of non-declarative sentences. In *Human agency: Language, duty and value*, ed. by Jonathan Dancy, J.M.E. Moravcsik & C.C.W. Taylor, 77–101. Stanford: Stanford University Press.

——— & ———. 1993. Linguistic form and relevance. *Lingua* 90:1–25.

☙

WHAT LANGUAGE STABILITY TELLS US ABOUT
LANGUAGE CHANGE, AND VICE VERSA

Mary S. MacKeracher
University of Toronto

LANGUAGE CHANGE has been studied by many linguists, but language stability or the lack of change has received little attention. In this paper, I look at what S-curve change, the most common form of language change, predicts about language stability: stability at near-categorical levels is the only possible result of language innovation. I proceed by first presuming the internal-influence logistic model can adequately represent S-curve change and then I expose some of its inherent assumptions. Next, using data from four projects of the Dialect Topography of Canadian English, I illustrate that this model is appropriate for many of the variables investigated.

However, some of the language variation patterns do not conform to this model. Stable variation, or stability at middling frequencies of use, is one such pattern. But when I slightly alter the initial mathematical model, this uncommon but not rare phenomenon is accommodated. The new model now predicts that language change will end in stability at either near-categorical or middling frequencies.

This adaptation inspires us to ask if the model with additional modifications can be made to accommodate other less common change patterns. I show that with small alterations we can accommodate language reversal, that is, when an innovation fails to achieve lasting prevalence.

We should note that language change is only one type of human innovation. Everett M. Rogers has created a theoretical paradigm on the basis of over four thousand works from many disciplines on human innovation. Rogers (1995:10) defines innovation diffusion as 'the process by which an *innovation* is *communicated* through certain *channels* over *time* among the members of a *social system*. The four elements are the innovation, communication channels, time, and the social system'. I will discuss language change using this paradigm.

1. S-CURVE LANGUAGE CHANGE: THE STANDARD MODEL. The most common pattern of language change is S-shaped (Bailey 1973:77, Inoue 1997:79, Labov 1994:65). The S-curve diffusion pattern is illustrated by Figure 1 (overleaf).

An innovation begins slowly, with only a small percentage of the society using the new variant. When the innovation has been adopted by 10–20% of the population, it reaches a 'critical mass', the number of people needed for diffusion to take off, or more precisely, the point at which the innovation's rate of adoption becomes self-sustaining. It then proceeds more quickly, as the majority of people adopt the innovation. And finally the change slows when almost all use the variant (Rogers 1995:261–63).
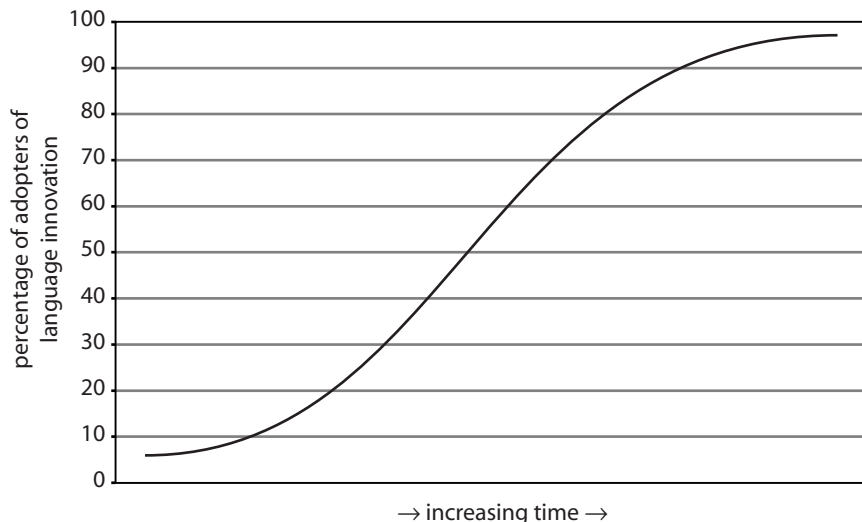
Figure 1. *The S-curve diffusion pattern.*

This pattern can be represented by many mathematical models, the two most commonly used being the logistic and cumulative normal statistical distributions (Griliches 1957:503, Labov 1994:65, Mahajan & Peterson 1985:10). For ease of manipulation, I work with the logistic distribution. The simplest version of this distribution that I feel reflects language change is the internal-influence model and it will serve as our starting point.

2. THE INTERNAL-INFLUENCE LOGISTIC MODEL. The fundamental premise of the internal-influence logistic model is that the innovation propagates throughout the chosen society or population through 'horizontal' channels, that is, solely through personal contact between members of the society (Mahajan & Peterson 1985:17). The mechanism of change is the social interaction between prior adopters of the innovation and current-non-adopters-but-potential-adopters. Figure 2 gives the differential version of the internal-influence logistic model (adapted from ibid:17–21).

The first line is the actual equation. Below it are two notes; the first gives the boundary condition for the differential version if it is to be integrated and the second indicates what we solve for. Following the notes is a description of the variables, constants, parameters (actually just one parameter here), and some of the composite expressions in the equation.

We express the number of people who adopt the language innovation as a function of time: the range variable is *N*, the number of people who adopt the language innovation, and the domain variable is *t*, time. In the differential version, the incremental number of people who will adopt the innovation at time *t* forms the left side of equation. It is equal to the probability of adopting the innovation (the first term of the right side) multiplied by the number of potential adopters who have yet to adopt

$$\frac{dN(t)}{dt} = \boldsymbol{b}N(t) \times [\bar{N} - N(t)]$$

1.   with boundary condition: at $t = t_0$ (start of the diffusion process), $N(t_0) = N_0$.
2.   where parameter $\boldsymbol{b}$, indicated in bold, is to be determined.

$N(t)$ is the cumulative number of adopters of the innovation at time t

$\dfrac{dN(t)}{dt}$ is the rate of diffusion of the innovation at time t

$\boldsymbol{b}$ is the rate of adoption of the innovation
$\boldsymbol{b}N(t)$ is the coefficient of diffusion for the internal-influence model; it is the second term of
    the general coefficient of diffusion, $g(t) = a + \boldsymbol{b}N(t) + cN(t)^2 + \ldots$
    (consider the coefficient of diffusion as the probability of adoption of the
    innovation at time t)
$\bar{N}$ is the total number of potential adopters in the social system
$[\bar{N} - N(t)]$ is the number of potential adopters who have not yet adopted the innovation
    at time t (it is the difference between the total number of potential adopters, $\bar{N}$, and the
    number of previous adopters, $N(t)$, at time t)

Figure 2. *Differential version of internal-influence model (adapted from Mahajan & Peterson 1985:17–21).*

the innovation (the second term of the right side). We solve not for the variables t and N, which are known from survey data, but for the parameter b, the rate of adoption of the language innovation.

Note that the integral version (not given in this paper) is what actually produces the S-curve of Figure 1. Because its meaning is not particularly transparent, I use its corresponding differential version here.

3. THEORETICAL PREDICTIONS AND ASSUMPTIONS OF THE MODEL. It is my impression that those who discuss the S-curve model presume an innovation is in *either* a state of stability *or* a state of change (non-stability). Technically speaking, this is a notional or qualitative reading of the varying rates of change along the S-curve, but because this idea seems to be generally accepted, I shall presume that the model may be interpreted in this way.

Therefore, when we examine Figures 1 and 2, we see that S-curve change as represented by the internal-influence logistic model makes two predictions about stability. First, stability occurs only before or after change, never as an intermediate stage of change. And second, stability occurs only at near-categorical levels of frequency of the population (close to the 0 or 100% asymptotes of N).

In addition to these predictions concerning stability, the model makes many assumptions (Mahajan & Peterson 1985 passim; Rogers 1995:260), three of which I list here. First, as mentioned above, change or the rate of adoption of the innovation is solely a function of time. Second, also discussed above, the innovation propagates through internal, personal contact (internal influences), and not through any influence from change agents outside of the population (external influences). And third,
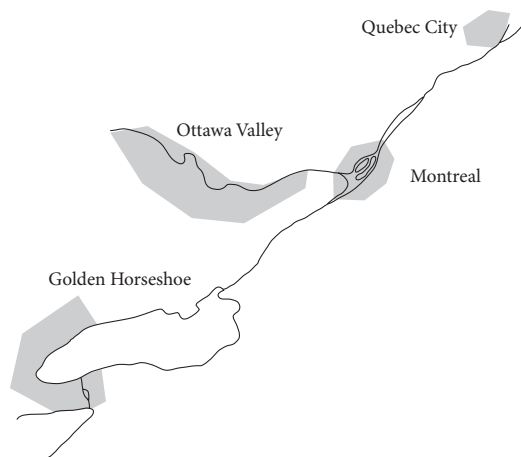
Figure 3. *Canadian English Dialect Topography projects.*

the innovation is 'successful', that is, all of the population adopts it and nothing supplants it (Rogers 1995:260).

Looking at the predictions and assumptions together, we may question whether the S-curve model and our mathematical representation are in any way robust. Are they good models, that is, do they adequately reflect most if not all of the patterns we encounter in language variation? And we can pose more specific questions. First, how are sociolinguistic factors incorporated into the model? We sociolinguists would prefer a model that reflects the influence of these potentially explanatory factors to a model that merely says at a given time A, the number of adopters will be B. Second, what is the status of stable variation, that is, stability at middling frequencies? Is it a natural exception to the model, or a case which ought to be modeled too? Third, what exactly is an unsuccessful innovation and can it be modeled? And fourth, what are external influences and can they be incorporated into the model as well as internal influences?

4. DATA AND METHODOLOGY. The data I use here are from the Dialect Topography of Canadian English (Chambers 1994 passim, *inter alia*). It is a language survey that has been completed in four regions to date, namely the Golden Horseshoe, the Ottawa Valley, Montreal, and Quebec City (see Figure 3). The instrument is a self-report postal questionnaire. It consists of: 11 personal types of questions (i.e., age, sex, education, places where the respondent is currently living and was living when aged 8 to 18, birthplaces of the respondent and parents, and occupations of the respondent and parents); 4 English Language Use questions, for all projects except the Golden Horseshoe; and 81 linguistic questions.

I have analyzed 45 linguistic variables for the four projects. In particular, I have investigated the behavior of the 'dependent' linguistic variable with respect to the 'independent' or potentially explanatory variables of region/central place, age, sex, social class, education, and three indices devised by J. K. Chambers: Regionality Index,
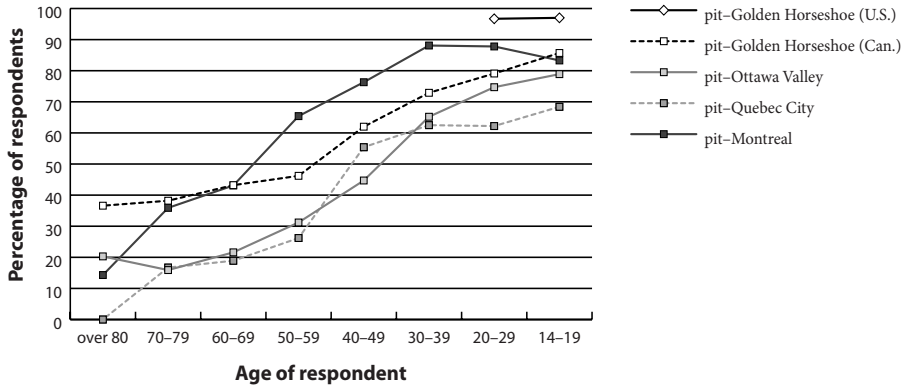
Figure 4. *Age chart for (peach) – pit.*

Occupational Mobility Index, and Language Use Index. In this paper, I discuss 4 variables: (peach), (sick), (either), and (mom). And I will concentrate primarily on the behavior of the linguistic variable with respect to age.

There are two major assumptions that I make here. First, I presuppose the critical age hypothesis, that an individual does not alter his or her speech after a 'critical age', say around puberty. I make this assumption in order to presuppose the apparent time hypothesis, that age can stand for time. I must make this latter assumption if I wish to use a cross-sectional instrument to study diffusion, an intrinsically longitudinal problem. The second assumption is that the standard or expected temporal pattern of language change is modeled by the internal-influence logistic model.

5. VARIABLES THAT EXHIBIT THE STANDARD S-CURVE PATTERN.
5.1. PEACH. The variable (peach), investigated in an open-ended question, is bivariate: in all areas, 85% of the respondents answered either *pit* or *stone* to the following question:

In the middle of a peach you always find a _____ .

As shown in Figure 4, the age chart for the innovative variant *pit*, its adoption pattern across apparent time (age) is a typical, complete S-curve. The adoption of *pit* begins slowly in the earliest times (oldest age groups), then speeds up in more recent times (middle-aged groups), and then slows in the most recent times (youngest age groups). The change ends in stability at near-categorical levels of adoption of this variant.

5.2. SICK. The variable (sick) concerns how the adjective *sick* is modified by the noun *stomach*. Results from the following closed-ended question reveal that the variable is not particularly variable: it is almost univariate.
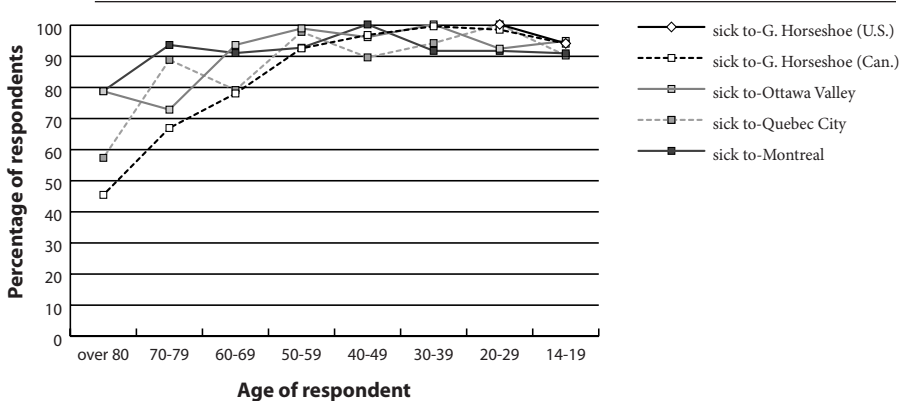
Figure 5. *Age chart for (sick) – sick to my stomach.*

       Which do you say?   ❐ Yesterday I was sick at my stomach.
                               ❐ Yesterday I was sick in my stomach.
                               ❐ Yesterday I was sick to my stomach.
                               ❐ Yesterday I was stomach-sick.

In all areas, the frequency of *sick to my stomach* is at least at the low eightieth percentile. Figure 5, the age chart for the innovative variant *sick to my stomach*, shows the variable is first changing or non-stable, then becomes stable in all regions. We see a typical S-curve in its last phase. The change ends in a long period of stability at near-categorical levels.

    Now let us begin to answer the questions posed in section 3. First, how are sociolinguistic factors incorporated into the internal-influence logistic model of Figure 2? They are not incorporated directly into the model, but instead, indirectly: they affect the value of parameter *b*. If social factors conspire to promote the language innovation aggressively, the value of parameter *b* will be relatively large, but if social factors promote the innovation less aggressively, the value of parameter *b* will be relatively small. Simply put, the more rapid the change, the larger the value of parameter *b*. We should note that the value of parameter *b* is the only avenue in which social factors, the potentially explanatory variables, are incorporated into the model. We may ask whether we can do better: can we involve the social factors more directly into the model and how? Let us see what happens when we try to alter the mathematical model to accommodate more language change patterns.

6. TWO VARIABLES THAT DO NOT EXHIBIT THE STANDARD S-CURVE PATTERN.
6.1. STABLE VARIATION: *EITHER*. The well-known bivariate variable (either) is investigated in the following closed-ended question:

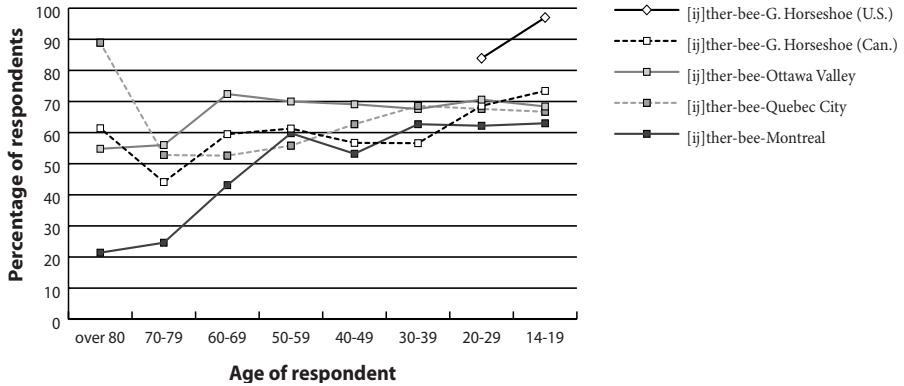   Is the <u>ei</u> of <u>EITHER</u> pronounced like the ❐ ie of pie, or the ❐ ee of bee?

