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ON THE VALUE OF NEUROLINGUISTIC DATA

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Modern linguistics is the study of the mental representation of language--both in terms of innate language structures and in terms of its universal/language-specific learned aspects. The mental representation of language, like all mental representation, has a physical realization: the brain, including its physical memory structures. Neurolinguistics (NL) is the field of study which uses language performance data in conjunction with neurological data to test linguistic theory. Ultimately, the linguistic constructs thus confirmed can serve as part of the foundation for a neurology of language--i. e., a direct study of the actual physical means by which knowledge of language is represented in the brain.

But why bother with the NL stage in the schema outlined above? Is it really worth the trouble? After all, psycholinguistics--which uses performance data without additional, neurological data--might seem sufficient.

To be able to answer these questions satisfactorily, we need an appraisal of the advantages and disadvantages of using NL data versus psycholinguistic data or formal linguistic data to test linguistic theory. The purpose of this paper is to provide an appraisal of the unique advantages NL data has to offer linguistics. (A later paper is planned to deal with some of the special problems involved in using NL data.)

A premise of this paper is that performance data--such as psycholinguistic and NL data--can and should be used to test linguistic theories. In discussing NL data, I will not be trying to justify this premise so much as to show that there exist certain advantages of using NL data over other kinds of evidence in constructing theories of language. Note, however, that the extent to which NL yields interesting results the premise is justified.

Below, I will be concerned primarily with language performance data obtained from brain damaged persons, including data from aphasia. For now, "aphasia" can be defined as a linguistic impairment in producing or comprehending speech due to brain damage. Of course, NL data also include the language performance of normal individuals when it can be assessed in conjunction with neurological data, such as the speech of a person undergoing cerebral blood flow monitoring.

Before discussing NL's advantages for linguistics per se, I would like to make a short, necessary detour into a well-known problem area.

1. NL as Metapsychology

Not all linguists conceive of the goal of linguistics as being the determination of the mental representation of language, but the majority probably do. Within this majority there are definitely various interpretations of what is entailed in stating that the mental representation of language is the primary object of linguistic study. However, most of this majority seems close to the intentions of Noam Chomsky in this regard. And Chomsky, at least, is clear: The constructs of linguistics (rules, categories, etc.) are hypotheses about isomorphic mental structures (Steinberg 1982:74-5).

Now comes the tricky part--the notorious mind-body problem. The question is precisely the extent to which the mind (including language-mind) is the result of, or is related to, physical states and events located in the brain. There is no end of opinions on this matter, ranging between the two extremes of

(1) the mind and body (brain) as completely separate and distinct realities; and

(2) the absolute reduction of mind to brain.

Linguists, interested in the "mental representation of language", hold a variety of positions on this question, although some sort of tentative agreement would seem to be necessary before beginning to do NL.

In this regard, we could adopt a common sense approach which is probably tacitly shared by many. Namely, we could assume a causal connection between brain and mind on the basis of manifest experience: Whenever something goes wrong with a brain the mind in question is never quite right. Such an approach may not be precise enough for some purposes, but it will probably do for ours, considering the following.

The intricacies of the mind-brain problem can be viewed as issues which we hope to clarify by research, as empirical issues in other words, rather than as necessary pre-theoretical decisions. If the mind can be an object of scientific inquiry (which is surely a premise of mainstream linguistics), then likewise for the relation between mind and brain. Ditto for the relation between language-mind and brain. Here we come to the first--and probably one of the biggest--advantages of doing NL: Doing NL provides insights into the nature of the mind-brain relation.

To the extent that mental states do not correspond to brain states (extreme (1) above), it should not be possible to do NL (or neurology of language, for that matter). Thus, the ultimate failure of the NL enterprise to relate the independently motivated, empirical findings of linguistic theory to neurological data would be an achievement--albeit of a negative kind. We would then have evidence of the ultimate unrelatability

of mind and brain. Even though of a negative nature, this information would be significant. And how else could such knowledge be gained? There are not many other cognitive sciences besides linguistics (and, by extension, NL) explicit enough to allow such a test.

However, from manifest experience and the small amount of NL research already carried out, we have good reason to view the above scenario as highly unlikely. The more we learn about the brain and language following brain damage, for example, the more we realize that there is a very close correspondence between brain structure and mental functions as hypothesized in formal linguistics.

Detour over. For the remainder of this paper I will be concerned exclusively with the advantages of NL data for linguistics.

2. NL Data as a Unique Source of Answers

NL is still underdeveloped as a field of study. However, it is already possible to see ways in which NL may be or become a main source--perhaps even the best or sole source--of certain kinds of information necessary to linguistic theory. Such information gained primarily or uniquely from NL may offer dis/confirmation of linguistic constructs, answers to questions in linguistic theory or even the prospect of unsuspected problems which linguists ought to be dealing with. In each case, linguistics could potentially benefit greatly from a consideration of NL data.