Figure 6. *Age chart for (either) – [ij]ther-*bee *([ij]ther which rhymes with the word* bee*).*

In all areas, there is an approximate 2-to-1 split for the variants '[ij]ther-*bee*' and '[ɑj]ther-*pie*' respectively. For the innovative variant of (either), '[ij]ther-*bee*', we find that, in the regions of the Canadian Golden Horseshoe, the Ottawa Valley, and Montreal, it is first changing or non-stable and then becomes stable. However as shown in Figure 6, the change ends in stable variation, stability at middling frequencies of use, and not in near-categorical adoption of the innovation as was seen for the variable (sick) in Figure 5.

Stable variation is not a common phenomenon, but it is not rare, either. Can stable variation be incorporated into the model? The answer is yes, if we change two constants (t_0 and $\bar{N}$) into parameters; in fact, both stable variation and staggered start times of different innovations are achieved with these alterations. As shown in Figure 7 (overleaf), this solution (based on Griliches 1957 passim) has three parameters, *s*, *b*, and *n*, which correspond respectively to the 'origin' at the start of the process, the 'slope' or rate of change during the process, and the 'ceiling' at the end of the process. Most importantly, they represent three ways in which social factors are incorporated into the model: the social factors which begin the innovation, those which develop it, and those which are at the end of the change. Please note that the three parameters cannot be compared with each other: they are measured differently, in time units, number of adopters per unit time, and number of adopters.

There are important consequences of this solution: it predicts that stable variation is a result of S-curve language change and is not a temporary plateau in the midst of a change. When the initial social factors promoting the adoption do not alter during the change, or alter to ones that continue to promote the innovation, the adoption of the innovation increasingly develops until it reaches near-categorical levels. But when the social factors alter in such a way that they no longer promote the innovation during the change process, stable variation results. By considering stable variation as a form of stability, we needed to alter our initial language change model. Can we alter the model to incorporate other types of language changes?

$$\frac{dN(t)}{dt} = \boldsymbol{b}N(t) \times [\boldsymbol{n} - N(t)]$$

1. with boundary condition: at t = $\boldsymbol{s}$ (start of the diffusion process), N($\boldsymbol{s}$) = N$_S$.
2. where parameters $\boldsymbol{b}$, $\boldsymbol{n}$ and $\boldsymbol{s}$, indicated in bold, are to be determined.

N(t) is the cumulative number of adopters of the innovation at time t

$\frac{dN(t)}{dt}$ is the rate of diffusion of the innovation at time t

$\boldsymbol{b}$ is the rate of adoption

$\boldsymbol{b}$N(t) is the coefficient of diffusion for the internal-influence model (consider it as the probability of adoption of the innovation at time t)

$\boldsymbol{n}$ is the ceiling or long-term equilibrium number of potential adopters in the social system

[$\boldsymbol{n}$ − N(t)] is the number of potential adopters who have not yet adopted the innovation at time t (it is the difference between the ceiling number of potential adopters, $\boldsymbol{n}$, and the number of previous adopters, N(t), at time t)

Figure 7. *Differential version of internal-influence model with origin and ceiling parameters (based on Griliches 1957 passim).*
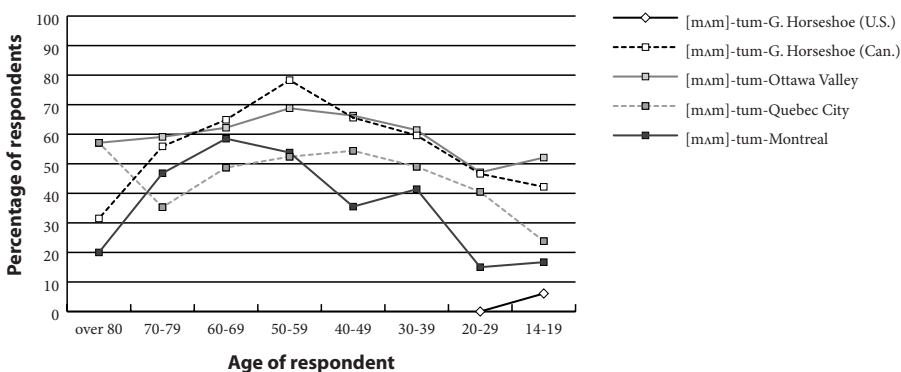
Figure 8. *Age chart for (mom) - [mʌm]-tum ([mʌm] which rhymes with the word tum).*

6.2. THE UNSUCCESSFUL INNOVATION: MOM. The variable (mom), investigated by a closed-ended question, is bivariate with variants '[mʌm]-tum' and '[mɑm]-Tom':

Does MOM, as in 'My Mom's gone fishing with my Dad', rhyme with
❏ tum or ❏ Tom?

As shown on Figure 8, the variable is changing in all regions. The change pattern is a reversal, where the innovation '[mʌm]-tum' first increases in frequency but is unsuccessful in achieving lasting prevalence at middling levels or higher.

I have not found any satisfactory mathematical model that logically suits the ascent and descent of an unsuccessful innovation. However, if we consider a rever-

$$\frac{dN_1(t)}{dt} = [a_1 + b_1N_1(t) - \beta_1N_2(t)] \times [n_1 - N_1(t)]$$

$$\frac{dN_2(t)}{dt} = [a_2 + b_2N_2(t) - \alpha_2N_1(t)] \times [n_2 - N_2(t)]$$

$i = 1, 2$ for variants/innovations 1, 2

1. with boundary conditions: at $t = s_i$ (start of the diffusion process of the innovation i),
    $N_i(s_i) = N_{is}$.
2. where parameters a_i, b_i, n_i, s_i, α_2, and β_1, indicated in bold or in Greek letters, are to be
    determined.

$N_i(t)$ is the cumulative number of adopters of the innovation i at time t

$\frac{dN_i(t)}{dt}$ is the rate of diffusion of the innovation i at time t

a_i is the coefficient of diffusion for the external influences of the innovation i

b_i is the rate of adoption of the innovation i

$b_iN_i(t)$ is the coefficient of diffusion for the internal influences of the innovation i (consider
    it as the probability of adoption of the innovation i at time t if innovation i could not be
    substituted by the other innovation)

$[a_i + b_iN_i(t)]$ is the coefficient of diffusion for the mixed-influence model if innovation i
    could not be substituted by the other innovation

$- \alpha_2N_1(t)$ consider as the probability of non-adoption of innovation 2 at time t if innovation
    2 can be substituted by innovation 1

$- \beta_1N_2(t)$ consider as the probability of non-adoption of innovation 1 at time t if innovation
    1 can be substituted by innovation 2

n_i is the ceiling or long-term equilibrium number of potential adopters in the social system
    for innovation i

$[n_i - N_i(t)]$ is the number of potential adopters who have not yet adopted innovation i at
    time t (it is the difference between the ceiling number of potential adopters, n_i, and the
    number of previous adopters, $N_i(t)$, at the innovation i at time t)

Figure 9. *Differential version of two-innovation substitute mixed-influence model
(adapted from Peterson & Mahajan 1978:213).*

sal as a case of two successive innovations where the first was interrupted, then we
can use a two-innovation 'substitute' model with start and ceiling parameters, as
exhibited by Figure 9 (adapted from Peterson & Mahajan 1978:213). Each innova-
tion is modeled by its own equation. In 'substitute' models, the effects of competing
innovations are subtracted: hence the negative term in each equation ($-\alpha_1N_2(t)$ and
$-\beta_2N_1(t)$) based on the other variant's frequency.

Why was the variant '[mʌm]-tum' unsuccessful? Extensive statistical analysis with
the eight potentially explanatory variables gave no pan-regional correlation. I hypoth-
esize that an influence external to the given society, Canadian English speakers, was
at work, namely American English. In general, external influences, complements

to internal influences, are change agents outside the given social system. They are so-called vertical channels of communication, often structured, hierarchical, or formal, such as government and its agencies or mass media. To include the potential effects of external factors, a mixed-influence S-curve model is created by adding the parameter *a* to the internal-influence model. Parameter *a* is not related to the number of past adopters of the language innovation. It is assumed that the effects of the external and internal influences are simply additive.

7. CONCLUSIONS. The internal-influence logistic distribution is a descriptive model of S-curve language change, and it comes with many assumptions and predictions, only some of which were discussed here. It describes the number of people who adopt a language innovation as solely a function of time, and incorporates the effects of potential social factors as a parameter to be determined. When we make small alterations, the mathematical model no longer captures just S-curve change: we can accommodate some language change patterns hitherto considered exceptions, for example, cases involving stable variation, reversal, and external influences. MacKeracher (2001) discusses more instances such as trivariant variables and differing sub-populations.

REFERENCES

BAILEY, CHARLES-JAMES N. 1973. *Variation and linguistic theory*. Arlington VA: Center for Applied Linguistics.

CHAMBERS, J. K. 1994. An introduction to Dialect Topography. *English world-wide* 15:35–53.

GRILICHES, ZVI. 1957. Hybrid corn: an exploration in the economics of technological change. *Econometrica* 25:501–22.

INOUE, FUMIO. 1997. S-shaped curves of language standardization. In *Issues and methods in dialectology*, ed. by Alan R. Thomas, 79–93. Dept. of Linguistics, University of Wales Bangor.

LABOV, WILLIAM. 1994. *Principles of linguistic change: internal factors, vol. 1*. Cambridge MA: Blackwell.

MACKERACHER, MARY S. 2001. Language stability in a world of language innovation. Unpublished generals paper. Dept. of Linguistics, University of Toronto.

Mahajan, Vijay & ROBERT A. PETERSON. 1985. *Models for innovation diffusion*. Newbury Park CA: Sage.

PETERSON, ROBERT A. & VIJAY MAHAJAN. 1978. Multi-product growth models. In *Research in marketing: an annual compilation of research*, ed. by Jagdish N. Sheth, 201–31. Greenwich CT: JAI Press.

ROGERS, EVERETT M. 1995. *Diffusion of innovations*. 4th ed. New York: Free Press (MacMillan).

❧

SPEECH ERRORS: ENGLISH VS. JAPANESE

Haruko Miyakoda
Tokyo University of Agriculture and Technology

THE DIFFICULTIES AND LIMITATIONS of employing speech errors as linguistic evidence have been discussed in the literature (e.g., Cutler 1982). Researchers have pointed out the problems associated with the use of speech errors as data. If similar distributional patterns are attested in different collections, then the corpus itself is criticized on the grounds that people (error collectors) tend to 'recognize' certain error types more often than others. On the other hand, if different patterns show up, then it is claimed that the data are not reliable because there is too much idiosyncrasy involved. Although error data are open to criticism, they are, however, a fruitful source of information about language.

In this study, the speech errors of Japanese will be compared with those of English. The similarities and differences observed for the two languages indicate that speech errors can reveal many interesting aspects of language and are indeed valid as linguistic evidence.

We first will review some of the problems related to the use of speech errors as linguistic data.

1. COLLECTING SPEECH ERROR DATA. The first serious attempt to study speech errors was made by Meringer and Mayer (1895). Since then, speech errors have been used in support of various linguistic and psycholinguistic arguments. They have in particular played a major role in the development of models of speech.

There are two ways of collecting speech errors: the 'pen–and–paper' method and the experimental method. We review both methods together with some of the problems related to each procedure. We draw heavily on Cutler 1982 and Poulisse 1999.

1.1. THE PEN-AND-PAPER METHOD. Initially, most collections were gathered by writing down the slips of the tongue actually attested in conversation. This is referred to as the 'pen–and–paper' method. This method has become the target of criticism concerning its reliability. Cutler (1982:6) discusses the three major problems associated with speech error research of this kind. First, it is not possible for collectors to record all slips occurring in a given period of time or a given number of utterances. It is extremely difficult for collectors to listen for errors and to pay attention to the content of the conversation at the same time. Second, some kinds of errors are harder to detect than others. This problem of perceptual bias may result in the latter being 'over-represented' in the corpus. Third, there is the danger of the collection being biased by the distributional characteristics of a language. It is of no theoretical interest if some

types of errors are reported more often than others if there was a distributional difference to begin with.

1.2. EXPERIMENTAL METHOD. In an attempt to overcome some of the limitations and problems of collecting spontaneous speech errors, experiments have been conducted in order to elicit errors in highly controlled contexts. Many techniques have been used to elicit errors in experiments. One of the most widely used techniques is the SLIP (Spoonerisms of Laboratory-Induced Predisposition) procedure developed by Motley et al. The advantage of experimentally-induced errors is that they are not open to the criticisms that spontaneous errors face. Since these experiments are designed to elicit errors in highly controlled contexts, researchers need not worry about the detectability problem or the distributional bias. However, there are certain disadvantages to the experimental method. First, the validity of the data becomes questionable when we consider the highly artificial tasks the informants are required to perform. The use of tasks that require more difficult articulation than spontaneous speech may lead to an alteration in normal planning processes. The second disadvantage of the experimental data concerns the type of errors that can be elicited. Since experiments can yield only certain types of errors, it is not possible to capture error patterns as a whole. We need assumptions and hypotheses in order to conduct the experiments. The experiments themselves cannot provide these. Here again, just as in the case of the pen-and-paper method, we find that there are problems.

1.3. SPEECH ERRORS: VALID OR INVALID? We see above that there are problems related to both the pen-and-paper method and the experimental method. This being the case, then, the question is the validity of the use of speech errors as linguistic evidence. Are the problems and limitations discussed an indication that speech lapses are not reliable as linguistic data?

It turns out that speech errors can tell us some interesting things about language. We just need to recognize the problems associated with speech error research and also to take into consideration the advantages and disadvantages of the two research methods. An advantage of the experimental procedure is that it is suitable for testing hypotheses in a direct manner. However, it is the errors collected from spontaneous speech that allow researchers to raise the hypotheses to be tested in experiments. Therefore, if the pen-and-paper method is conducted with the goal of raising hypotheses about language storage and processing, then there is no need to worry about detectability and distributional bias. This is the position taken in this paper. By focusing on the errors from this standpoint, we provide hypotheses worthy of testing in future studies.

In the following section, we compare the speech errors of English and Japanese speakers. The basis of this study is a collection of 298 Japanese errors recorded mostly by the author from spontaneous speech. The English data come from the literature (e.g., Cutler 1982, Fromkin 1973, Laubstein 1987). We attempt to find out whether the generalizations claimed for speech errors in English also apply to Japanese. Of particular interest is the interaction between the speech errors and the syllable

structure of a language. We limit ourselves to two hypotheses related to syllable structure that were put forth on the basis of slip research: the syllable structure hypothesis and the syllabic similarity hypothesis.

2. TWO HYPOTHESES. In this section, we review the basic assumptions of the two hypotheses related to syllable structure. We begin with the syllable structure hypothesis.

2.1. THE SYLLABLE STRUCTURE HYPOTHESIS. The basic assumption of the syllable structure hypothesis is that syllable initial consonant errors occur more frequently than errors in other positions because of the internal subgrouping of elements within the syllable: the initial consonant(s) independently constitute the onset of a syllable whereas the vowel is subgrouped into a rhyme together with the final consonant or consonant cluster:

(1)   σ

    onset   rhyme

           nucleus   coda

      C   V   C
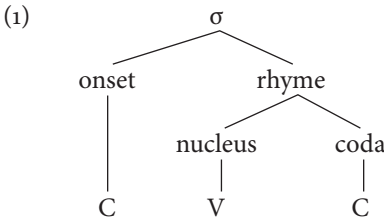
This claim regarding the 'initialness effect' was first made by MacKay 1970, who reported that 96% of 'within word' exchanges, and 81% of the 'between word' exchanges involved syllable initial elements. A similar percentage was also reported by Laubstein 1987, whose study of English speech errors showed that of 559 speech lapses, 63% involved the substitution of onsets by onsets, thus supporting the syllable structure hypothesis.

2.2. THE SYLLABLE SIMILARITY HYPOTHESIS. The syllabic similarity hypothesis or syllable position constraint is based on a claim made in Boomer and Laver 1968. It posits that phonemes in initial syllabic position replace those in initial position, nuclear replace nuclear, and final replace final. Simply put, this hypothesis accounts for the interaction of consonants with consonants only, vowels with vowels only.

The claim that phonological units involved in errors retain their original position in the syllable has been supported in many studies of English speech errors. For example, MacKay 1970 reported that reversed consonants occurred in the same syllabic position 98% of the time, and in the case of reversed vowels, 81% originated in the same syllabic position. Laubstein 1987 also reports that errors involving interaction between onset-onset, coda-coda, peak-peak make up 88% of the total. Details are given in Table 1 (overleaf).

Lately, however, some researchers have questioned the validity of this hypothesis (e.g., Meijer 1997). These researchers argue that the evidence presented so far does not

Syllabic position	Frequency (Number)	Frequency (Percentage)
onset-onset	352	63%
coda-coda	59	11%
peak-peak	81	14%

Table 1. *Syllabic position of segments interacting in errors (based on Laubstein 1987: 345).*

Group	Age	Number of errors collected
A	3–6 years old	84
B	10–16 years old	26
C	20–29 years old	95
D	30–55 years old	93

Table 2. *The four groups used in the study.*

necessarily support the claim that consonants (or vowels) are bound to a particular position in the syllable. Rather than 'syllabic' similarity, they claim that the reason why consonants and vowels do not interact with one another in speech errors is due to their 'phonetic' similarity. They point out that the substitution of a vowel by a consonant might result in an unpronounceable string and should therefore be rare.