The way that the speech of brain damaged persons, for example, most often provides dis/confirmation of linguistic hypotheses can be summarized as the traumatic dissociation of linguistic systems/categories/factors along natural linguistic boundaries. Impairments to speech and comprehension reveal linguistic patterns by the systematic divergence of abnormal verbal behavior from the linguistic norm. The term "dissociation", or "isolation", refers to a condition of different effects on two or more classes of linguistic behavior (see Bisazza 1983 for discussion). Such dissociations throw into relief one linguistic construct or category against another by "selectively" sparing/facilitating (relatively or absolutely) one and selectively impairing the other. The effect or value (along some dimension) of both is thus rendered more visible. Spared functions become more salient by virtue of the removal of the confounding effects normally caused by the impaired faculties; the function of impaired faculties is rendered conspicuous by their very absence.

Psycholinguistic parallels for the dissociations encountered in NL data can often be found, as we will see. But there is also the question of how easy such dissociations are to come by outside NL, and whether they provide the same clarity of effect.

I will discuss, in turn, impairments and dissociations of entire subsystems, of performance and linguistic competence, and dissociations of single features or factors--though the distinction between these is perhaps only relative.

2.1. Example I--A Dissociation of Entire Performance Subsystems

One example of a performance subsystem would be any of the different modalities for language use: speaking, listening, writing, reading, etc. These different modalities have been of varying usefulness to psycholinguists in testing different aspects of linguistic theory. For example, in recent research the modality of reading has been the most widely used in experiments on the syntactic and lexical determinants of parsing strategies. (Of course, any linguistic factors thus found to operate in parsing receive strong empirical confirmation.) Influential examples are Frazier (1978), Frazier and Fodor (1978) and Ford, Bresnan and Kaplan (1983).

On the other hand, other modalities seem more convenient for testing other aspects of linguistic theory. For testing various aspects of segmental and feature analysis in phonology, speech production--in the form of speech errrors--seems to be a useful data source involving normal subjects (see, for example, Fromkin 1971). Comprehension is just not as useful for investigating these points of phonology. It seems that fewer errors are made at the level of phonological decoding--or perhaps more often go unnoticed. In fact, due to redundancy in the speech signal, many speech errors seem to be "corrected" by listeners! In any case, to detect segmental phonological errors in comprehension we would have to depend more on reports, or complex experimental designs.

The speech production modality in NL data is also useful for the study of segmental analysis in phonology, although the advantages are slightly different.

Consider the case of the so-called "Broca's aphasia", resulting from damage to a certain section of the anterior area of the dominant cerebral hemisphere. The precise referent of this term is a matter of some debate, but the characteristics of this disorder include a markedly impaired production of speech. Speech production may be impaired both segmentally--with many substitutions, metatheses, distortions and omissions--or grammatically--resulting in so-called "telegraphic" speech. The latter is impoverished with regard to grammatical function words, inflections, etc. Comprehension is also impaired in Broca's aphasia, although it is impressionistically better spared than production, especially in the earlier stages of the disorder. Thus, a relative dissociation between the comprehension and production modalities may be apparent in Broca's aphasia on a case by case basis, and subject to certain qualifications which are matters of debate (see Marshall (1982) for a discussion).

The segmental aspect of speech production in Broca's aphasia

provides much the same type of information as that provided in the speech errors of the normal, intact persons discussed by Fromkin in her famous 1971 paper. Blumstein (1973) has analyzed the segmental errors of Broca's (and other types of) aphasics and discussed partly similar findings.

Fromkin was able to discuss constructs in the theory of phonology using speech errors produced in non-experimental conditions by normal persons, Blumstein using the speech of brain damaged patients. Blumstein's data source was a dissociative impairment in the speech production of her subjects as a result of brain damage. It might be argued that something analogous--although of a less serious, more transient nature--was true for Fromkin's normal subjects, perhaps as a result of fatigue, nervousness, etc. However, Blumstein's data offer certain unique advantages.

First, there is the pragmatic aspect of being able to obtain a large number of errors in a relatively short time from the type of subjects Blumstein used, versus the laborious recording of spontaneous speech errors from normal persons over a long period as reported in Fromkin. Rapidity of data collection aside, it is also easier to control target utterances in the case of brain damaged subjects, since they can be expected to make phonological errors even when asked to do simple tasks, such as single-word repetition. This last advantage has two important effects:

(1) In cases where errors produce acceptable utterances (e. g., a metathesis of the consonantal segments in "bat" produces the real word "tab"), we will be more likely to spot them as errors in the speech of brain damaged persons.

(2) Since we can control the targets, we can include all of the different, possible sound combinations which should be tested for a given purpose. In the the case of collecting spontaneous speech errors we have to take what comes; we might have few data on the types of speech errors that result from less common combinations of sounds, simply because, by definition, these do not occur frequently enough to provide many chances for errors to occur. And, as follows from Murphy's Law, such combinations are often the ones of greatest theoretical interest!

The important point here is that it is difficult to systematically collect speech errors from normal persons by controlling the target utterances, because normals can not be depended on to make the "desired" errors. Often the tasks used in therapy and tests with brain damaged persons are just too easy for them. Various tricky techniques for inducing speech errors in normals and controlling targets might be imagined, such as extremely rapid repetition, administration of ethanol, tongue twisters, etc. Many such techniques have been reported in the literature; see, for example, the (fairly successful) attempt to

induce tip-of-the-tongue states in normal subjects in Brown and McNeill (1966). Such efforts are not always successful and may require intricate design and execution (see Baars 1980), frequently yielding only a few usable errors per volume of data, or raising questions about the naturalness of the task situations.