3. DATA ANALYSIS: ENGLISH VS. JAPANESE. In this section, we analyze Japanese speech errors by comparing them with English speech errors. Our main focus is on the two hypotheses given in section 2. We studied the four groups shown in Table 2.

3.1. THE SYLLABLE STRUCTURE HYPOTHESIS AND JAPANESE ERRORS. As we see above, the basic assumption of the syllable structure hypothesis is that syllable initial errors are more frequent than syllable final errors because final consonants form a subgroup (rhyme) with the preceding vowels. Proponents of this hypothesis have taken for granted the fact that errors occur often in onsets due to syllable structure. This is a matter of course when we consider the 'Germanic' type of syllable structure as in (1), repeated on the next page as (2).

Here a break exists between the onset and the nucleus; the nucleus forms a constituent, the rhyme, with the following consonantal element. The frequency of syllable initial errors is attributed to this break between the onset and the rhyme, thus supporting the syllable structure hypothesis. However, in Japanese, syllable structure differs. In this language, there is cohesiveness between the onset and the nucleus, as shown in (3):

Error type		Number of errors
a.	syllable initial consonant errors	162 (54.4%)
b.	errors involving the nucleus	60 (20.1%)
c.	errors involving <onset + nucleus>	22 (7.4%)
d.	errors involving onset or <onset + nucleus>	23 (7.7%)
e.	errors involving moraic phonemes	31 (10.4%)

Table 3. *The occurrence rate of each error type.*
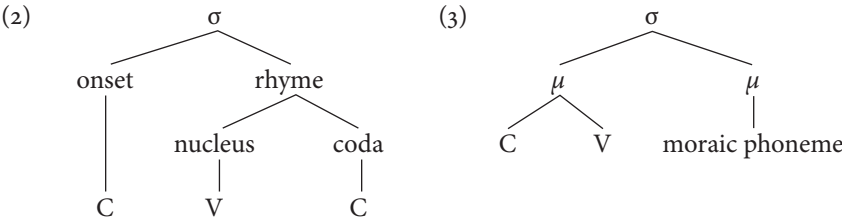
In Japanese, the onset and the nucleus are attached to a single mora node. If the 'initialness effect' is due to syllable structure, then it can be postulated that the occurrence rate of errors involving just the onset would be low in Japanese.

In Table 3, the occurrence rate of each error type is given. Here we find that nearly 55% of the errors in Japanese involve just the onset. If we consider the internal syllable structure in (3), we would expect more errors of the (c) type (errors involving <onset + nucleus>) to occur. However, these types constitute only 7.4% of the total. Even if type (d) errors (errors that can be interpreted either as onset only or as <onset + nucleus>) were added, the occurrence rate comes to only 15.1%. This high percentage of the syllable initial type of errors indicates that irrespective of syllable structure, the onset has a special status in inducing errors. This implies that the frequency of syllable initial errors has little to do with the internal structure of syllables, thus denying the basic assumption of the syllable structure hypothesis.

3.2. SYLLABIC SIMILARITY HYPOTHESIS. The syllabic similarity hypothesis, which claims that phonological units involved in errors retain their original position in the syllable, has been supported in many studies of English. Examples from English are given in (4):

(4)   **C → C**   left hemisphere   →   heft lemisphere
              pots and pans   →   pons and pats
      V → V   Wang's bibliography   →   Wing's babliography
              Bev and Bill   →   Biv and Bell
                                      (Laubstein 1987:343, 349, 350)

In Japanese, the general pattern attested is the same as for English: a consonant replaces a consonant, a vowel replaces a vowel. Furthermore, the CV syllable (or mora) is exchanged with another CV. Examples are given in (5):

(5) **C → C**   *karini*   → *tarini*   'if'
     V → V   *ondoku*   → *ondaku*   'reading out loud'
     CV → CV *ta ra ko su pa* → *ta ra su ko pa* 'cod roe spaghetti'

There are, however, exceptions to this correlation, and this involves the so-called moraic phonemes.

The mora plays a central role in the Japanese language. In practice, the mora often overlaps with the syllable, and indeed in many cases moras are syllables. The main reason why the mora and not the syllable is assumed to be the basic prosodic unit in Japanese is that several elements serve as independent units, although they do not qualify as independent syllables. These elements, the moraic phonemes, fall into four kinds: the nasal coda (N), the geminate consonant (Q), the second half of long vowels, and the second half of diphthongal vowel sequences. Examples of words with moraic phonemes are given in (6):

(6) examples of moraic phonemes
    a. nasal coda   ro N do N   'London'
    b. geminate consonant   pi Q tsu ba a gu   'Pittsburg'
    c. second half of long vowels   ro o ma   'Rome'
    d. second half of diphthongal   ha wa i   'Hawaii'
       vowel sequences

The nasal coda represents the sounds [n], [m], [ŋ], depending on the following phonetic context (e.g., paN to [panto] 'bread and', paN mo [pammo] 'bread also', paN ga [paŋga] 'the bread'). The geminate consonant represents a doubling of the consonant that appears in the onset of the following syllable (e.g., kiQ sa [kissa] 'tea room', kiQ te [kitte] 'postage stamp', kiQ pu [kippu] 'ticket'). These moraic phonemes, although not capable of forming an independent syllable, serve as an independent timing unit just as the regular CV one-mora syllables do.

A close observation of Japanese speech errors shows that in the case of moraic phonemes, consonants can be replaced by vowels, and vice versa. Examples are given in (7):

(7) **V → C** *ke i za i te ki* → *ke N za N te ki* 'economical'
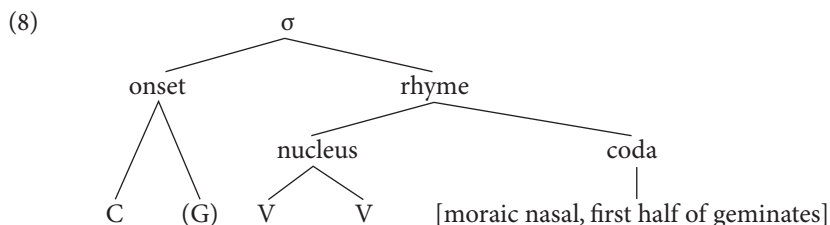   *sa N sai*   → *sa N sa N*   'three-year-old'
     C → V *ra N ki N gu*   → *ra i ki N gu*   'ranking'
          yu zu Q te   → *yu zu u te*   'to hand over'

The examples in (7) indicate that interaction between a consonant and a vowel can take place as long as they interact within their 'own kind', that is, the interaction is

either between a moraic nasal or obstruent with the second half of long vowels or diphthongs. The examples involving moraic phonemes support the syllabic similarity hypothesis. If, as some researchers claim, it is the 'phonetic' similarity that is crucial, we would not expect such an interaction to occur. What distinguishes the moraic phonemes from the other phonemes is their position within the syllable: moraic phonemes can only occupy coda position (cf. (3)).

4. DISCUSSION. In section 3 we analyzed the Japanese data relative to two hypotheses. Our findings suggest that 1) the 'initialness effect' is not due to syllable structure; in both English and Japanese, languages that have different syllable structure, there is a strong tendency for errors to occur in onsets; 2) although data from English does not strongly support the syllabic similarity hypothesis, the behavior of moraic phonemes in Japanese implies that phonological units involved in errors retain their original position in the syllable. These two findings based on Japanese speech errors show that speech errors retain their original syllable position, the exception being the onset.

At this point, it might be argued that the Japanese syllable structure given in (3) is wrong, and that Japanese, together with the 'Germanic' type of languages, has the syllable structure in (8) (Abe 1987:6):

(8)   σ

            onset   rhyme

                              nucleus   coda

        C   (G)   V   V   [moraic nasal, first half of geminates]

The structure in (8) is actually the one adopted by Abe 1987. The internal structure of the syllable proposed by Abe corresponds to the 'Germanic' syllable type. The advantage of this approach is that it can account for the independent involvement of the onset independently in errors in both English and Japanese: the break between the onset and the rhyme accounts for the 'initialness effect'. However, the problem with this structure is that it in no way reflects the linguistic realities observed in Japanese. First, if we look at the blending process and blending errors of Japanese (Kubozono & Ota 1998:32), we find that the break after the CV is crucial. Examples are given in (9):

(9)   a. **blending**   o + (si)Qpo   → oQpo 'tail'
          cf. English: sm(oke) + (f)og   → smog
      b. **blending errors** do. (o. si. te) + (na). N. de → do. N. de 'why'
          cf. English error: cl (ose) + (n) ear   → clear

Second, in the case of stuttering as well, the CV as a constituent is repeated in Japanese as shown in (10):

(10) **stuttering**
        sa – sa – sa – sakana   'fish'
        to – to – to – tombo   'dragonfly'
        cf. English: *s – s – s – six, t – t – t – ten*

The syllable structure depicted in (8) clearly does not reflect what is actually attested, whereas the structure in (3) does. Therefore, it seems reasonable to assume that in the case of Japanese, the onset and the nucleus form a subgroup, and that irrespective of syllable structure, the onset has a special status in inducing errors. This claim is worthy of exploring further in future studies, especially using the experimental method. Of particular interest would be to test to see whether the 'initialness effect' is concerned with syllable-initial position, as reported by Del Viso 1987 (in Poulisse 1999:12), or, with word-initial position, as claimed by other researchers (e.g., Davis 1989, Shattuck-Hufnagel 1992).

5. CONCLUSION In this paper, we have analyzed the speech errors of Japanese based on two hypotheses regarding syllable structure that were posited in earlier studies: the syllable structure hypothesis and the syllabic similarity hypothesis. Our findings suggest that phonological units involved in errors retain their original position in the syllable, thus supporting the syllabic similarity hypothesis. However, the basic assumption of the syllable structure hypothesis that the 'initialness effect' is due to syllable structure could not be supported. Rather, the Japanese error data suggest that irrespective of syllable structure, the onset has a special status in inducing errors.

REFERENCES

ABE, YASUAKI. 1987. Metrical structure and compounds in Japanese. In *Issues in Japanese linguistics*, ed. by Takashi Imai & Mamoru Saito, 1–20. Dordrecht: Foris.

BOOMER, DONALD & JOHN LAVER. 1968. Slips of the tongue. *British journal of disorders and communication* 3:2–12. Reprinted in Fromkin 1973: 120–31.

CUTLER, ANNE. 1982. *Slips of the tongue*. The Hague: Mouton

DAVIS, STUART. 1989. On a non-argument for the rhyme. *Journal of linguistics* 25: 211–17.

DEL VISO, SUSANA, JOSE M. IGOA & JOSE E. GARCIA-ALBEA. 1987. Corpus of spontaneous slips of the tongue in Spanish. Unpublished manuscript.

FROMKIN, VICTORIA A. (ed.) 1973. *Speech errors as linguistic evidence*. The Hague: Mouton

———. (ed.) 1980. *Errors in linguistic performance: Slips of the tongue, ear, pen and hand*. New York: Academic Press.

KUBOZONO, HARUO & SATOSHI OTA. 1998. *Oninkouzou to akusento [Phonological structure and accent]*. Tokyo: Kenkyusha shuppan.

LAUBSTEIN, ANN STUART. 1987. Syllable structure: the speech error evidence. *Canadian journal of linguistics* 32:339–63.

MACKAY, DONALD. 1970. Spoonerisms: the structure of errors in the serial order of speech. *Neuropsychologia* 8.323-50. Reprinted in Fromkin 1973: 164–94.

MEIJER, PAUL. 1997. What speech errors can tell us about word-form generation: The roles of constraint and opportunity. *Journal of psycholinguistic research* 26:141–58.

MERINGER, RUDOLF & KARL MAYER. 1895. *Versprechen und Verlesen, eine psychologisch-linguistische Studie*. Stuttgart: G schen'sche Verlagshandlung.

MOTLEY, MICHAEL T. 1980. Verification of 'Freudian Slips' and semantic prearticulatory editing via laboratory-induced spoonerisms. In Fromkin 1980: 133–47.

POULISSE, NANDA. 1999. *Slips of the tongue: Speech errors in first and second language production*. Amsterdam: John Benjamins.

SHATTUCK-HUFNAGEL, STEPHANIE. 1992. The role of word structure in segmental serial ordering. *Cognition* 42:213–59.

SPOKEN AND WRITTEN NARRATIVES

Ming-Ming Pu
University of Maine at Farmington

Qing-Hong Pu
University College,
Beijing Polytechnic University

ALTHOUGH THERE HAS BEEN a considerable body of research in various fields dealing with similarities and differences between speech and writing, there is little agreement on the salient characteristics of the two modes. The written language or discourse is in general considered structurally more elaborated, complex and formal, whereas spoken language or discourse is characterized as being structurally simple and informal (Chafe 1985, Gumperz et al. 1984). On the other hand, while spoken discourse has a higher degree of interaction between the speaker and the audience, written discourse seems more apt to show the writer's detachment from the audience. Nevertheless, some studies have found little difference between spoken and written discourse, and still others argue that speech is more elaborated and complex than writing (Halliday 1987).

Are oral and written languages different? The general consensus is that they are, because they represent different ways of communicating and offer different ways of knowing and of reflecting on experience. They serve distinct functions and purposes in a discourse community, in time and space. They utilize different contexts. While oral language is typically associated with conversation that is produced and processed in the context of face-to-face exchange, written language is typically associated with the language of books and explanatory prose. Oral language is characterized as informal, interpersonal, and narrative-like with prosodic cues, deixis and paralinguistic devices readily available, while written language is considered formal, planned and expository-like with limited reciprocity between the writer and the reader (Horowitz & Samuels 1987). Nevertheless, there is much variation and overlap in this simple dichotomy between oral and written texts, depending upon the purposes for which they are used and the audience they serve.

Furthermore, cognitive psychology has offered us much evidence that spoken and written language involve different mental processes. There is an abundance of research in the field that addresses the processing of speech and writing and the short-term consequences of exposure to oral versus written discourse. Jahandarie (1999) presents a thorough and detailed review of various issues and empirical evidence concerning the comprehension of oral and written words and sentences, the retention of these elements, and both comprehension and retention of connected discourse. This large body of evidence demonstrates that major differences exist between spoken and written language, as shown in the distinct ways our mind deals with each language. It is obvious that the majority of evidence from separate lines of research indicates the existence of the differences between spoken and written discourse. Nevertheless, the comparison

between the two modalities is usually either done between the typical speech and writing (e.g., conversation vs. explanatory or expository prose), or between two different genres (e.g., narrative vs. academic writing). While distinct patterns have emerged from these comparisons between oral and written discourse, the characterizations may be partly due to genre differences, because patterns of discourse such as rhetoric structures, attribution, adversative, covariance, response, etc., do not work in the same way across readers of various age groups and grades and across text topics (Horowitz & Samuels 1987). It would be more interesting to examine oral and written language of the same genre and the same style to discover differences and similarities between the two modalities where other variables of discourse are kept constant. The present study attempts to explore contrasts between oral and written language from a much narrower perspective: a comparison between the two modalities of the same type of discourse—narrative accounts of the same video clip that are produced under the same controlled conditions by subjects of a homogeneous background. This limited and controlled comparison would help us better understand differences as well as similarities between oral and written language in a stricter sense, because other apparent differences such as audience, purpose, content, discourse context, discourse structure, etc., are kept minimal in oral and written narratives.

1. THE NARRATIVE STUDY. A narrative study was designed and conducted to elicit both oral and written stories from native English and Mandarin Chinese speakers. The purpose of the study is to investigate how speakers construct and tell a story in either oral or written form after watching the same stimulus material, a short video-clip, and what systematic similarities and differences occur in both narratives. The study included participants of two languages so as to explore a cross-linguistic contrast between the two types of narratives.

1.1. STIMULUS MATERIAL. The stimulus material consists of a 4-minute video-clip entitled 'The New Doorbell'. The clip is a cartoon about a man who installs a new doorbell in his apartment and then waits anxiously for people to ring it. The cartoon is a silent color movie with background music; no written language ever appears on the screen except the title, which is shown at the beginning of the video-clip in both Chinese and English. As in any other typical story, 'The New Doorbell' consists of a beginning which introduces the main character and the setting and the theme of the story, a middle which unfolds the development of the storyline, highlighted by a climax, and an end which brings the outcome of the story. The overall structure of the story is schematized in Figure 1.

The story is both linearly and hierarchically structured. We wanted to see how speakers would organize the story information, what information they would consider important, less important or marginal, how they would encode the hierarchical structure of the story into a linear text, and what linguistic devices they use to achieve the story coherence. Furthermore, we were particularly interested in subjects' performance between oral and written task across the two languages.
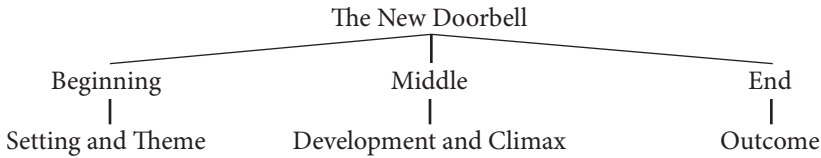
The New Doorbell

| Beginning | Middle | End |

| Setting and Theme | Development and Climax | Outcome |

Figure 1. 'The New Doorbell' structure.

1.2. METHODS AND PROCEDURES. The narratives were produced either individually or in groups. Before the story-telling began, a subject or a group of subjects was given a passage of written instruction on what they were going to do. The subjects were asked to first watch a video-clip of four minutes length, and then describe orally or in written form what happened in the story. The written instruction did not describe the nature of the video-clip or the purpose of the study, nor did it mention the word 'story' or 'story-telling'. We hoped that the subjects were able to perceive, construct and describe the video-clip with as few preconceived notions as possible. The oral narratives were tape-recorded and later transcribed; the written narratives were collected immediately after the subjects finished writing the story.

1.3. SUBJECTS. Seventy subjects participated in the narrative study. Forty were native American English speakers from the University of Maine at Farmington, and thirty were native Mandarin Chinese speakers from the Central China University of Finance and Economics. The subjects were randomly assigned to two groups in each language: 20 in English Oral (EO), 20 in English Written (EW), 15 in Chinese Oral (CO) and 15 in Chinese written (CW). All subjects were undergraduates and about two-thirds were women.

2. RESULTS AND DISCUSSION. In general, the speakers of both tasks across the languages produced the narratives in comparable ways in terms of episode selection and description, coherence-building, event sequencing, reference tracking, and inference making. They did not merely represent the visual data, such as the movements of the objects or persons, but rather, an interpretation of the events (Loftus 1979). In other words, they construct a meaning. While describing what happened in the video-clip, they were more concerned about the cause and outcome of the actions and the purpose and explanation of the characters who performed the actions. They stayed on the main event line, focused on the actions of the central character, and paid less attention to information that was less important in the development of the storyline. Although some subjects only touched upon or even omitted some scenes which were not critically related to the theme of the story, all narratives exhibited the global structure of story construction: a beginning consisting of background information such as the setting and the main character, a middle showing the main storyline and the development of the story, a climax dealing with the conflict between the man (the main character) and a postman, and an ending describing the disappointment of the man. This global organization of the narrative discourse demonstrates that

subjects, or rather, people in general, have expectations about how to tell a story. They take in visual stimuli, construct a mental representation of what they perceive, and encode it into a linguistically structured message. Even though the written instruction does not ask subjects to 'tell a story', they nonetheless uniformly organized the visual data into a hierarchical structure with interrelated events, describing who, when, what, where, what happened, and why in their narrative, whether oral or written.

While the overall structure of the narrative across the four groups is quite similar, striking differences are found between the two modalities. The remaining sections of the paper focus on the patterns of differences that emerged at the word/phrase and clause/sentence level between the two modalities.

2.1. AT WORD/PHRASE LEVEL. The first major difference at this level is the use of verb tense between English oral and written narratives. Even though past tense is considered as norm in narratives (Biber 1988), the majority of subjects (15 out of 20) in the writing group employed present tense, as compared to only 8 subjects using the tense in the oral group. It seems that the tense preference is different between speakers and writers. The use of present and past tense is found to be associated with the modality ($\chi^2 = 6.46^*$, $p \leq 0.01$). The tense difference might be due to the nature of oral versus written story-telling. Thirteen out of twenty subjects in EO started their narrative with a comment that they just *watched* a video/movie clip, and this very first sentence sets up a time frame for their subsequent story-telling, i.e., what *happened* in the video. In EW, however, very few of the subjects mentioned the fact that they were about to tell a story of a video clip they just watched; sixteen out of twenty subjects started the narrative directly with the central character of the story, e.g., *A man…* or *There is a man…*, and then continued almost always with the present tense description throughout the written narrative. The typical beginning of the oral and written English narratives is presented in (1) and (2) below.

(1) *EO: S10* We watched a video… called the new doorbell. It was about a man…

(2) *EW: S5* There is a man who lives in…

It seems as if speakers are recounting past events but writers are describing what is happening at the moment. Although Chinese does not have overt markers for verb tense, subjects did begin their narratives in the two modalities differentially to a certain degree. Seven out of fifteen subjects in CO started their narrative with the mention of the cartoon/video, suggesting that they were telling the story of a cartoon or video they watched previously. Only one subject did so in CW; the other fourteen opened their narratives, as did their English counterparts in EW, with some kind of introduction of the central character, then continued to report the events in their temporal sequence. For example:

(3) *co: s9* umm, zhè-ge dòng-huà-piān de míng-zi jiào 'xīn zhuāng de mén-líng'…

                 cartoon   of name   call 'new-install   doorbell'

        'Umm, the title of the cartoon is "The New Doorbell"…'

(4) *cw: s2* yī-ge nán-rén zhù-zài yī-zhuàng gōng-yù   lǐ,…

           a   man   live   a   apartment in

        'A man lives in an apartment,…'

Another difference found at the word/phrase level between the oral and written narratives is the occurrence of hedges and hedge-like expressions such as *kind of/kinda, sort of, seem like, anyway*, etc, which are used almost exclusively by the oral group in both languages. For example,

(5) *EO: s3* So he shut the door and the delivery man *kinda* looked at the doorbell, and then walked away.

(6) *EO: s7* Then… a short little man came and knocked on the door,… and this made him *kinda* angry.

(7) *co: s5* tā *hǎoxiàng* shì yǒu-diǎnr   náo-huǒ, shì-ba?

        he seem-like is have-a little frustrated isn't he

        'He seems a little frustrated, doesn't he?'

(8) *co: s12* tā *kě-néng yóu-diǎnr* shī-wàng   ba,…

        he maybe somewhat disappointed

        'He is kind of disappointed.'

Such expressions suggest that speakers were ambiguous about the use of adjectives like *angry, frustrated, disappointed, upset*, etc. Chafe and Danielewicz (1987) attribute the hedge use to the limited lexical choice on the part of the speaker, who is not completely satisfied with his/her lexical choice, yet has no time to ponder on a better word because speaking is done on the fly. They argue that speaking calls for greater expenditure of cognitive effort and hence, speakers tend to operate with a narrower range of lexical choices than writers. As a result, the vocabulary of spoken language is more limited in variety, regardless of the kind of speaking involved. While the argument is intuitively appealing, it is not evidenced in our study. Although writers of both languages never used hedges such as those shown in the above samples, their choice of vocabulary is very much the same as those of the speakers, even if they 'have the leisure to dip into the rich storehouse of literary vocabulary, search for items that will capture nuance' (Chafe & Danielewicz 1987:88). For example, when describing the man's disappointment, subjects across the four groups used the same set of adjectives, such as *upset*, *angry, frustrated, disappointed, disheartened, sad*, and *mad*, sometimes accompanied by intensifiers or quantifiers such as *very, extremely, a little, more,* and

so. It seems, at least for the genre of story-telling, the range and level of vocabulary are quite similar between the two modalities, which is in general simple and of high frequency.

Why, then, do speakers seem less certain of their lexical choice than writers, given the fact that they use practically the same set of words? On the one hand, we agree that the fundamental difference lies in the inherent cognitive constraints of speaking and writing, as pointed out by Chafe and Danielewicz: speakers have little time while writers are not pressed to do on-line production. We argue, on the other hand, that what our speakers hesitated about but had no time for is not so much the availability of vocabulary as the verification of what they actually saw prior to the story-telling. For example, the speaker in (5) was making a simple statement that the postman looks at the doorbell button before he leaves. The subject had no trouble with the choice or preciseness of the verb 'look (at)', but she wasn't sure, at that moment of the on-going production process, whether or not the postman actually looked at the doorbell. She couldn't afford to think more about it, but used a hedge instead to show her uncertainty. Similarly, (6), (7) and (8) above reveal that the speakers knew that the man was angry, frustrated, or disappointed but they were not sure of the degree of his anger, frustration, or disappointment. In order to maintain the quick and smooth flow of production, they had no choice but to use a hedge to mark their state of mind on that particular juncture. Of course, speakers can pause, false start, or even comment on their uncertainty to revise what has been said, but too much fumbling is harmful to effective communication on the one hand, and acknowledging *I'm not sure/I don't know* is damaging to the speaker's credibility on the other. In written narratives, on the other hand, the use of hedges and false starts almost never occurred, because in writing, with or without editing, one always has more time for language processing. Writers usually plan a clause/sentence ahead before they actually write it down. It seems that they are aware, consciously or not, of the permanency and the formality of the writing and try to avoid hesitation and uncertainty in their narratives. What is written down exists (more permanently), spread out on the page. Informal words such as *kind of/kinda* don't usually belong to the written form unless one purposely tries to mimic the oral language.

2.2. AT CLAUSE/SENTENCE LEVEL. As discussed in the prior section, speakers did not appear to have a more limited set of vocabulary than writers in their production of narratives: both employed relatively simple and high-frequency nouns, verbs, adjectives and adverbs. At the clause or sentence level, we also find that both types of narratives are comprised primarily of main clauses, usually simple and short. The similarity in clause length and syntax reflects the nature of narratives, be it oral or written, which are modeled on the *story-telling genre*, because narratives depend for their effect on interpersonal involvement between the speaker/writer or the character and the reader (Tannen 1984). Of all the complex sentences in our data, adverbial clauses occur most frequently. Although adverbial clauses have a very similar frequency of

	Temporal				Cause		Purpose		Total
	MC+SC		SC+MC		MC+SC	SC+MC	MC+SC	SC+MC	
	E1+E2	E2+E1	E1+E2	E2+E1					
EO	13	3	10	1	21	0	4	0	52
EW	17	11	11	1	11	0	5	0	56
Total	30	14	21	2	32	0	9	0	108

Table 1. English adverbial clause.

distribution among the four groups, they exhibit distinct patterns between the two modalities.

In general, subjects in both English groups prefer the unmarked structure of main clause preceding subordinate clause (Pu & Prideaux 1994), i.e., MC+SC. Nevertheless, for complex sentences with temporal adverbial clauses, subjects prefer to describe events as they occur in the natural order, namely, E1+E2, even if that results in a marked construction of SC+MC. For example, (9) and (10) below are coded in the order of SC+MC while following the temporal sequence of E1+E2.

(9) *EO: s7 When* he pressed it, it played music.

(10) *EW: s4 After* he listens to the music of his doorbell, he sets a chair near
        the door…

Table 1 summarizes the complex sentences containing adverbial clauses found in the two English groups. The result indicates that the order of temporal sequence appears to override the MC+SC construction frequently, especially for the speakers, who seem to be more constrained by the temporal sequence of events in processing the story information than writers.

It makes perfect processing sense that speakers, rather than writers, should rely more heavily on the temporal sequence of events during the storytelling, because they are more constrained in cognitive resources during narrative production. As several researchers have observed, it is easier to encode, store in memory and retrieve a chain of events that are narrated in a coherent sequence, and temporal coherence facilitates mapping process within units or episodes (Gernsbacher 1990, Givón 1993). Although writers are sensitive to the reader's needs by trying to follow event sequence and code them in the unmarked structure (MC+SC), they can nonetheless afford to manipulate the sentence structure to a certain degree to serve special functions. Of interest here is the fact that in EW, all 12 complex sentences with the marked order of SC+MC are found at the beginning of an episode, signaling the episode boundary. Examples (11) and (12) below illustrate such sentences marking the advent of a new episode.

(11) *EW: s8 After* the man waited some more, he heard another person come up
        the stairs.

(12) EW: S10 *Every time* he hears someone walking up the stairs, he gets excited.

The use of marked structure to indicate episode boundaries is explored in prior studies (Carpenter & Just 1975, Gernsbacher 1990, Pu & Prideaux 1994), which show that speakers and writers use certain devices to signal for their listeners and readers the begining of a new passage or episode, where there is a change in topic, point of view, location, or temporal setting.

While there are different aspects to the use of adverbial clauses by English speakers and writers, adverbial clauses found in Chinese spoken and written narratives differ in other ways, even though both have the same frequency of occurrence. Unlike their English counterparts, most Chinese complex sentences containing adverbial clauses have a fixed order of SC+MC. For example,

(13) CW: S8 *suī-rán* tā děng-le hěn cháng shí-jiān, dàn tā bìng-bū qì-něi
           though   he wait   very long time   but he   not   discouraged
           'Though he has waited for a long time, he is not discouraged.'

(14) CO: S3 (yī-ge zhōng-nián rén huí-dào jiā-zhōng.)
            a   middle-aged man come   home
             Huí-dào jiā   *yǐ-hòu*, tā bǎ wài-tào tuō-le,
             come   home after   he OM jacket take-off
           '(A middle-aged man comes home.) After (he) comes home, he takes
            off his jacket ...'

(13) contains a clause of concession that precedes the main clause, and (14) is a complex sentence of SC+MC with the subordinator at the end of the temporal clause. In CO narratives, the majority of the adverbial clauses indicate time such as the one with *yǐ-hòu* ('after') in (14), whereas CW narratives contain different types of adverbial clauses. Table 2 summarizes the results, which shows that adverbial clauses in CO are almost exclusively the *after*-type in the SC+MC construction, clearly reflecting iconicity of event sequence. It is not surprising to find nearly 90% of adverbial clauses are of the same type in the spoken narrative, because it is easier for speakers with limited cognitive resources to construct sentences that mirror the temporal sequence of events. The construction of other complex sentences must call for the expenditure of some cognitive effort, which speakers constantly lack but writers have. Consequently, the written narrative witnesses adverbial clauses of a greater variety.

Another important difference between oral and written narratives at the clause/sentence level lies in how speakers and writers package and encode events in sentences. While constructing narrative orally, subjects tended to put sequences of events in consecutive yet separate clauses or sentences, usually one proposition per clause. In written narratives, on the other hand, clauses and sentences are more compact, consisting of multi-propositions per clause. The contrast is illustrated in the following examples:

Group	Temporal			Cause	Purpose	Concession	Total
	after	*before*	*other*				
CO	52	3	4	0	0	1	60
CW	22	2	20	8	6	4	62
Total	74	5	24	8	6	5	122

Table 2. Chinese adverbial clause.

(15) *EO: s6* Next a little girl comes up, and she's bouncing a ball,… And she bounces the ball outside his door, and then leaves the scene. She doesn't ring the doorbell.

(16) *EW: s14* A little girl bouncing a ball approached the door and walked away without ringing the doorbell.

(15) and (16) describe the same sequence of events about the little girl who passes by the man's door without ringing his doorbell. The subject who told the story orally coded the episode almost scene by scene in 5 separate clauses; each clause consists of only one proposition, whereas the subject who wrote the story encoded the episode in only two clauses, each of which is comprised of more than one proposition. Although writers in general did not employ more subordinations than speakers did, their descriptions are nonetheless more compact and more complex in terms of the number of propositions per clause and the strategic deployment of present and past participials. Speakers in general prefer single-propositional sentences, because it is presumably easier to store and retrieve them than multi-propositional sentences. Kintsch and Keenan (1973) discovered that reading time increases as a function of the number of propositions within a text, and Kintsch and Glass (1974) found that recall is better for single proposition sentences than for multiple proposition sentences in texts, even when the number of words is constant.

Moreover, subjects in producing oral narratives are often found to re-encode part of the event that has already been depicted in the preceding sentence, revealing speakers' online processing. Samples of the following sort are quite common in Chinese oral narratives:

(17) *EO: s2* yī-ge nán-de tā xià-bān huí-laí, xīn <u>zhuāng-le</u> <u>yí-ge</u> <u>mén-líng</u>,
    a   male   he off-work return new install   a   door-bell
    zhuāng-shàng-le yǐ-hòu,
    install-up   after
    'A man returns from work, and installs a new doorbell. After (he) installs (it),…'

(18) *EO: s5* rán-hòu tīng-jiàn <u>jiǎo-bù</u>   <u>shēng</u> <u>lái-le</u>, *jiǎo-bù   shēng lái-le* yǐ-hòu,
      then   hear   foot-step sound come foot-step sound come after
      'Then (he) hears footsteps coming up. After the footsteps come up,…'

(19) *EO: s9* zhī-hòu   jiù <u>lái-le</u> gè <u>yóu-dì-yuán</u>, *yóu-dì-yuán lái-le ne*,
      afterwards just come a   post-man   post-man   come
      Afterwards comes a postman. As the postman comes,…'

This kind of overlap between events, or rather, repetition of a statement, or partial statement, serves as a stalling mechanism in oral language, which enables speakers to continue *their* production with relatively less effort, to find all or part of the utterance ready-made, so they can proceed with verbalization before deciding exactly what to say next (Tannen 1993). At the same time, repetition provides the listener with redundant, semantically less dense discourse with sufficient pauses for auditory processing. Our written narratives, on the other hand, witness very few of such overlaps or repetitions because writers have sufficient time to plan their clauses or sentences before actually producing them.

3. CONCLUSION. Prior studies on spoken and written language in general examine oral and written discourse of different varieties such as letters, academic writings, newspaper articles, narratives, conversations, and lectures, which are distinct in the first place in their structural organization, level of formality, message content, genre of text, time and space of production, general audience, and context. With so many different facets of discourse involved in the investigation on speaking and writing, the results are often convoluted and contradictory, because there are too many uncontrolled factors that might have affected or led to the results. The present study tries to avoid these dispersive factors in our investigation of similarities and differences between speaking and writing by examining the two modalities in a narrower scope yet a more controlled manner. We asked subjects of homogeneous background from two typologically different languages to produce a piece of narrative after they watched a video clip. Besides the different medium in which subjects produced the narrative, all other aspects of discourse were kept constant: same stimulus material, same environment, same goal orientation, same context, same genre, same level of formality, same awareness of task (i.e., being recorded either on tape or on paper), same preparation time (i.e., immediately after watching the video-clip), and finally, no audience present and no time limit placed on production for either mode. With all those variables under control, we hoped to be able to compare spoken and written language in a stricter sense with cross-linguistic validation.