So, we are led back to the pragmatic advantages of phonological errors from a source like Blumstein's.

By a kind of curious confluence of effects, it is often the case that the performance of brain damaged persons offers a greater chance of exploring speech production than speech comprehension under experimental conditions, while normal performance offers far easier access to comprehension. This is not surprising, given the following facts.

With brain damaged persons, comprehension is often difficult or impossible to check because patients frequently lack the means to reply to questions (= part of production ability), but production is easy to test by simple tasks such as repetition, reading aloud, etc. These tasks would be too easy for normal subjects to yield interesting results, unless secondary measurements like reaction times were used, or the difficulty of the tasks were somehow increased artificially (e. g., by adding time pressure). In spontaneous normal speech, there is also the problem discussed above of controlling targets.

On the other hand, speech comprehension is easy to test with normals owing to their adequate ability to respond to probe questions. Of course, comprehension tests allow target control by their very nature.

Mini-conclusion: We can reasonably expect aphasic data to offer a better chance of examining production than many types of normal data. And, since production data are very useful for testing phonological analyses, aphasic production data should be doubly so.

So far, I have discussed some of the practical advantages of data from the dissociation of linguistic performance modalities due to brain damage. There is also a more theoretically interesting advantage to Blumstein's type of data, however.

Blumstein (1973) was able to make conclusions about the markedness of different phonological features with her error data. A large number of these errors included substitutions of one segment for another (target) segment--for example, [p] for /b/--which could not be explained as either anticipation or perseveration (see Fromkin 1971) or by other environmental effects. She found that these erroneous segmental substitutions were significantly in the direction of less marked features, as in the example just given, for Broca's and other aphasics. Thus, the theory of markedness in phonology receives some confirmation from these data. Fromkin's 1971 paper does not contain a discussion of markedness--probably for the reason that normal

speech errors often involve metatheses and substitutions which can be explained by environmental factors. Here we have an instance where NL data offer the possibility of a conclusion where the same conclusion would at least be more difficult to draw from non-NL data. Such conclusions would be more feasible with child language data than with adult psycholinguistic data, but NL data offer the advantage of a direct test of markedness in the adult system of competence.

My purpose in this section has been to illustrate the types of advantages which NL data hold in the case of a dissociation of language performance modalities. As a reminder, let me say that my intent here was not to suggest that NL data can replace psycholinguistic data such as Fromkin's. There are probably conclusions to be had from her data which would be difficult to draw from aphasic speech.

2.2. Example II--A Dissociation of Subsections of the Grammar

Above, an example was given of a dissociation (of use in testing linguistic hypotheses) at one of the broadest possible levels--that of performance modality. In this section, I will discuss a slightly more focused dissociation--this time between subsections of grammatical competence--from a different aspect of Broca's aphasia.

In a series of papers, Kean (1978, 1980, 1981, 1982, etc.) has discussed the symptom-complex termed "agrammatism", which may accompany the symptoms of Broca's aphasia discussed above to a greater or lesser degree. (Marshall (1982:402) has suggested that the articulation symptoms described in the previous section and agrammatism may occur separately.) "Agrammatism can be characterized for English as the selective loss of 'function words' and various bound grammatical elements (e. g., tense markers on verbs)" (Kean 1982:174). As a result of agrammatism, Broca's aphasics have a comprehension deficit in addition to effortful production. For example, they seem to have trouble comprehending sentences which place a heavy burden on grammatical function words for full decoding.

Kean's analysis claims that the symptoms of agrammatism can be adequately characterized only by reference to a phonological level within the grammar near the phonology-syntax interface. Her analysis states that "it is the phonological words of a sentence which tend to be retained in agrammatism" (Kean 1980:256), and Kean tries to show how this fact characterizes both the production and comprehension problems of Broca's aphasics. The crux of her analysis resides in a division of the morphemes of a language into two classes: an "open" class containing the major lexical catgeories, which receive stress; and a "closed" class, containing clitics, function words and various bound inflections, which do not receive stress. Kean claims that agrammatic patients retain access to the open class items, but not to the closed class. This dissociation between

sections of the lexicon can explain both the failure of agrammatic patients to understand certain sentences and the telegraphic nature of their speech output--which, as the term implies, is lacking in articles, prepositions, inflections, etc.

The type of sentences which Broca's aphasics have the most trouble comprehending was clearly demonstrated by Caramazza and Zurif (1976). Broca's aphasics can comprehend better sentences which permit knowledge of the world to act in lieu of syntactic parsing, as opposed to sentences for which syntactic parsing alone--heavily dependent on the decoding of function words and inflections--will produce the correct grammatical relations for their lexical items. Thus, a sentence like (1)

(1) The apple that the boy is eating is red.

(2) The lion that the tiger is chasing is fat.

(Caramazza and Zurif 1976:575)

is more likely to receive correct decoding by Broca's aphasics than one like (2). In sentences like (2), the NPs are often assigned the wrong case functions by Broca's aphasics; e. g., the lion may be thought to be chasing the tiger. The difference between sentences (1) and (2) consists of the fact that (2) is "semantically reversible"--i. e., its argument NPs could be transposed and the sentence would still make sense. Not so for sentence (1), which is therefore "semantically irreversible".

As a minimum characterization of the comprehension facts noted above, a theory would first have to mention the dissociation between (impaired) linguistic ability and (spared) knowledge of the world, which enables Broca's aphasics to do better on sentences like (2). In addition, it would have to state the precise nature of the difficulty involved in sentences like these. Kean claims that agrammatics can not access for use in syntactic computation function words and inflections, which significantly contribute to the passive form in these sentences.

Further clarification of this access problem for the closed class items is provided by the research of Bradley, Garrett and Zurif (1980). These researchers measured the reaction times of Broca's aphasics and normals asked to judge whether lexical items presented individually were English words or not. Results: Normals have shorter reaction times within the open class the more familiar a word is; within the closed class familiarity does not affect reaction time--i. e., familiar and unfamiliar closed class items are accessed with uniform speed. On the other hand, Broca's aphasics (who can do this task) show a familiarity effect for both classes. In reviewing these results, Bradley et al. conclude that closed class items are contained both in the general lexicon (speed of access to which is dependent upon familiarity) and in a special fast-access store which does not show a familiarity effect. The reason for the existence of the latter store is to facilitate parsing. Normals can use the

special store for closed class items in tasks such as Bradley et al.'s; Broca's aphasics can not. Broca's aphasics must get such items from the general lexicon, which is not the normal access route for these morphemes. Hence the familiarity effect. Presumably, the lack of a previous use of this route for parsing somehow makes it of no avail to Broca's aphasics in tasks like those conducted by Caramazza and Zurif.

The above facts and speculations reported in the work of Kean, Caramazza and Zurif and Bradley et al. point to a fairly neat dissociation in terms of impairment between two classes of linguistic items in agrammatism. Kean's theory of agrammatism is still a controversial one, but its viability to date has provided many insights into the pathology of Broca's aphasia. In Bisazza (1983) it was noted that NL does not have to do the work of speech pathology to justify itself, but that NL and formal linguistics will certainly provide a basis for much explanation of aphasic symptoms. Here, then, is a case in point.

In addition, the work summarized above underlines two advantages of NL for psychology in general and linguistics in particular.

First, note the degree of detail which linguistic theory can bring to bear on a characterization of the functional effects of damage to the physical brain. Here there is surely something of relevance to the mind-brain problem. That something is a conundrum for dualists on the mind-brain issue:

> If the mind and the brain are of two distinct substances which interact in some way as yet unknown, then why is it that characteristic brain lesions produce characteristic cognitive deficits? (Schnitzer 1982:256)

And, we might add, how is it that the cognitive deficits are so precisely describable in terms of (mentalistic) linguistic constructs? As pointed out in Section 1., the field of NL seems to offer more possibility of beginning to approach the mind-brain problem than any other discipline.

Second, the major point for linguistics to be had from Kean's work described above is its support for a general level of linguistic description/derivation. The fact that this level of grammatical description--i. e., the level at which word boundaries have been assigned and readjusted (Kean 1978:190)--can be applied so rigorously in the description of a symptom-complex of aphasia lends empirical support to the theoretical validity of that level as posited in formal linguistics and, by extension, to the constructs which make up this systematic level of description. Note that it would be hard to find data capable of simultaneously lending support to an entire level of linguistic description from psycholinguistic studies on normals. Bradley et al. demonstrated a different type of lexical access for open and closed class items in normals, but this is not evidence for an entire level of linguistic description. The evidence for such a

multi-faceted construct as a certain "level" of phonological representation must be correspondingly wide-ranging. An aphasia symptom-complex, involving a variety of effects on a variety of constructs, is one of the few things that fills the bill.

2.3. Evidence Provided by the Isolation of Single Factors-- A Detailed Case of NL Data to the Rescue

A major problem for psycholinguistic tests of linguistic theory using normal subjects involves the notion of "controlling intervening, or confounding, variables". That is, in psycholinguistic research it is often difficult to see the effect of the factor(s) we are interested in because of the difficulty of eliminating/controlling/analyzing-out the effect of other factors known to be relevant to the behavior under study.

For example, in tachistoscopic tests of the effect of lexical category (i. e., NOUN, VERB, etc.) upon visual recognition speed, it is necessary to control for a bewildering variety of factors: orthographic complexity, length, concreteness, morphological complexity, imageability (as distinct from concreteness), familiarity, etc. However, NL data from brain damaged persons sometimes are the result of a combination of influences making the control of possible, normal intervening variables either unnecessary or irrelevant. In such cases, it is thus possible to study a linguistic factor or factors in a (perhaps unnatural) situation of relative isolation from other linguistic or performance factors. An example follows.

The kind of tachistoscopic work just referred to makes a good starting point. First, necessary background:

Tachistoscopes are machines used for flashing visual stimuli at rapid exposure durations. Simple versions are used in teaching speed reading. Versions used in psychological research are slightly more elaborate, allowing a more accurate control of, among other things, exposure durations.