The present study argues that both oral and written narratives produced by our subjects exhibit patterns of similarities to a certain extent because of the common characteristics of the narrative genre, but considerable and significant differences manifest themselves between the two modalities because of the distinct mental processes involved in oral and written language production. Our production data lend

support to our argument. On the one hand, the two types of narratives produced under the same condition present important similar patterns. Regardless of modalities, subjects perform uniformly at the discourse level in global structure organization, episode building, and reference tracking. However, striking differences are also found between the two modalities at various levels of discourse. The differences arise basically from the distinct mental processes and mechanisms between speaking and writing in general, and varying degrees of inherent dependency of both on interpersonal involvement in story-telling.

REFERENCES

BIBER, DOUGLAS. 1988. *Variations across speech and writing*. Cambridge: Cambridge University Press.

CARPENTER, PATRICIA A. & MARCEL A. JUST. 1975. Sentence comprehension: A psycholinguistic processing model of verifications. *Psychological review* 82:45–73.

CHAFE, WALLACE. 1985. Linguistic differences produced by differences between speaking and writing. In *Literature, language, and learning: the nature and consequences of reading and writing*, ed. by David R. Olson, Nancy Torrance & Angela Hildyard, 105–23. Cambridge: Cambridge University Press.

—— & JANE DANIELEWICZ. 1987. Properties of spoken and written language. In *Comprehending oral and written language*, ed. by Rosalind Horowitz & S. Jay Samuels, 83–113. San Diego: Academic Press.

GIVÓN, TALMY. 1993. *English grammar: a function-based introduction, vol 2.* Amsterdam: John Benjamins.

GERNSBACHER, MORTON A. 1990. *Language comprehension as structure building*. Hillsdale: Lawrence Erlbaum.

GUMPERZ, JOHN J., HANNAH KALTMAN & MARY CATHERINR O'CONNOR. 1984. Cohesion in spoken and written discourse. In *Coherence in spoken and written discourse*, ed. by. Deborah Tannen, 3–20. Norwood: Ablex.

HALLIDAY, M.A.K. 1987. Spoken and written models of meaning. In *Comprehending oral and written language*, ed. by Rosalind Horowitz & S. Jay Samuels, 55–82. San Diego: Academic Press

HOROWITZ, ROSALIND & S. JAY SAMUELS. 1987. Comprehending oral and written language: Critical contrasts for literacy and schooling. In *Comprehending oral and written language*, ed. by Rosalind Horowitz & S. Jay Samuels, 1–52. San Diego: Academic Press .

JAHANDARIE, KHOSROW. 1999. *Spoken and written discourse: a multi-disciplinary perspective*. Stamford: Ablex.

KINTSCH, WALTER & JANICE M. KEENAN. 1973. Reading rate and retention as a function of the number of propositions in the base structure of sentences. *Cognitive psychology*5: 257–74.

KINTSCH, WALTER & G. GLASS. 1974. Effects of propositional structure upon sentence recall. In *The representation of meaning in memory*, ed. by Walter Kintsch, 140–50. Hillsdale: Lawrence Erlbaum.

LOFTUS, ELIZABETH F. 1979. *Eyewitness testimony*. Cambridge: Harvard University Press.

PU, MING-MING & GARY D. PRIDEAUX. 1994. Coding episode boundaries with marked structures: a cross-linguistic study. *The Canadian journal of linguistics*, 39:283–96.

TANNEN, DEBORAH. (ed.) 1984. *Coherence in spoken and written discourse*. Norwood: Ablex.

———. 1993. What's in a frame?: surface evidence for underlying expectations. In *Framing in discourse*, ed. by Deborah Tannen, 14–56. Oxford: Oxford University Press.

PROSODIC THEORY AND EVIDENCE IN ORAL DISCOURSE

Steven Schaefer

Ecole Supérieure de Chimie Organique et Minérale, France

How do we make sense of oral linguistic phenomena? When we look for evidence of phonological consistency in contemporary speech, what criteria do we follow? The presence of extremely flexible prosodic forms in General American English (GAE) is problematical for theorizing a phonological structure having a stable relationship to speech. Providing evidence of the way prosody is used must invariably come from concrete acoustic examples taken from real-life situations, and not merely by simulating contexts in laboratory conditions. Only then can we build up theories from observation, much as the description of unwritten languages has traditionally proceeded. Speakers of GAE typically use several acoustical attributes to emphasize one or more words in an utterance. These emphasized words are more prominent and have a communicative function, like for pertinent information that has to be made apparent to the co-utterer. Speakers emphasize words mainly by changing the fundamental frequency (Lehiste 1970, Ladd 1996), but there are other acoustical correlates to consider, like duration and intensity.

Today most approaches deal with pitch or fundamental frequency exclusively. The fundamental frequency can be measured and the intonation curve is often stylized and labeled according to the theory one adheres to. In autosegmental theory, this is a linearized model of static tones which are said to define the melodic contour. Attempts to create an 'inventory of tunes' (Nicaise and Gray 1998:80) which can be assigned to simple phrases like *Is John coming ?* as well as *I think John's coming* (both could be coded as the tone sequence: Medium-High-LowMedium%, this last symbol indicating a tone boundary established by a rise in pitch) illustrate a disregard for the complexity of the utterer's constructing meaning not just in one pre-assigned manner, but by his manipulating a number of acoustic parameters in the sound continuum to achieve a contextualized, 'hearer-sensitive' message. According to the authors, what can be (rather arbitrarily) labeled Pre-head, Head, and Nucleus in the British tradition are assigned target values in the tone string, regardless of individual variation or nuance. We find that such a static phonological model does not capture the discursive, dynamic aspect of speech.

The approach proposed here attempts to integrate other ways of looking at sound, especially in relation to its connectedness to the speech situations in which it constructs meaning. We advocate using such varied methods as discourse analysis, along with an acoustic analysis of the stream of speech, which are integrated as an integrated prosodic analysis into the utterer-centered model in the Theory of Enunciative Operations formulated by Antoine Culioli in France. The main specificity of this model is the attention

paid to the central role of the utterer (or enunciator) in the construction of meaning; theorizing the model proceeds from observation of authentic language data collected in specific situations which can be shown to influence or constrain the interpretation of any given utterance. As viewed in this theory, what is generally called 'sentence accent' functions as a marker for linguistic operations carried out by the utterer[1]: he uses it to draw the attention of his co-utterer to an element of the utterance (here we depart from the syntactic term 'sentence') and the fact that (for him) this element stands in some unique relationship either to himself and/or to some other element included in the discursive text or the situation of the speech act. Further analysis provides the linguist with a more complete understanding of the limitations and necessary character of prosody in speech production/perception, which go beyond mere phonological descriptions of what sound in speech should correspond to.

Previous investigations show that there is nonetheless a demonstrable relationship between pitch movement and perceived prominent accent in the utterance. The problem here is to know whether all speakers use pitch movements to emphasize a word in a sentence, or if other attributes can be used to emphasize a word. A second problem is to find the linguistic tools, or schemata, to interpret the relationship between prosodic form and meaning.

We used a perception experiment to detect the prominent words in a number of spontaneous utterances in GAE. When the majority of the 4 listeners indicate that a given word is emphasized by that speaker, we consider that word as prominent. We call this a prominent point in the prosodic schema of the utterance. (We should point out that the schema is not always coextensive with the utterance. Complex or longer utterances can contain a number of schema, and the same schema can theoretically extend over more than one short utterance, where an interjection, for example, may constitute an utterance.)

This study investigates 1) whether all prominent accents are produced by a pitch movement, e.g. a rise, fall, or peak (both rise and fall); 2) whether the pitch movements in the prominent words can be distinguished from the pitch movements in the non-prominent words, but cannot be represented as static points in a linearized model; and 3) whether the linguistic meaning of the utterance is reflected in the prosodic marking of pertinent elements.

1. LISTENING EXPERIMENT—METHOD. A listening experiment was carried out to investigate which words in the sentences were perceived as prominent. Once this information was available, the pitch movements in these prominent words were investigated.

The present study isolated a corpus of spontaneous utterances containing the initial sequence 'I think…' taken from a 45 min. recording of an unscripted televised interview, 'The Whitewater Debate'. The relationship between perceived prominence accent and the measurement of acoustical pitch movements in spontaneous speech is described by means of a listening experiment in which we identified which words in 32 utterances pronounced by 4 speakers were perceived as prominent by the majority of 4 (other) listeners—a small group of native-speaker informants who

Prominence	RISE	FALL	RISE/FALL	LEVEL	TOTAL
MAX/SCHEMA	4	3	2	2	
TOTAL/32 SCH	45	41	13	7	107
AVERAGE/SCH	1.4	1.3	.41	.21	3.34

Table 1. *Pitch movement in utterance.*

independently distinguished a double-binary ordering scheme of prominent/non-prominent and prominent/super-prominent syllables. The pitch contours of these words were analyzed in detail using the spectrographic software program *Signalyze*, measuring not only fundamental frequency, but also amplitude and duration on a coordinate time axis. Special attention was paid to the type of pitch movement (rise, fall or peak) and whether or not other parameters accounted for a lack of perceptibility in the pitch movement.

2. RESULTS. There appeared to be a characteristic difference between the pitch movement in prominent, super-prominent and non-prominent words. As a result of this investigation it can be said that most of the prominent sentence accents are marked by rising pitch movement, either on the syllable or as a difference between two successive syllables. In the prominent words the different types of the pitch movements (fall, rise, or peak) were counted. Results of the investigation show that most prominent words do have a pitch movement (fall, rise or peak), but are generally accompanied by some dynamic rising movement.

For the 32 utterances in the corpus, Table 1 shows a preponderance of rising pitch movement in the prominent points of each prosodic schema containing 'I Think…' (45 prominent points out of 107 total for the 32 schemas). The first three columns indicate respectively the number of prominent points which exhibit rising fundamental frequency on the syllable, falling movement (but preceded by a jump up in values from the preceding syllable), or a peak (a rise followed by a fall on the syllable. The fourth column indicates no movement on the syllable (but following a jump up from the preceding syllable). It is worth noting that in this case, the prominent point is almost always accompanied by a jump up, as was the case for the syllables with falling movement. The one exception was Utterance No. 1. There the perception of a prominent point was due to vowel length nearly double that of the average length of vowels in the same schema.

The average of prominent points in utterances involving pitch movement on the syllable is 92.5%, involving jumps is 70%, rises 42%, falls (with jumps) 38%, peaks (rise-fall) 12%, level (with jumps) 6%. From this we can conclude that pitch movement is important in the perception of prominence, but that in any case ascending movement (either of continuous pitch, or discontinuities between two voiced segments) is more readily perceptible than descending movement.

Utterance	I	Pitch movement	THINK	Pitch movement	Transition
1	¢	Fall	1	Fall	Jump up
2	¢	Fall	¢	Fall	Rising
3	¢	Fall	1	Fall	Jump up
4	1	Rise	1	Fall	Jump up
5	¢	Level	1	Fall	Jump up
6	¢	Level	1	Fall	Jump up
7	1	Rise	¢	Fall	Falling
8	1	Jump/fall	¢	Fall	Falling
9	¢	Rise	1	Fall	Jump up
10	¢	Rise	1	Rise	Rising
11	¢	Level	1	Rise-fall	Rising
12	¢	Rise	1	Rise	Jump up
13	1	Jump/rise	¢	Fall	Falling
14	¢	Rise	1	Fall	Jump up
15	¢	Fall	1	Level	Jump up
16	¢	Rise	1	Fall	Jump up
17	1	Rise-fall	¢	Fall	Falling
18	1	Rise-fall	¢	Fall	Falling
19	¢	Fall	1	Rise-fall	Rising
20	¢	Fall	1	Rise	Jump up
21	¢	Fall	1	Rise	Jump up
22	¢	Level	1	Fall	Jump up
23	2	Jump/rise	¢	Fall	Falling
24	¢	Level	1	Fall	Jump up
25	¢	Fall	1	Fall	Jump up
26	1	Rise	¢	Fall	Falling
27	¢	Rise	1	Rise-fall	Rising
28	1	Rise	¢	Fall	Rising
29	1	Jump/fall	¢	Fall	Falling
30	1	Jump/rise-fall	¢	Level	Falling
31	1	Jump/rise	¢	Fall	Falling
32	¢	Rise	1	Rise	Rising

Table 2: *Pitch movement on I THINK (Prosodic levels are: 1/Prominent, 2/Super-prominent, and ¢/Non-prominent).*

For the 32 utterances, Table 2 indicates a preponderance of rising pitch movement in the sequence 'I think'. The column indicating pitch movement for the pronoun 'I' is marked in gray for all prominent syllables, which reveals a consistent dependence on ascending (rise or jump) pitch movement. We see that if a rise is a necessary condition for the perception of prominence, it is not a sufficient condition. This is particularly true when a small rising movement is continued during the voiced portion of the following verb, which 'overshadows' it. As for the syllable corresponding

to the verb 'think', the columns indicating pitch movement (and the transition from the pronoun) are also marked in gray for all prominent syllables, which again reveal a consistent dependence on ascending (rise or jump) pitch movement.

The marking of prosodic levels on the two initial syllables 'I' and 'think' corresponds to specific iconic contours[2] of pitch prominence. In general, it can be said that there is a relationship between a pitch movement and the perception of a prominent accent. A relatively high frequency step and a clearly perceptible pitch movement are indications that people will perceive a given word as prominent. The distributions of using rise, fall or peak pitch movements for prominence are not the same. The results indicate that most (but not all) of the pitch movements in prominent and non-prominent words can be distinguished by the acoustical information of frequency.

3. DISCUSSION. The phonological imprint of the utterer's subjectivity is directly reflected in the choices that one is liable to make in structuring discourse: to position oneself in a discussion, identifying with one's own arguments and opposing them to the arguments of one's 'co-utterers', or conversely, by identifying with the arguments of others, implicitly co-opting them. We can see this most clearly with the pronominal referent of the ego ('I'), which for Benveniste (1971:244), belongs in turn to all who participate in discussion; this is of even greater interest when combined with the verbal form 'think', expressing the relation of the speaking subject to his *dictum*.

3.1. EXAMPLES FROM THE CORPUS. Four examples of prosodic configurations taken from our mini-corpus will illustrate the functioning of 'I think' in differing discursive strategies. We will first consider the possible nuance of meaning exemplified in an utterance where neither 'I' nor 'think' is marked by a prominent point; second, an utterance with a prominent point corresponding to the verb 'think'; third, an utterance with a (super-)prominent point corresponding to the personal pronoun 'I'; then finally an utterance with both words corresponding to prominent points in the prosodic schema. The general theme treated by all participants is whether or not there is any evidence of wrongdoing on the part of President Clinton or his staff in their treatment of the Whitewater affair. (In this and following transcriptions a single slash, /, marks a brief pause with no pitch movement, and a double slash, //, marks the end of a prosodic schema with falling intonation.)

(1) MK: The opening statements were more forceful/ There seem, appear to be numerous discrepancies on the part of the various players/ so there could be something there/

CR: Like what ?

MK: at least of an unethical nature // Well, to begin with, Roger Altman, is, is clearly on the hot seat // um, beginning to be on the hot seat/ and he'll be before the Senate committee next week // He testified to minimal contacts between Treasury/ and Resolution Trust Corporation people uh/and the White House when he appeared before the Senate

last February 24th, I believe it was. Since then, we've had a whole, series
of revelations that it was / these contacts were much more extensive/
<u>and I think we're beginning to see a pattern</u> /of why it was important
/or why it may have been important for, people in the White House to
try to keep a lid on the RTC investigation. (Utterance No. 2)

In (1), the absence of prosodic prominence on the initial sequence transmits their
meaning at face value, with the speaker designating himself simply as the origin of the
expression which follows, and avoiding any nuance or contrast. Indeed, the economy
of effort in articulation draws the attention to the substance of the message, to the
detriment of the messenger. This is hardly surprising at the start of the discussion, as
no other speaker has at this point has offered a diverging analysis of the situation.

(2) CB: But the point is a lot of this wasn't raised- why wasn't this, all this
         raised in the presidential campaign ?
    CR: In the campaign, in the coverage of the 1992 campaign.
    MK: Oh, it was.
    CR: There was some criticism, in the fact// I mean The New York Times
         did raise the issue of/ what was going on in Arkansas/ and, and other
         publications, and I can't speak for Time and Newsweek/
    JK:Yeah, but in the, in the densest, most…
    CR: And it kind of, but really didn't…
    JK: …incomprehensible way; we never got to the, to the $100, 000 in this…
    MK: It IS dense// This sort of affair is somewhat…
    JK: …in the commodities business. I *think* that/ Carl's right// Every politi-
         cian is hiding something now/ and it is, if you could sum it up in a
         word, it's their humanity. (Utterance No. 12)

In (2), the utterer concedes a point to one of his fellows, which he takes the liberty
to elaborate on; the prominent point on 'think' foregrounds the vulnerability of his
adherence to this argument. We might contrast this with a super-prominent point
here, which would gloss as 'I say he may be right, yet I have good reason to doubt it'.