When normal subjects are asked to recognize single words presented at very short durations with a tachistoscope, it turns out that nouns can be recognized at faster durations than verbs. This result is fairly robust, having been reported in several studies (e. g., Paivio and O'Neill 1970, Holmes, Marshall and Newcombe 1971, Bisazza 1980). To make a long story short, non-linguistic factors, such as concreteness, can be eliminated as the cause of the results reported in these studies (see Bisazza 1980:34-71 for a review). This leaves us to imagine something linguistic about the category NOUN which might explain the experimental results. Marshall, Newcombe and Holmes (1975) and Bisazza (1980) both hypothesize that it is the number of arguments lexical items co-occur with in sentences which determines their processing complexity when recalled in isolation from long term memory (LTM). Marshall et al. claim nouns are easier to process than verbs, because they just so happen to have

fewer arguments on average than verbs. The noun "gift" is a three argument word, like "give", because we can say "Mary's gift of macadamia nuts to her friend" (cf. "Mary gave macadamia nuts to her friend"). But, in general, nouns (e. g., "apple", "road", etc.) co-occur with fewer arguments than verbs.

By contrast, in Bisazza (1980) I claim that it is not the average number of arguments that nouns and verbs co-occur with in sentences which determines tachistoscopic recognition difficulty; nor is it the absolute number of arguments that lexical items can co-occur with. (Marshall et al. (1975) would claim that "gift" and "give" should have the same processing difficulty, based on their absolute number of arguments.) Rather, I claim it is only the number of obligatory arguments which determines the processing complexity of single lexical items in isolation. Thus, according to Bisazza (1980), "gift" should be easier to process in isolation (such as in a tachistoscopic recognition task), since it has fewer obligatory arguments than "give" required for its use in a grammatical sentence. (Compare "Books make nice gifts" with *"He gave to him" in the sense of "hand over".) My theory states that it is only the minimum number of arguments required for use in a grammatical sentence which will be recalled from LTM along with a word seen in isolation--i. e., a situation requiring a minimum of syntactic processing. And it is a general characteristic of nouns in English, and many other languages, that they have fewer obligatory arguments than verbs.

Now, with this much as background, I will move on to an explanation of the kind of advantages NL data can offer in terms of the study of relatively discrete linguistic factors.

For my dissertation (Bisazza 1980), I first set about conducting a psycholinguistic test of my theory versus Marshall et al.'s using Japanese stimuli and normal Japanese adult subjects in a tachistoscopic design. The Japanese stimuli consisted of single argument, derivationally related noun-verb pairs like yasumi-yasumu ("(a) rest"-"(to) rest"); and multiple argument pairs like nayami-nayamu ("(a) worry"-"(to) worry"). (See Tonoike and Bisazza 1984 for a discussion of the many similarities between such pairs and English derivational pairs like "gift"-"give", including the parallel of optional arguments for Japanese nominals derived from verbs with obligatory multiple arguments.) These stimuli were superior to analogous English pairs, since they made it possible to control many potentially confounding factors (such as orthographic length) impossible to control in English. (E. g., many English derived nominals increase in length over their source verb by the addition of a suffix such as "-ation" or "-ment".)

The results of this Japanese tachistoscopic experiment were somewhat equivocal. They supported my theory in that Japanese nouns similar to English "gift" were easier to recognize than derivationally related verbs parallel to "give". They seemed to support Marshall et al. in that single argument Japanese nouns and verbs were easier than nouns parallel to "gift". My theory

entailed that single argument nouns/verbs should be no easier than nouns like "gift", since the latter can be used with as few arguments as the former in sentences.

To explain the part of the results that did not agree with my theory I argued that it had not been possible to completely control the tachistoscopic stimuli for familiarity. A post-test ranking of the stimuli by the subjects themselves showed that the single argument items were more familiar than the multiple argument items. I had balanced the stimuli for frequency in print according to word counts published in Japan, but such frequency does not overlap perfectly with familiarity--one of the control problems mentioned at the beginning of this section!

Thus, I claimed that the only reason the single argument nouns/verbs were easier than the nouns like "gift" was their greater familiarity as revealed in the rankings. The nouns like "gift" were not significantly different in familiarity from the verbs like "give", and so that part of the results--faster recognition for nouns like "gift" than for verbs like "give"--which supported my theory stood.

The line of argument just sketched might do faute de mieux but is less than completely convincing. However, I was able to provide more convincing evidence using NL data which allowed an isolation of the linguistic factors implicated in these theories from the (confounding) performance factor of familiarity.

In an earlier paper on the goals of NL (Bisazza 1983), I discussed at length Whitaker's (1972) paper on noun facilitation among certain English-speaking brain damaged patients. There it was noted that such patients find it easier to produce nouns in their spontaneous speech, repetition, etc. than verbs. Similar patients can be found among speakers of Japanese, as well as many other languages.

Now, I also mentioned in that 1983 paper that a major concern in NL research is to demonstrate parallels between aphasic and normal language processing along linguistic parameters. In addition to being a research goal, such parallels often have to be assumed as a methodological premise for NL work on the language of brain damaged persons. Linguistic factors operative in cases of brain damage which have no effect in normal linguistic behavior are problematic at least, and perhaps anomalous. I thus decided to conduct another test of Marshall et al.'s theory and my account of noun facilitation in tachistoscopic designs, this time using NL data from a brain damaged speaker of Japanese. (My theory, as well as Marshall et al.'s, was intended to generalize to such cases.) Such a test would both help decide between our competing theories and also provide information on whether noun facilitation in brain damaged persons is parallel in terms of causality to noun facilitation phenomena shown by normals in tachistoscopic and other tasks.

A tachistoscopic test would have been too difficult for most

brain damaged subjects, so in my dissertation research I decided to use the tachistoscopic stimuli in a simple reading aloud test with a patient who already showed some evidence of noun facilitation. (Such tests are often used to assess linguistic capacity in brain damaged persons. See Holmes et al. (1971) for a good example.) As in the tachistoscopic experiment, the stimuli were presented completely in kana (Japanese syllabary). Later, I was able to test another patient in the same way. Both subjects had large lesions centered primarily in the left parietal region and dyslexia, in addition to a general impairment of other language functions. The results for two tests with the first subject and one with the second subject are shown below in Table 1.

The number of correct responses in Table 1 is the key to the respective difficulty of the four categories of stimuli: Single Argument Nouns and Verbs, Multiple Argument Nouns and Verbs. As can be seen, the first three categories were approximately equal in difficulty, with only the fourth category (Multiple Argument Verbs) having significantly fewer correct responses. This agrees with my 1980 hypothesis that multiple argument nouns like "gift" should be equal in processing difficulty to single argument nouns and verbs, since they can be used in sentences with as few arguments as the latter two categories. (Recall that in the tachistoscopic test with normal Japanese adults there was a difference between multiple argument nouns and single argument nouns/verbs due, I argued, to differences in familiarity.) Marshall et al.'s theory was thus disconfirmed to the extent that it did not provide for the effect of optionality of arguments.

Table 1. Results of a Reading Aloud Test for Single and Multiple Argument Nouns and Verbs with Two Japanese Dyslexics.

	Number of Stimuli Read Correctly (N= 15 in Each Category)			
	Single Argument		Multiple Argument	
Subject:	Nouns	Verbs	Nouns	Verbs
OH				
first test	11	9	9	4
2 mos. later	14	12	12	5
KS	9	10	9	4
Totals	34	31	30	13
(Total possible N in each category = 45)				

The reasoning behind this conclusion is as follows. The causal mechanism of noun facilitation in brain damaged persons and normals (e. g., in tachistoscopic tasks) is the same--i. e., the optionality of arguments associated with derived and other nominals. The tachistoscopic task brought out both this effect (multiple argument nouns were recognized more easily than multiple argument verbs) and the confounding effect of familiarity (single argument nouns/verbs were easier than the multiple argument verbs and nouns). The two brain damaged subjects were given a simple reading aloud test, which would have been too easy for normal subjects. Their linguistic impairment made the effect of number of obligatory arguments apparent even in this simple task. However, these two subjects did not show a familiarity effect (at least for tasks at this low level of difficulty). Therefore, their performance followed exactly the predictions made in Bisazza (1980) with regards to the effect of optional and obligatory arguments.

A crucial point: Both of these patients had been tested for their reading ability on familiar and unfamiliar items prior to my test, and neither had shown a familiarity effect. So we have an independent reason for viewing the results in Table 1 as being free of such an effect.

Note also that any task--such as reading aloud--easy enough to avoid a familiarity effect in normal subjects would probably also have had no effect for number of obligatory arguments. Thus, the isolation of the linguistic factor of obligatory arguments--and hence the testability of the two theories of noun facilitation--came about in the case of the NL data as a result of the confluence of two conditions:

(1) The brain damaged subjects had a linguistic impairment severe enough to show up in a relatively simple task; and

(2) They also had a relatively normal response to the factor of familiarity, which meant that at the level of difficulty represented by this task familiar and unfamiliar items could be read aloud equally well.

Another twist: A second unique aspect of my NL noun facilitation data compared to normal data was the subjects' tendency to nominalize verbs in reading them aloud. That is, when asked to read aloud a word like nayamu ("(to) worry"), they often responded with a derivationally related nominal (e. g., nayami--"(a) worry"). Whitaker's (1972) English-speaking subjects also made such nominalizations (indicating that this is not a fluke of Japanese), despite greater orthographic length (usually a source of difficulty for brain damaged persons). My subjects made most of their nominalizations in response to multiple argument verbs, as in the above example; the couple of nominalizations made in response to single argument verbs seemed due to the testing order. These nominalizations in response to multiple argument verbs can easily be explained in my theory:

The change to a derived nominal represented by a patient's nominalization is a change from obligatory to optional arguments, which do not have to be recalled from LTM in simple tasks requiring no syntactic computation. Marshall et al.'s theory, based on absolute number of arguments, can not explain these data. Most importantly for the purpose of this paper, these nominalizations, which are an important part of the arguments in Bisazza (1980), came only from the NL data. In my tachistoscopic experiment with Japanese stimuli there was almost no guessing; the subjects either recognized a word or had no response.