(3) CR: Let me go to, to John. Your paper has been very very critical/ and
         your editorial page has been enormously critical /of the Clinton White
         House. What do you think so far//
    JF: <u>*I* think this whole process</u>/ really is designed to /downplay the whole
         scandal/ whatever is THERE / and if you LOOK at the Congressional
         hearings/this is the first Congressional hearing we've ever had/ where
         Congressmen actually want to put people to sleep/ and want the cam-
         eras to go away// (Utterance No. 7)

In (3), the participant seeks, by marking the pronoun with an acoustic prominence, to contrast his opinion with those of the others who have already spoken. This conscious choice establishes his argument as the most accusatory, and possibly the truest. It does not, however, prevent others from rallying to it, but on the contrary seeks to convince.

(4) CR: Joe, what's, what do you expect to come out of this that might be dam-
          aging to the Clinton presidency?
     JK: Embarassing/ people changing their stories, Treasury officials fighting
          with each other /that sort of thing// But, I disagree with Michael just a
          little bit// I, I think that, there really isn't all that much there , there// uh
          I… I *think* that there's not going to be *an*y evidence that an investiga-
          tion was impeded// I think that there is going to be some evidence that
          there were meetings where people from the,the Treasury Department
          told the White House that there was a, an investigation, in progress, but
          you know, it's kind of ironic. (Utterance No. 4)

In (4), the prosodic form adopted by the utterer is modulated by a pause (and repeti-tion of the personal pronoun) between the two elements, which renders them as two successive prominent points. Even without the repetition of the pronoun, a pause (or unstressed element) seems necessary in order to realize two successive prominent points. This configuration differs from the first in that the express identification both of the utterer as the source of the dictum and the nature of the relationship between utterer and thought content are made explicit. Not choosing to highlight this fact for the benefit of the co-utterer suggests that these factors are either clear for the latter, non-pertinent for the utterer, or both. The nature of this utterance tends to convince not only by the prosodic choice made by the utterer, but also by his insistence on the lack of evidence (prominent point on *any*).

Although other configurations are theoretically possible for the couplet, our corpus only covers these four common cases. The analysis of many examples reveals the working of two inter-dependent discursive principles: taking the spotlight on the one hand, which involves an insistence on the primacy of one's own point of view (emphasis on 'I') on the level of the enunciative exchange (turn-taking in polite circles), and a modalizing of one's endorsement of one's own 'thought content' dictum on the other. A weakening of relative prominence in either case results in a weakened position, to the opinion of one's co-utterers, or in relation to one's own opinion (to the point of calling it into question).

3.2 THEORY. The Theory of Enunciative Operations propounded by Antoine Culioli takes the speech event as a starting point, with the concept of the Speaking Subject or utterer and the Moment of Enunciation (Uttering) as the center of all phases in the construction of an utterance[3]. We find three values for locating operations in relation to the utterer or Enunciating Subject (S_0): **identification** with S_0 (1st person pronoun);

difference in relation to S_0 (for the 2nd person); and **non-relatedness** in relation to S_0 (for the 3rd person).

Prosody, as an integral part of the acoustic image of the utterance, should be taken into account when considering the multiple facets of the construction of meaning transmitted to and shared with the co-utterer. Our contribution is therefore to add prosodic data input to the parameters of the Subject (S_0), and Time/space coordinates (T_0) in an analysis which is already resolutely cognitive, but grounded in individual discursive acts. The proposed method here first identifies the acoustic profile of discrete levels for the prominent points in the corpus utterances; these levels are then analyzed in terms of relations constructed on the basis of the semantic core of the utterance, or *lexis* relations which represent an initial cognitive level of choice (Culioli 1990, 1995).

In previous work (Schaefer, in press), I have attempted to show that four discrete prosodic levels can be shown to operate for personal pronouns in sentence initial position, where vowel reduction in the monosyllabic form is the most common. The four levels, corresponding to Prosodic Level 0 (encliticized form with vowel reduction), Prosodic Level 1 (full phonetic form plus minor pitch extrusion) and Prosodic Level 2 (full form plus major pitch extrusion), are linked to specific discursive modes of reference. A fourth 'minimal' level (¢) covers cases where the full phonetic form is maintained, but in the absence of pitch extrusion[4]. These levels are analyzable, in a linguistic sense, as abstract 'paths', which are the traces of the utterer's choice within a cognitive paradigm (of a definable group of elements, i.e. the co-utterers in the Whitewater discussion—see note 1).

On this view, Level 0 (or ¢, if vowel quality is maintained) represents the simple selection of the element to which the pronoun refers, with no mention of the paradigm (i.e., *others*) to which it belongs. The Level 1 makes a distinction in the choice of an element in a paradigm (the choice is schematized as a path among other possible paths) without explicitly rejecting the other elements. Level 2 always corresponds to a specific choice which explicitly rejects another element or elements in the paradigm, and is contingent upon the pre-construction of the paradigm in the context of the situation.

4. CONCLUSION. With respect to the statements that we considered to be worth investigating, we now come to the following conclusions.

Pitch movement and perceived prominent accent are related. Not all of the prominent accents in the sentences are produced with a pitch movement, but the large majority (92.5%) are. The most prevalent form of movement is a jump up to the perceived prominent syllable (70%). In the remainder of cases, it seems that the duration of the vocalic portion of the syllable may be responsible for the perception of prominence. In such cases, the length of the vowel approaches double the average length of all other vowels in prominent syllables of the same schema (uncorrected for inherent vowel articulation rates).

Two distinct levels of pitch movement in the prominent words can be clearly distinguished from those in non-prominent words. The pitch movements in the super-prominent syllables are effected with a greater difference in frequency than

the pitch movements in the prominent /non-prominent words. In general we can say that the pitch movements in prominent words are more clearly effected than the pitch movements in the other words of the sentence. On the other hand, they are in no way related to a simple static point corresponding to an accented syllable bearing sentence stress, whether it be characterized as high, medium or low (H, M, L).

The pitch movement in prominent words, occurring in the stressed syllable of the prominent words, corresponds to prosodic markers of linguistic operations. In the case of 'I think…' in initial position, these operations can be traced to turn-taking, positioning of one's argument relative to those of one's co-utterers, and to the evaluation of the importance of one's arguments relative to the arguments advanced by one's co-utterers. Depending on the positioning of the prominence, the utterer can modulate the meaning of his utterance.

We have tried to show on a very small scale that a careful methodology can provide provisional evidence for the production of meaning through the manipulation (on the part of the utterer) of prosodic markers. It is important to add that the adjunction of prosodic schema is not simply an add-on to the syntactic or semantic core of the utterance, but in certain ways allows the utterer to manipulate the (acoustic) form of the utterance in ways which actually modify meaning.

The prominent points in the utterances and corresponding prosodic levels pertinent to the construction of meaning permit the inclusion of acoustic analysis in the larger framework of the cognitive operations involved in the utterance act. Prosody, as an integral part of the acoustic image of the utterance, must be taken into account when considering the multiple facets of the construction of meaning transmitted to and shared with the co-utterer. Our contribution is therefore to add prosodic data to the parameters of the Enunciative Subject (S_0), and Time/space coordinates (T_0) in an analysis which is already resolutely cognitive, but grounded in individual discursive acts. On the basis of these parameters of the speech event, problems of the functioning of discourse can be addressed more completely. Further evidence of this can only be found in extensive analysis of corpora, validating the proposed theory in a constant to-and-fro between the enunciative model and the speech data it is derived from.

---

[1]   According to the theory, the 'utterer' or 'enunciator' is a linguistic concept, which refers to an abstract function in the production of an utterance. It is the central coordinate point in the situation of uttering. The utterer is also the origin of all successive choices that contribute to the construction of an utterance, and which make it unique. Any choice of an element at any point in the utterance is made in relation to a set or paradigm of elements, which could replace it at that point. The term co-utterer (co-enunciator) is given to the abstract function of the addressee as a necessary parameter in the situation of uttering, which is taken into account when the utterer makes these choices.

[2]   For an account of the iconic motivation of prominent points in the utterance, see Rouskov-Low (1993).

[3]   Here, the goal of linguistics is 'to apprehend language through the diversity of natural languages' (Culioli 1990:179). This involves a quest for the invariants which underpin and

regulate language activity: 'The goal is not to construct a single grammar, but to reconstruct, by a theoretical and formal process, the primitive notions, elementary operations, rules and schemata which generate grammatical categories and patterns specific to each language.' This in turn implies evidence which is based on a theory of observable data: the analysis of authentic utterances, which helps develop a holistic model to account for all facts, including ambiguity, slips of the tongue, deformations, metaphors, etc.

4   This distinction has been discussed at length in Schaefer (1998).

REFERENCES

BENVENISTE, EMILE. 1971. *Problems in general linguistics (Miami linguistics series 8)*. Coral Gables: University of Miami Press.

CULIOLI, ANTOINE. 1990. *Pour une linguistique de l'énonciation*. Paris: Ophrys.

————. 1995. *Cognition and representation in linguistic theory (Current issues in linguistic theory 112)*. Amsterdam: John Benjamins.

LADD, D. ROBERT. 1996. *Intonational phonology*. Cambridge: Cambridge University Press.

LEHISTE, ILSE. 1970. *Suprasegmentals*. Cambridge MA: MIT Press.

NICAISE, ALAIN & MARK GRAY. 1998. *L'intonation de l'anglais*. Paris: Nathan.

ROUSKOV-LOW, JENNIFER. 1990. La prosodie et l'analyse linguistique. In *Cours de linguistique anglaise—Préparation à l'agrégation d'anglais, option linguistique*, ed. by J. Boulle: 65–73. Vanves: C.N.E.D.

————. 1993. Iconocité énonciative des marqueurs prosodiques. In *Faits de langues I*: 118-127, Paris: PUF.

SCHAEFER, STEVEN. 1998. *Le comportement prosodique des compléments en anglais américain contemporain*. Thèse de Doctorat. Université de Paris VII, Lille: Septentrion Presses.

————. in press. Prosodic constraints on the functioning of personal subject pronouns in discourse structure. In *34th linguistic colloquium at FASK, Universität Mainz*. Frankfurt am Main: Peter Lang Verlag.

❧

GEMINATES, NC CLUSTERS, AND
WORD-MEDIAL CC SEQUENCES IN PONAPEAN

CHANG-KOOK SUH
Cheonan University

IN PONAPEAN[1], only geminates and place-linked (homorganic) nasal-obstruent clusters (hereafter NC clusters) can be found in word-internal syllables[2]. In other words, coda consonants are prohibited word-medially, except for geminates and place-linked NC clusters. On the other hand, word-final syllables can have single coda consonants or double, if they are geminates or homorganic NC clusters (Itô 1989:226). Onsets permit only single consonants, so the morpheme-initial geminates appear only when the morpheme is not word-initial. (Rehg 1986, cited in Levin 1989:39).

In this paper I focus on word-medial CC sequences in Ponapean and make the following two points. First, vowel epenthesis in Ponapean satisfies PLONS (PLONS: If there are place features, then they must be in onsets [cf. Steriade 1995, Suh 1997]). Second, the geminate integrity effect (cf. section 2) is the result of a specific ranking of constraints: MAX-IO >> PLONS >> DEP-IO[3]. I also argue that vowel epenthesis is due to illicit coda consonants in Ponapean. That is, vowel epenthesis remedies the unacceptable quality of coda consonants, not the unacceptable quantity of consonants. According to Rehg (1981) and Rehg and Sohl (1979), word-medial biconsonantal clusters are split by the insertion of a vowel. On the other hand, geminates and place-linked NC clusters resist vowel insertion. At issue is the integrity effect of geminates and NC clusters in this language as well as the character of the coda consonants.

1. WORD-MEDIAL CC SEQUENCES IN PONAPEAN. Our major concern is to account for the character of coda consonants and the integrity effect of geminates and NC clusters in Ponapean, which does not allow illicit coda consonants in word-medial position. Thus, in this paper we focus on word-medial consonant clusters only[4]. The following Ponapean examples are taken from Levin (1989), Itô (1989), McCarthy and Prince (1986), and Rehg and Sohl (1979):

(1) a. *arewalla*   'to return to the wild'
         kemmad   'to change into dry clothing'
         urenna   'lobster'
         nappa   'Chinese cabbage' (loanword)
     b. *nampar*   'trade wind season'
         nankep   'inlet'
         dindil   'penetrate'

(1) c. /ak̲-d̲ei/   [akedei]   'a throwing contest'
         /ak̲-pʷuŋ/   [akupʷuŋ]   'petty'
         /ak̲-t̲antat/   [akatantat]   'to abhor'
         /kitik̲-m̲en/   [kitikimen]   'rat, indef.'
         /pʷiik̲-m̲en/   [pʷiikimen]   'pig, indef.'

Examples given in (1)a have geminates. Examples in (1)b have place-linked or hom-organic NC sequences. (1)c shows that potential biconsonantal clusters resulting from morpheme concatenation are broken up by an epenthetic vowel. In summary, as we can see in the above data, vowel epenthesis in Ponapean is motivated to remedy the unacceptable quality of the coda consonants, not the unacceptable quantity of the consonants (1)c. In the following section, we will characterize the nature of the coda condition and the integrity effect in Ponapean.

2. CHARACTERIZING THE CODA CONDITION AND THE INTEGRITY EFFECT. My goal in this section is to provide a general description of the coda consonants and geminate behavior known as integrity which has been widely discussed in the traditional rule-based approaches (Kenstowicz & Pyle 1971, Hayes 1986, Schein & Steriade 1986, etc.)

Languages differ according to the closed/no closed syllable parameter (cf. Kaye 1990). Some languages do not allow codas (e.g. Hawaiian, Desano, Fijian), while other languages allow codas, resulting in word-medial consonant clusters (e.g. Yawelmani, English, Arabic). In some languages which allow codas, only a restricted set of consonants make licit codas (e.g. Axininca Campa, Diola Fogny, Italian, Japanese, Lardil, Ponapean).

To explain the peculiar aspects of the coda consonants found in many languages, the CODACOND and NOCCDA have been proposed in Optimality Theory (hereafter OT) literature (Prince & Smolensky 1993, McCarthy & Prince 1993a, Itô & Mester 1994, among others). Although Itô and Mester (1994) try to characterize the behavior of coda consonants by combining the CODACOND and NOCODA constraints with the concept of Alignment from McCarthy and Prince (1993b), their description is unsatisfactory and cannot be generalized. On the basis of Ponapean examples, I argue that these constraints can be replaced by concrete constraints like PLONS, CODASON, and others.

Now, consider the geminate integrity issue. Hayes (1986:321) defines geminate integrity as given in (2):

(2) **Geminate Integrity**: Insofar as they constitute two segments, long segments (i.e. geminates) cannot be split by rules of epenthesis.

To account for this special behavior of geminates, attention has focused on representational properties that distinguishes geminates from singletons. This results from the unique branching geometry of geminates. In previous approaches (Hayes 1986, Schein & Steriade 1986), the integrity effect of geminates is explained by a universal constraint against crossing association lines as shown in (3) (cf. Goldsmith 1976):

(3)   *C   V   C

         Rc Rc
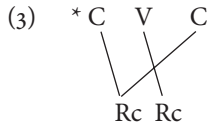
However, geminate integrity is just a general tendency and a more accurate description needs to be made to accommodate such anti-integrity cases as Marshallese (cf. Goldsmith 1990, Suh 1996). Accordingly, in this paper, the integrity effect is accounted for in a radically different way by the universal constraint interaction model of OT (e.g. Prince & Smolensky 1993a; McCarthy & Prince 1993, 1995; Suh 1997), not by the universal No Crossing Constraint (Goldsmith 1976). That is, integrity is explained in OT in which input-output pairs are evaluated by an procedure that checks all possible outputs for some input against a set of constraints. The constraints are universal and language variation is explained by the different rankings of constraints.

3. AN OT ACCOUNT.
3.1. THE CONSTRAINTS NEEDED FOR PONAPEAN. For an OT account of Ponapean, the following four key constraints are proposed. They play an important role in the analysis of Ponapean word-medial examples. First, MAX-IO and DEP-IO are core faithfulness constraints crucial to the whole OT model. In place of the PARSE/FILL type of system presented in McCarthy and Prince (1993a) and Prince and Smolensky (1993), in which the input is maintained as a literal substructure of the output, the notion of correspondence relation betweens representations plays a key role. MAX-IO and DEP-IO are defined as follows:

(4)   **MAX-IO**: Every segment of the input has a correspondent in the output (McCarthy & Prince 1995)
(5)   **DEP-IO**: Every segment of the output has a correspondent in the input (McCarthy & Prince 1993a, Prince & Smolensky 1993)

For the analysis of coda consonants, we need a constraint on the coda: PLONS:

(6)   **PLONS:** If there are Place features, then they must be in onsets (Suh 1997, cf. Steriade 1995)

The coda node will, in many cases, fail to license place features. The constraint PLONS has been developed from the discussions of Steriade (1982, 1995), Itô (1988), Itô and Mester (1994), Heiberg (1993), Scobbie (1992), and Suh (1997), among others. The main idea of Steriade's proposal (1995) is that the consonantal point of articulation features are directly licensed in the onset, indirectly so in the coda:

'[αF], where F is a consonantal point of articulation feature, must be licensed, in at least one associated segment, by membership in the onset.' (Steriade 1995:43).