Thus, the unusual dissociation of linguistic and performance factors in these brain damaged subjects along natural lines made possible the confirmation of a particular psycho- and neurolinguistic theory over another, in the process confirming the nature of the linguistic constructs (e. g., the optionality of arguments for derived nominals) used in one of these theories. Recall that the tachistoscopic test of these two theories had been impossible to control completely despite the advantages of Japanese over English stimuli for such a test.

Finally, it should be emphasized that the above example of a way in which NL data offer unique advantages in the testing of competing theories of performance did really produce information relevant to formal linguistic theory. Namely, the demonstration that lexical items like "gift" are recalled from LTM without non-obligatory arguments in simple tasks argues strongly against a generative semantics approach to derived nominals (and any other transformational approach) in which only verbs are listed in the lexicon and related nouns are derived by rule, as in Lakoff (1970:58). If such transformations really existed, access to the source verb when a derived nominal was "derived" from LTM would surely activate its obligatory arguments. The lexicalist approach to derived nominals outlined in Chomsky (1970)--along with other non-transformational accounts of derived nominals--is thus further confirmed as a result of the NL data discussed above.

In his 1972 paper, Whitaker also deals with noun facilitation, including nominalizations, by brain damaged patients as evidence in favor of the lexicalist hypothesis. Whitaker's data are relevant to this issue and suggestive. He claims that derivationally prior items (i. e., verbs according to generative semantic theory) are not likely to be impaired while derivationally more complex items (i. e., derived nominals) are spared in any plausible model of brain dysfunction. While agreeing in principle, in Bisazza (1983) I claimed that Whitaker's conclusion in favor of the lexicalist approach based on his noun facilitation and nominalizing data was not unassailable, since he had not demonstrated unequivocally that the cause of such facilitation was really linguistic in nature and tied up with the issue of the relation between derived nominals and source verbs. Such a demonstration is emphatically necessary, since a non-linguistic cause behind the noun facilitation data--e. g., the greater imageability of derived nominals, a "nomination" can be

visualized as the person or thing nominated--would simply make all discussion of linguistic theories irrelevant. Whitaker does briefly mention that such non-linguistic factors do not seem too plausible, but this does not constitute a demonstration of the precise linguistic causality. It is possible to imagine a linguistic cause of noun facilitation phenomena which would also be irrelevant to the debate over the lexical representation of derived nominals. For example, one might claim that verbs were difficult for such patients because they caused them confusion over which verbal inflection to use--past or present, third person singular or otherwise, etc.

On the other hand, the results in Bisazza (1980) described above go a step further than Whitaker (1972) and establish that missing link, since they show obligatory arguments to be the cause of the processing difficulty for verbs in cases of noun facilitation. Thus, my psycholinguistic and NL data combine to establish a linguistic cause for noun facilitation, and--more importantly--a linguistic cause which is relevant to the issue involved in the two competing theories of derived nominals (lexicalist and generative semantic), since derived nominals in the generative semantics framework must have the obligatory arguments of their source verb at some stage in their derivation.

3. NL Data as a Source of Questions

The final advantage of NL data for linguistics which I will discuss has to do with questions rather than answers. From studying the language of brain damaged persons and other types of NL data, linguists may discover facts that should properly be accounted for in a theory which seeks to determine the form of the mental representation of language. It might be argued that something like this happened in the late nineteenth century and the first half of the twentieth century.

Roman Jakobson (1973:32) points out that both J. Hughlings Jackson, a nineteenth century British neurologist, and Sigmund Freud in his dissertation, On Aphasia (1953, originally published in German 1871), stressed the need to explain the "close correspondence between functional retrogression [in aphasic breakdown] and the development of the language pattern [in childhood]". Partly to acknowledge Jackson's interest in this correspondence, Jakobson elsewhere (1971:322, "Discussion") states that

> in his emphasis on the verbal aspects of aphasia... Jackson surpassed his contemporaries. I would place him among the precursors of modern linguistics. He launched many ideas which were later developed in the science of language, partly under his influence, partly independently. (emphasis added)

In the 1973 paper, Jakobson also notes the fact that in 1878 Jackson had

> refuted the notion of an immediate transition from words (or morphemes, the smallest grammatical units) to "an articulatory movement, a physical state", describing it as...a "fallacy" which "confuses the real issues" and is "not warrantable in a medical inquiry". (Jakobson 1973: 31; quoting from Jackson 1958:156, originally published 1878)

From Jakobson's discussion preceding and following this passage, it is fairly clear that, correctly or incorrectly, he took Jackson's remarks as more than just a caution not to mix psychological and medical terminology. (He refers to a "linguistic search" for a way out of "this impasse" immediately after quoting Jackson (Jakobson 1973:31).) In addition, it seems that he considered Jackson's statement as being generalizable to the transition from phonemes to sounds in speech and from sounds to phonemes in comprehension, transitions which occupied Jakobson in many of his works, including his Child Language, Aphasia and Phonological Universals (1968, originally published in German 1941!). This work contains a reference to the same paper by Jackson as referred to in the 1973 remarks cited above, showing Jakobson's interest in his work even prior to 1941. Makkai (1972:293) writes that Jakobson's attempts to define phonemes exclusively in terms of features, and his shift from articulatory to (the more mentalistic) acoustic features, date from 1939.