Thus, the coda consonants in geminates and in NC clusters are assumed to have no place features in the phonology. Instead, coda shares the same place features with the following element (i.e. onset) in the phonetic component by the coarticulation process. As Keating (1988) puts it: 'coarticulation occurs in part because segments may lack inherent specification for particular articulations'. If a coda lacks a place specification, it is coarticulated with the following onset.

Finally, following the proposals for alignment by McCarthy and Prince (1993a, b, and c), I postulate ALIGN(WD-R, M-R) to capture the morpheme-final requirement on the surface at the right edge of a word:

(7)   **ALIGN(WD-R, M-R):** The right edge of every word coincides with morpheme final elements (]WD=)M) (cf. McCarthy & Prince1993a, b, and c, 1994; Itô & Mester 1994)

As expected, this constraint plays an important role in the analysis of word-final consonant cases.

3.2. RANKING OF THE CONSTRAINTS. In Ponapean, vowel epenthesis is due to illicit coda consonants, not to the consonant clusters in syllable edges[5]. In actuality, the two consonants in word-internal position belong to two different syllables, and thus there is no problem of unacceptable consonant clusters in the syllable margins. Here, PLONS plays a central role in accounting for this phenomenon and is highly ranked in Ponapean.

PLONS is higher ranked than DEP-IO (PLONS >> DEP-IO). This allows vowel insertion to shift a problematic coda consonant to onset position in a new syllable. Thus, DEP-IO can be violated to satisfy PLONS. In place-linked NC clusters and geminates, we do not need to insert a vowel between the clusters violating DEP-IO constraint, since they already satisfy PLONS condition. I assume ALIGN(WD-R, M-R) and MAX-IO are higher ranked than PLONS and DEP-IO. Below is the summary of the ranking which is relevant to the discussion of Ponapean word-medial consonant clusters:

(8)   **Ranking of constraints for Ponapean:** ALIGN(WD-R, M-R), MAX-IO >> PLONS >> DEP-IO

In the following section, a tableau analysis will be given to show how the integrity effect of geminates and NC clusters is explained under OT framework. It will be shown that the so-called geminate integrity effect can be drawn from the interaction of the constraints as the byproduct of the constraints, without any specific stipulation or treatment[6].

3.3. A TABLEAU ANALYSIS. First, we will look at an analysis deriving the (geminate) integrity effect. Tableau 1 and Tableau 2 show how the integrity effect is produced.

/nampar/	ALIGN(WD-R, M-R)	MAX-IO	PLONS	DEP-IO
☞ a. nam.par			*	
b. na.m*a*.par			*	*!
c. na.mar		*!	*	
d. nam.pa.r*a*	*!			*
e. nam.pa	*!	*		

Tableau 1. /nampar/ 'trade wind season'

μ⁷ | /napa/	MAX-IO	PLONS	DEP-IO

Tableau 2. /nappa/ 'Chinese cabbage'

Recall that the integrity effect permits no vowel insertion into homorganic NC clusters and geminates. Let us first look at homorganic NC clusters[8].

In NC clusters like /nampar/ 'trade wind season', as shown in Tableau 1, candidate *a* is selected as the optimal output. Insertion of an epenthetic vowel between the NC cluster causes a violation of DEP-IO as well as PLONS (candidate *b*). Because of this, *b* is eliminated. In *c*, because of the deletion of a word-internal segment, we have also crucial MAX-IO violation. Changes in word-final position do not help either, as shown in *d* and *e*. That is, addition or deletion of a word-final segment causes a fatal ALIGN(WD-R, M-R) violation. Thus, they are all eliminated.

Now let us consider geminate cases as shown in Tableau 2. In the case of /nappa/ 'Chinese cabbage', which contains geminate in the middle of the word, the completely faithful candidate, *a*, is optimal, just as in the case of homorganic NC clusters. Candidate *b* is eliminated because it has a fatal violation of MAX-IO due to the underparsing of the mora. Candidate *c* is also out because it violates DEP-IO with

/ak-dei/		MAX-IO	PLONS	DEP-IO
	a. ak.dei		*!	
☞	b. a.k*e*.dei			*
	c. a.dei	*!		

Tableau 3. */ak-dei/ 'a throwing contest'*

the insertion of an epenthetic vowel into a geminate cluster. In this way, the geminate integrity effect is produced.

Finally, we turn to the case in which vowel epenthesis separates two consonants. Here, the CC sequence does not constitute a well-formed coda-onset sequence. Thus, vowel epenthesis is required. This type of epenthesis is different from those seen in such languages as Palestinian Arabic, Pero, Berber, etc.. In these languages, epenthesis resolves an unacceptable number of consonants in syllable edges. In Ponapean, however, vowel epenthesis is due not to an unacceptable number of consonants in the syllables, but to illicit coda consonants.

The word /ak-dei/ 'a throwing contest' (Tableau 3) has the prefix *ak-* 'to demonstrate, demonstrating' and the stem *dei* 'far, far along'. The completely faithful candidate, *a*, crucially violates PLONS, because coda consonant [k] has its own place feature in that position. Rather, the optimal candidate has an epenthetic vowel and no coda consonant, as shown in *b*. In *b*, PLONS is satisfied at the cost of violating the lowest-ranked constraint, DEP-IO. Case, *c*, satisfies PLONS, but it crucially violates MAX-IO. Thus, it is eliminated from the competition. Among the candidates, *b* is selected as the optimal output form. This convincingly tells us that DEP-IO can be violated to satisfy a more highly-ranked constraint like PLONS.

4. CONCLUSION. It has been shown that medial non-homorganic CC sequences in Ponapean are split by an epenthetic vowel, which resolves an illicit coda problem by changing the illicit coda consonant into an onset consonant. This kind of vowel epenthesis is differently motivated in nature from the common cases of vowel epenthesis, which are motivated to resolve an impermissible number of consonants (e.g. CCC).

In addition to that, the (geminate) integrity effect arises as the byproduct of the constraints, which are independently motivated in the description of phonology. In particular, MAX-IO, PLONS and DEP-IO play pivotal roles in accounting for that phenomenon in Ponapean and presumably in other languages, too.

Finally, we might be able to explain coda-related phenomena without using CODACOND and NOCODA, which are cover terms and lack content. More substantial constraints like PLONS (and CODASON, etc.) account for the behavior of the coda consonants. Moreover, PLONS and CODASON constraints are independently motivated to explain phonological phenomena such as coda sonorantization (e.g. Persian, Hausa) and place assimilation and neutralization processes in many unrelated languages.

This study also sheds light on the typological study of vowel epenthesis caused by quality and quantity of the consonants in the syllable.

---

1   Ponapean is a Micronesian language. It is a member of the Ponapeic subgroup. It belongs to Western Ponapeic, and Mokilese and Pingelapese belong to Eastern Ponapeic (Levin 1989:5).

2   I am grateful to two anonymous reviewers for their valuable comments and suggestions. All errors are my own responsibility.

3   See figures (4), (5) and (6) for their definitions.

4   For a comprehensive analysis of Ponapean data within the Optimality Theory framework, the reader is referred to Suh 1997.

5   The nature of the epenthetic vowel is beyond the scope of the present paper. For discussion see Rehg 1986.

6   Geminates do not receive any special treatment in the OT framework, since there is no concept of rule matching against the input structure having single or double association lines.

7   We assume geminates are underlyingly moraic.

8   All notational conventions are those of OT.

REFERENCES

GOLDSMITH, JOHN. 1976. *Autosegmental phonology*. Doctoral dissertation, MIT, Cambridge MA. (Distributed by Indiana University Linguistics Club.)

——. 1990. *Autosegmental and metrical phonology*. Basil Blackwell, Cambridge.

HAYES, BRUCE. 1986. Inalterability in CV phonology. *Language* 62:321–51.

HEIBERG, ANDREA. 1993. The proper representation of Diola Fogny geminates in Optimality Theory. ms. The University of Arizona, Tucson.

ITÔ, JUNKO. 1988. *Syllable theory in prosodic phonology*. New York: Garland Press.

——. 1989. A prosodic theory of epenthesis. *Natural language and linguistic theory* 7:217–59.

—— & R. ARMIN MESTER. 1993. Licensed Segments and Safe Paths. *Canadian journal of linguistics* 38(2):197–213.

—— & ——. 1994. Reflections on CODACOND and alignment. ms. Univesity of California, Santa Cruz.

KAYE, JONATHAN. 1990. "Coda" licensing. *Phonology* 7:301–30.

KEATING, P. 1988. Underspecification in phonetics. *Phonology* 5:275–92.

KENSTOWICZ, MICHAEL & CHARLES PYLE. 1971. On the phonological integrity of geminate clusters. In *Issues in phonological theory*, ed. by Michael J. Kenstowicz & Charles W. Kisseberth, 27–43. The Hague: Mouton.

LEVIN, JULIETTE. 1989. The autonomy of the skeleton: Evidence from Micronesian. ms. University of Texas, Austin.

MCCARTHY, JOHN J. & ALAN PRINCE. 1986. Prosodic morphology. ms. Waltham MA, Brandeis University.

——— & ———. 1993a. Prosodic morphology I: Constraint interaction and satisfaction. ms. University of Massachusetts, Amherst, and Rutgers University.

——— & ———. 1993b. Constraint satisfaction in prosodic morphology I: Edge alignment constraints. Handout distributed at the University of Massachusetts, Amherst, and Rutgers University.

——— & ———. 1993c. Generalized alignment. Yearbook of Morphology. 79-153. ed. by G. Booij and J. van Marle. Kluwer, Dordrecht.

——— & ———. 1994. The emergence of the unmarked: Optimality in prosodic morphology. *Proceedings of the Northeast Linguistic Society* 24:333–79.

——— & ———. 1995. Faithfulness and reduplicative identity. ms. University of Massachusetts, Amherst, and Rutgers University.

PRINCE, ALAN & PAUL SMOLENSKY. 1993. Optimality theory: Constraint interaction in generative grammar. ms. Rutgers University and University of Colorado at Boulder.

REHG, KENNETH L. 1981. Ponapean reference grammar. University Press of Hawaii, Honolulu.

———. 1986. The phonology of Ponapean vowels. Doctoral dissertation, University of Hawaii, Honolulu.

——— & DAMIAN G. SOHL. 1979. Ponapean English dictionary. Honolulu: University Press of Hawaii.

SCHEIN, BARRY & *Donca Steriade*. 1986. On geminates. *Linguistic inquiry* 17:691–744.

SCOBBIE, JAMES. 1992. Licensing and inalterability in Tiberian Hebrew. *Proceedings of CLS* 28:457–71.

STERIADE, DONCA. 1995. Underspecification and markedness. In *The handbook of phonological theory*, ed. by John Goldsmith, 114–74. Oxford: Basil Blackwell.

SUH, CHANG-KOOK. 1996. Variation as optimality in Marshallese word-initial geminates. In *Proceedings of the Twelfth Eastern States Conference on Linguistics*, ed. by Marek Przezdziecki & Lindsay Whaley, 308–17. Cornell: CLC Publications.

———. 1997. Consonant geminates: Towards a theory of integrity and inalterability. Doctoral dissertation, The University of Arizona, Tucson.

❧

EVIDENCE FROM THE ICELANDIC PERFECT

Toshiko Yamaguchi
National University of Singapore

Magnús Pétursson
University of Hamburg

THE CONFERENCE THEME of the 28th LACUS Forum[1] is the nature of linguistic evidence, and many papers present data from different disciplines that illustrate, directly or indirectly, what Lamb calls 'the transparency illusion' (1998: 12)—the failure to discover and observe reliable data that reflects the functioning of our minds. According to Lamb, this failure appears to be partially due to the fact that our minds and their connection to the real world are infinitely complicated and elusive (see Lamb 1998:12, 161), and, consequently, much of the evidence available to linguists is still restricted to the province of 'analytical linguistics' or the analysis of *texts* (ibid 6, 9). Lamb's solution to this illusion is to propose a relational network, indicating that the scrutiny of our mental system, which is claimed to underlie our language use, is achieved by means of our associative abilities. It goes without saying that the meanings of lexemes in particular are in one way or another closely connected especially when we recognize the existence of a great number of polysemous lexemes, and this line of thinking is also shared by the majority of cognitive linguists (see for instance Langacker 1991:3). Bearing this in mind, we would like to examine Lamb's basic inquiry in the context of the conference's main theme, the nature of linguistic evidence, from cognitive-communicative perspectives. The focus on these two aspects of language is essentially not a novel idea. In Stein and Wright (1995) and Tomasello (1998), for instance, the role of the speaker as a locutionary force is ultimately the main concern in the analysis of linguistic data. Although we do not consider subtle differences in the theoretical perspectives presented in these books, this paper shares the same assumption that the understanding of a linguistic structure makes use of cognitive and communicative operations under the control of the speaker. Thus, the main objective of this paper is to reveal exactly how these operations are integrated in the formation of the Icelandic perfect.

The paper is organized as follows. In section 1 previous studies on the English perfect based on Bybee and Dahl (1989), Bybee et al. (1994), and Carey (1996) are briefly summarized. Section 2 demonstrates the facts of the Icelandic perfect. We first show that the Icelandic perfect is used to express the way the speaker construes an event from the perspective of remoteness, and, second, by examining some historical texts, we demonstrate that occurrences of the perfect serve as evidence for the emergence of the speaker's communicative strategies. In this light, the data from the Icelandic perfect signals that frequently cited semantic categories, 'anterior' and 'resultative', assigned to the perfect, might be regarded as general characterizations of the perfect but fail to pinpoint fine-grained functions operating within the perfect. Finally, we suggest, though tentatively, that the grammaticalization of the auxiliaries *hafa* and

vera arises from two interconnecting strategies, one being cognitive and the other communicative, rather than from a principle of unidirectionality (Bybee et al. 1994: 12–15), which relies heavily on the distinction between anterior and resultative.

1. BRIEF SUMMARY OF THE ENGLISH PERFECT. The perfect is often formed by two auxiliary verbs, *have* and *be*, cross-linguistically. As often stated in the literature, these two perfect forms share distinct but closely related semantics. Generally, the *have*-perfect has the semantics of anterior such that the past event has some relevance at the time of utterance, while the *be*-perfect expresses the resultant state which arises directly from the past action at the point of utterance. Both forms are conceived of as the general expressions of current relevance (see Dahl 1985:133–35), the only difference being that the current relevance interpretation in the former can be correlated to discourse components, while the latter is always lexically determined. Thus, the crucial difference between *John has gone* and *John is gone* is that the former can integrate a peripheral meaning. For instance, the semantics of the former can evoke such a situation that we cannot go to the party because we are now three; entering into the hall is only allowed when four people group together. By contrast, the semantics of the latter is restricted to the resultant state that is derived directly from the lexical meaning of *go*, that is, John's absence from the location; there are no peripheral or discourse-oriented interpretations. A diagnostic test to distinguish these two forms, which has been frequently cited, is the behavior of *still*. The stative nature of the resultative construction allows the stative semantics imparted by *still*. Thus, *John is still gone* indicates John's continued absence from the location, while *John has still gone* is assigned the dynamic semantics that no matter what you told him, John has gone anyway. Carey (1996) in this context remarks that the adverbial *since* also contributes to the distinction. *Since* is compatible with *have* (*I have seen her since Friday* from Carey 1996:33) because it refers to the up-to-the-present time period, collocating with the semantics of the anterior. The resultative *He is gone since Friday* (ibid) is claimed to be ungrammatical[2], because the *be*-perfect characteristically concerns the final state of the event, which is incompatible with the semantics of *since*. Whatever the shortcoming of these tests, the *have*-perfect emphasizes the process of a past action, whereas the *be*-perfect refers to the state which is the outcome of the past action.

Another often-cited characteristic of resultative and anterior is the claim that resultative has developed into anterior unidirectionally (Bybee et al 1994: 68). More precisely, after the loss of the agreement between subject/object and a past participle, a dominant form with *have* gradually replaced instances of the *be*-perfect. Bybee and Dahl (1989:69–70), for instance, argue that a shift from resultative to anterior is also influenced by lexical restrictions of the main verb, such that resultatives are only formed from telic verbs[3]. Aside from these observations, the hypothesis of unidirectionality has been strengthened by studies of some languages in which the inferential interpretation developed out of the perfect with the loss of its existing anterior meaning (Bybee et al 1994:73). In this context, Dahl (1985:153) and Bybee et al. (1994: 96) mention a close developmental connection between result and inference

as a triggering factor of the latter, in that both functions correlate with the result of a past action; in the former the result exists due to a past action, while in the latter the inference is drawn on the basis of the result of a past action, and this link is claimed to give rise to the inferential use of the perfect.