Jakobson (1968 [1941]) is perhaps the first, full-fledged NL text. In this pioneering work, Jakobson assumed a mentalistic approach to phonology based on distinctive features, which could relate the order of phonological acquisition in children to the order of phonological dissolution in conditions resulting from brain damage, as well as clarify the transition from phonemes to sounds and sounds to phonemes. Reviewing a later work on distinctive features by Jakobson and Halle, Fundamentals of Linguistics (1956), Chomsky (1972:346) remarks that

> Although this position is never explicitly stated, it seems clear that in their view there is a very direct... relation between a phonemic representation and the associated sequence of speech segments. That is, speech is taken to be literally constituted of a sequence of phonemes, each with its distinctive and redundant features.

(This "direct" view, of course, later became attenuated in work on distinctive features; a more abstracted view of distinctive features is already proposed by Chomsky in the same article (1972:347-8).)

Re-reading Jakobson's 1941 work--and his remarks (1973:31) on the desirability of the parallel approaches to phonology at the First International Congress of Slavists (Prague, 1929) and to aphasia at the annual meeting of the German Neurological Society (Würzburg, 1929)--one is struck by the possibility that

Jakobson, as one of the main developers of the distinctive feature hypothesis along with Trubetzkoy, did not merely resort to NL data for retrospective confirmation. "The newest developments in aphasic theory suggest that an inquiry into the phonemic character of sound disturbances may be necessary" (Jakobson 1968 [1941]:34). The NL zeitgeist may have been an impetus toward the development of such a theory, and the theory of distinctive features may have been developed partly to explain the correspondence between aphasia and language acquisition facts.

Even if Jackson's writings did not directly spur the development of the distinctive features hypothesis in the first half of the twentieth century, the fact is that his and others' work on aphasia suggested questions in need of a linguistic answer: Namely, by what paths do phonemes become sounds in language production, and sounds phonemes in language comprehension? And how can these transitions be related to the order of the childhood acquisition of phonemes and the dissolution of phonemes in aphasia?

Finally, it is interesting to note that at roughly the same time that Jackson was writing on the neurology of language, A. Melville Bell was publishing his Visible Speech: The Science of Universal Alphabetics (1867). A. Melville Bell was a speech therapist, and his book proposed an analysis of phones into articulatory features which in some ways became the basis of the later, mentalistic distinctive features. This work was expanded in a book by his son Alexander Graham Bell, The Mechanism of Speech (1911). Alexander Graham Bell was also a speech therapist and specialized in teaching speech to the deaf. Halle (1978) provides a short account of these works and their relation to modern phonology.

> ...the way that linguists think about the sounds of speech...derive in part from the work of Alexander Graham Bell and that of his father, A. Melville Bell. (Halle 1978:295)

> ...[A. Melville] Bell attempted to establish...that all sounds of all human languages can be produced, given the very restricted information about a small number of mechanisms that is provided in Visible Speech. Anybody who controls all the mechanisms singly and in combination can produce any speech sound whatever. It is therefore these mechanisms and not the individual sounds of language that are the fundamental building blocks of speech. This insight, which in the last quarter century has become almost a truism among students of language, was stated explicitly in the early 1900's by Alexander Graham Bell... (Halle 1978:298)

What I would like to say in conclusion is that early "neurolinguists" like Jackson proposed important questions for

the science which would come to seek the mental representation of language--and that at roughly the same time the Bells, also motivated by a consideration of medical data, were suggesting the key to the answer.

4. Conclusion--The Other Side of the Coin

So much for this introduction to some of the unique (and not so unique) advantages NL data have to offer the field of linguistics. This story would not be quite complete without noting the following.

First, there are certain problems--logistic, methodological, interpretative--involved with obtaining and using NL data. Some of these problems are unique to NL, some are shared by psycholinguistics. And some are truly daunting. These I will leave to a future paper.

Second, the power of NL evidence is still fairly weak. That is, there are few if any cases where NL data alone would be sufficient to cause us to reject unambiguous, well-motivated formal analyses in linguistics. NL does not have absolute veto power over linguistic hypotheses. To put it another way, for now NL data have the status of a contributing consideration in linguistic debate rather than an absolute one. (But contributing considerations can sometimes be decisive...)

This situation is the result of several factors: the fact that NL is still a relatively new field, the general problem of relating performance data to theories of competence (NL is not the only area of study faced with this one!), etc. These factors will also be discussed in a later paper.

Despite these observations, I do not think there is any cause to be pessimistic about the future of NL. For one thing, the advantages are worth the added practical and theoretical problems compared to psycholinguistic research, simply because if we are serious about studying the "mental representation of language" linguistics must ultimately come to grips with this data. For another, new ways of surmounting these problems are always emerging. And as more and more NL work is accomplished, the methodology of NL will become more substantial, standardized; the ancillary premises needed to relate performance to competence will be fleshed out, etc. As this happens, the power of NL data will become more decisive, leading to advances in both linguistics and the neurology of language.

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