2. EVIDENCE FROM THE ICELANDIC PERFECT. Like English and many other languages, Icelandic uses both *hafa* 'have' and *vera* 'be' to form the perfect. Despite this fact, the perfect in Icelandic diverges in many ways from the English perfect, although they share, at first glance, basic similarities: the *be*-perfect expresses the result of a past action (= resultative), while the *have*-perfect concerns a time period prior to the point of utterance (= anterior). Upon close investigation, however, we see that an understanding of the Icelandic perfect extends beyond this two-way distinction. We highlight the following two points. First, the crucial difference between *be-* and *have*-perfects in Icelandic lies in the degree of remoteness. Second, based on our investigation of selected historical texts, we argue that the rise of the *have*-perfect with different semantics interacts with the speaker's communicative strategies[4].

2.1. REMOTENESS. We mentioned in section 1 that the crucial difference between anterior and resultative is the different emphasis on the past action in a sentence. From the *since*-diagnostic, for instance, it seems that anterior focuses more on the process of an action, whereas the focus of resultatives is on the existence of a newly arisen state . Bybee et al. (1994: 69) describe this difference in the following manner: 'A resultative… expresses the rather complex meaning that a present state exists as the result of a previous action. An anterior, in contrast, expresses the sense that a past action is relevant in a much more general way to the present moment'. One problem we encounter with this claim is that the behavior of the Icelandic perfect does not coincide with diagnostic tests offered by explanations for English. The most striking evidence is that both *hafa* and *vera* in Icelandic can allow the adverbial *enn* 'still' and both impart the same dynamic, i.e., repetitive sense, as shown in (1):

(1) a. *Hann er enn farinn.*   b. *Hann hefur enn farið.*
        he is still gone   he has still gone
        'He (is) still gone again'   'He (has) still gone again'

As illustrated in (2), both *hafa* and *vera* can also permit *síðan* 'since, after', referring to the time-span between the past and present:

(2) a. *Hann er kominn síðan í gær.*   b. *Hann hefur komið síðan í gær.*
        he is come since yesterday   he has come since yesterday
        'He (is) come since yesterday'   'He (has) come since yesterday'

As indicated in (3), *vera* is in fact compatible with dynamic adverbial expressions
such as *í flýti* 'in a hurry', which contradicts the interpretation of the *be*-perfect as an
exclusively resultant state:

(3) a. *Hann er farinn í flýti.*   b. *Hann hefur farið í flýti.*
         he is gone in.a.hurry   he has gone in.a.hurry
         'He (is) gone in a hurry'   'He (has) gone in a hurry'

These examples signal that the definition of resultative and anterior, as given by Bybee
et al. (1994:54–55) and others, does not hold neatly for Icelandic. Our explanation for
this behavior of the Icelandic perfect is that *vera* should be understood as referring to
the time span from the past to the present in much the same way as *hafa*; they are two
variants of the expression of the perfect event. One piece of evidence for this claim is
that *vera* can co-occur with a past adverbial like *í gær* 'yesterday'. In Icelandic passives
are formed by a past participle and an auxiliary *be*, which is formally identical to the
perfect auxiliary: both have the form *er* in (4). Interestingly, however, as indicated in
(4)c, passive constructions[5] do not allow *í gær*, and the adjectival construction (4)d,
behaves in the same manner. The reason for this discrepancy might be that these two
constructions do not concern the time period from the past to the present but only
express the present state. In other words, despite the fact that these three construc-
tions formally resemble each other, the perfect differs significantly from passives and
adjectivals from a cognitive point of view.

(4) a. *Veggurinn er brotnaður í gær.* (*be*-perfect)
         wall.the is broken yesterday
         'The wall (is) broken yesterday'
     b. *Veggurinn hefur brotnað í gær.* (*have*-perfect)
         wall.the has broken yesterday
         'The wall (has) broken yesterday'
     c. **Veggurinn er brotinn í gær.* (passive)
         wall.the is broken yesterday
         *'The wall is broken yesterday'
     d. **Veggurinn er blár í gær.* (adjective)[6]
         wall.the is blue yesterday
         *'The wall is blue yesterday'

As shown in (5), however, a time adverbial such as *rétt áðan* 'just now', which refers
to the recent past, is only felicitous with *vera*, not with *hafa*. Interestingly, as exempli-
fied by (6) and (7), *vera* is not possible when the sentence expresses the remote or
distant past; dinosaurs lived thousands years ago and have no relevance to the current
situation, and, likewise, the expression of *five years ago* is more distant than that of
yesterday (see examples (4)c and (4)d).

Víga-Glúms saga (Glúma)	Present	Preterite	Perfect		Pluperfect
			hafa	*vera*	
total 3232	1344	1709	75	7	97
percentage 100%	41.6%	52.9%	2.3%	0.2%	3%

Table 1. *Tokens and percentages of different tense categories in* Glúma.

functions	*Hafa*-perfect			
	Current relevance	Extended-now	Inferential	Experiential
total 75	3	15	22	35
percentage 100%	4%	20%	29%	47%

Table 2. *Tokens and percentages of different functions of the perfect in* Glúma.

(5) a. *Hann er farinn rétt áðan.*
        he is gone just now
        'He (is) gone just now'
      b. *?Hann hefur farið rétt áðan.*
         he has gone just now
         'He (has) gone just now'
(6) a. **Risaeðlur eru hlaupnar hér.*
        dinosaurs are run here
        'Dinosaurs (are) run here'
      b. *Risaeðlur hafa hlaupið hér.*
         dinosaurs have run here
         'Dinosaurs (have) run here'
(7) a. *?Hann er farinn fyrir fimm árum.*
        he is gone five years ago
        'He (is) gone five years ago'
      b. *Hann hefur farið fyrir fimm árum.*
         he has gone five years ago
         'He (has) gone five years ago'

Examples (5) to (7) support our claim that the difference between the *be* and *have*-perfects in Icelandic has something to do with our cognitive encoding of an event or, more precisely, our recognition of the degree of remoteness; the speaker profiles or puts emphasis on an event which is closer to him in the *be*-perfect, whereas in the *have*-perfect the speaker profiles an event which is remote from him.

2.2. COMMUNICATIVE STRATEGIES. When we look at an Old Icelandic saga text such as *Víga-Glúms saga* (*Glúma*), written around 1330, as shown in table 1, it is noteworthy that the preterite is the most frequently occurring verb form (52.9%), while the frequency of the perfect is much lower (2.3%). Table 2 shows the percentage of each function assigned to the perfect (total 75) in *Glúma*[7].

It is worth mentioning that many preterite forms in Old Icelandic can be replaced by the perfect in Modern Icelandic; in other words, the preterite still retained the form whose function in a given situation would correspond to that of the present-day perfect. The text in (8) contains *hefir komit* 'have come' and *drap* 'killed'; the perfect form for the latter would be the most preferred form in Modern Icelandic given the presence of the time adverbial *í dag* 'today'.

Newspaper articles	Present	Preterite	Perfect		Pluperfect
			hafa	*vera*	
total 4145	1823	1815	279	27	210
percentage 100%	43.9%	43.7%	6.7%	0.6%	5.1%

Table 3. *Tokens and percentages of different tense categories in* Morgunblaðið *(2000–'01).*

(8)   *Hann svarar: "það er satt – eigi <u>hefir</u> mér   í   hug   <u>komit</u>*
       he   answers. it   is   true – not have me.DAT in mind.ACC come
       at segja", kvað Glúmr,"   at   ek <u>drap</u> Sigmund   Þorkelsson í dag".
       to say",   said   Glúmr.NOM that I   killed   Sigmundr.ACC Þorkelsson today

       'He answers: "It is true that it has not come to my mind, Glúmr said, that I killed Sigmundr Þorkelsson today"' (*Glúma*, ch. 8, p. 25)

Co-existence of the preterite and perfect in (8) suggests that preterite forms were probably gradually replaced with perfect forms. In fact, we can often observe fluctuations between these two forms in *Glúma*, as shown in example (9), where *í dag* is accompanied by the perfect and preterite in direct speech, both expressing the same semantic content of completed action with relevance at the point of utterance.

(9)   *Síðan mælti Glúmr   við Guðbrand: "þú <u>hefir</u>   mikillar*
       since said   Glúmr.NOM to   Guðbrandr.ACC you   have   great
       frægðar   <u>aflat</u>   þér   í dag, er   þú <u>lagðer</u> at jörðu
       celebrity.GEN gained you.DAT today when you put   to earth.DAT
       Þorvald   krók   ok mikit lið   <u>veittir</u> þú oss í dag".
       Þorvaldr.ACC hook.ACC and much support.ACC gave   you us   today

       'After that, Glúmr said to Guðbrandr: "You have gained great celebrity today, when you killed Þorvaldr the hook (literally: when you placed Þorvaldr the hook to earth) and you gave us great support today"' (*Glúma*, ch. 23, p. 6)

As shown in Table 3, extracted from selected current newspaper articles in Modern Icelandic[8], although the preterite is still used dominantly (the difference from the present is only 0.2%), the percentage of use of the perfect has clearly increased. This might point to the stabilization of the perfect form and its semantic functions in Modern Icelandic. Table 4 indicates that functions associated with the perfect have changed in the course of time and in Modern Icelandic inferential and experiential are no longer the dominant uses, whereas extended-now and current relevance are more frequently used[9].

functions	*Hafa*-perfect			
	Current relevance	Extended-now	Inferential	Experiential
total 279	134	97	22	26
percentage 100%	48%	34.8%	7.9%	9.3%

Table 4. *Tokens and percentages of different functions of the perfect in* Morgunblaðið *(2000–'01).*

With this background, we argue that the gradual change from the preterite to the perfect and, hence, their simultaneous co-occurrence in scenes in *Glúma* is to be interpreted as a sign of a shift in the communicative strategies the speaker was using to adjust his expressive attitude towards the speech event he was involved in. In what follows, two explanations are presented.

2.2.1. EXPRESSIVE ATTITUDES. In this subsection, we concentrate on inferential function, demonstrating why it was preferred in old texts, while it is no longer prefered in Modern Icelandic. We assume that the inferential in old texts has a wider range of semantics, including the functions of present-day modal adverbials such as *surely* or *certainly*. For instance, as seen by the contrast between (10) and (11), the speaker's expressive power is made clearer when adverbs such as *vissulega* or *örugglega*, both meaning 'surely', are added to the Modern Icelandic sentence (11); they emphasize the force of Glúmr's conviction about his inference from what people reported to him (that they could not recognize the target, *Skúta*, and were deceived by his false name and disguise). In (10), by contrast, the perfect alone is used to convey the same situation.

(10) *"Nú hefir orðit   ráðfátt", segir Glúmr,   "þar hafe þér*
       now have become at loss   said Glúmur.NOM there have you
       Skútu   fundit…
       Skúta.ACC found

       ' "Now you have been helpless", said Glúmr, "There you have found
       Skúta…" ' (Glúma, ch. 16, p. 47)

(11) *"Nú hefur <u>vissulega</u> orðið   ráðafátt", segir Glúmr,   "þar hafið*
       now have   surely   become at loss   said Glúmur.NOM   there have
       þið <u>örugglega</u> fundið Skútu…
       you surely   found   Skúta.ACC (Modern version of (10))

Modal adverbials are found, though not frequently, with the perfect in newspaper articles in Modern Icelandic. They serve to strengthen the locutor's expressive power. In (12), for instance, the presence of *örugglega* 'surely' reinforces what the children

judge from the weather in Iceland, in addition to the more basic evidential function of the perfect based on *prima facie* result (= less rain in Iceland).

(12)   Þó   sögðu krakkarnir   að veðrið   hér hafi
        nonetheless said   children.the.NOM that weather.the.NOM here have.SUBJ
        örugglega verið betra en   heima hjá þeim þar sem miklar
        surely   been better than home with them because great
        rigningar   hafi   verið í Frakklandi að undanförnu.
        rain.NOM.PL have.SUBJ been in France.DAT recently

        'Nonetheless the children said that the weather here has certainly been better than in their home country because there has been much rain recently in France' (*Morgunblaðið*, 21. July 2001: p.4)

From these differences, we suggest that the perfect in Old Icelandic was used to transmit the existing speaker's communicative needs.

2.2.2. FREQUENT OCCURRENCE IN DIRECT SPEECH. Another striking characteristic of the use of the perfect in Old Icelandic is that it frequently appeared in direct speech. All perfect forms in *Glúma* save one, for instance, only appear in direct speech; *Egils saga,* written around 1200, contains 29 instances of the perfect, 27 of which appear in direct speech (cf. Nordal 1993:50–59). Interestingly, in modern texts perfect forms appear regardless of text type; there is no tendency for the perfect to be linked specifically to direct speech (see Yamaguchi & Pétursson in preparation). Researchers agree that direct speech, particularly when integrated into narrative texts, reinforces the interpersonal involvement of the speaker and the use of the first-person makes the narration more vivid (Tannen 1986:312). Given this perspective, then, the dominant occurrence of the perfect in direct speech supports our hypothesis that the new functions of the perfect related to the speaker's expressive power emerged first in direct speech, because it fulfills that function more successfully.

3. CONCLUSION. This paper demonstrates that cognitive and communicative mechanisms have a bearing on the understanding of a language structure such as the Icelandic perfect. Our investigation suggests a new picture of the Icelandic perfect from the perspectives of the degree of remoteness, on the one hand, and the speaker's pragmatic involvement in a speech event, on the other. Although the results demonstrated in this paper are a tentative report from our ongoing research, we believe that the findings shed light on the conference theme, the nature of linguistic evidence. The findings point us in the direction of recognizing the nature of mind and support the cognitive and communicative aspects of a linguistic system that are part of Lamb's neuro-cognitive approach. Hence, as a final point, the study of the Icelandic perfect, particularly its historical aspect, implies that the emergence of various functions such as resultative, anterior, and inferential, might not be brought about exclusively as the result of a principle of

unidirectionality, and this further hints at the invalidity of the suggestion of Hopper and Traugott (1993: 66–67) that the process of grammaticalization essentially excludes the component of communication.

1   We are grateful to anonymous reviewers for their useful comments.
2   It is not ungrammatical, though its appropriateness certainly depends on context. We are grateful to Arle Lommel for pointing this out to us.
3   See Yamaguchi and Pétursson (to appear) for counterarguments to this claim.
4   Note that we mainly deal with the perfect with *hafa* in this paper. For an elaborate study of the *vera*-perfect in Icelandic, see Yamaguchi and Pétursson (to appear).
5   We do not make a distinction between verbal or adjectival passive constructions, since both forms do not allow the time adverbial such as *í gær* 'yesterday'. Thus, *í dag* 'today' is compatible with the passive: *Veggurinn er brotinn í dag af Jóni* 'The window is broken by John today'
6   Note that these sentences become grammatical, as we envisage, when the past tense form is used for the finite verb: *Veggurinn var brotinn í gær* 'The wall was broken yesterday' and *Veggurinn var blár í gær*. 'The wall was blue yesterday'
7   All numbers are tokens.
8   We have investigated 32 articles in a daily newspaper, *Morgunblaðið*, from July 2000 to July 2001.
9   Note, however, that many tokens for current relevance are replaceable with the preterite forms, while the other three functions are not easily replaceable.

REFERENCES

BYBEE, JOAN L. *&* ÖSTEN DAHL. 1989 The creation of tense and aspect systems in the languages of the world. *Studies in language* 13: 51–103.

BYBEE, JOAN L. et al. 1994. *Tense, aspect, and modality in the languages of the world*. Chicago/London: University of Chicago Press.

CAREY, KATHLEEN. 1996. From Resultativity to current relevance: evidence from the history of English and modern Castilian Spanish. In *Conceptual structure, discourse and language*, ed. by Adelle E. Goldberg. Stanford CA: CSLI Publications.

DAHL, ÖSTEN. 1985. *Tense and aspect systems*. Oxford: Blackwell.

HOPPER, PAUL *&* ELIZABETH CLOSS TRAUGOTT. 1993. *Grammaticalization*. Cambridge: Cambridge University Press.

LAMB, SYDNEY M. 1998. *Pathways of the brain*. Amsterdam: John Benjamins.

LANGACKER, RONALD W. 1991. *Concepts, image, and symbols: The cognitive basis of grammar*. Berlin: Mouton de Gruyter.

NORDAL, SIGURÐUR (ed.) 1933. (Late 12th, early 13th century, exact date of composition unknown). *Egils saga*. Reykjavík: Hið Íslenzka Fornritafélag.

STEIN, DIETER *&* SUSAN WRIGHT (eds.). 1995. *Subjectivity and subjectivisation: Linguistic perspectives.* Cambridge: Cambridge University Press.

TANNEN, DEBORAH. 1986. Introducing constructed dialogue in Greek, and American conversational and literary narrative. In *Direct and indirect speech*, ed. by Florian Coulmas, 311–32. Berlin: Mouton de Gruyter.

TOMASELLO, MICHAEL (ed.). 1998. *The new psychology of language: Cognitive and functional approaches to language structure*. Mahwah NJ: Lawrence Erlbaum.

YAMAGUCHI, TOSHIKO & MAGNÚS PÉTURSSON. forthcoming. The speaker and the perfective auxiliaries *hafa* and *vera* in Icelandic. To appear in *Language sciences*.

———— & ————. In preparation. The Icelandic *hafa*-perfect in newspaper articles.

Víga-Glúms saga (Glúma). 1880. (Composed 1330.) Copenhagen: Hið Íslenzka Bókmenntafélag.