THE PROBLEMATIC NATURE OF NEUROLINGUISTIC DATA

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Neurolinguistics (NL) resembles psycholinguistics in that both use language performance data to confirm or disconfirm formal linguistic theories. The difference is that NL uses such language performance data in conjunction with neuropsychological data. For example, a corpus of speech errors from normal individuals constitutes psycholinguistic evidence; from persons with known brain damage it is NL data. And the fact that there is even a field called "neurolinguistics" suggests that the latter type of evidence offers unique advantages (Bisazza 1985). The main advantage of NL evidence is almost tautological: if linguists are serious about intending to describe the language competence which people really have, NL data will eventually have to be taken into consideration.

However, the key word is "eventually". Psycholinguistic data are a long way from being a sufficient motivation for or against linguistic theories. The same goes double for NL data for reasons both obvious and subtle.

This paper, as a follow-up to Bisazza (1985), will present an overview of these reasons why NL data still have the status of icing on the cake in linguistics. That is, I will try to outline the basic problems that--only for the time being, hopefully-- bedevil NL data and prevent them from being sufficient, or sometimes even contributing, evidence for or against linguistic theories. Throughout this paper, "NL data" will refer to aphasic data, the main type of NL data.

These problems can be broken down into what I will term "methodological" (Section 1. below) and "interpretive" problems (Section 2.). Methodological problems are those which involve all aspects of getting the data, assessing their validity and determining their causality--e.g., why do some brain damaged patients find it easier to read aloud nouns than verbs? Interpretive problems are those which remain even after the methodological problems have been more or less taken care of. These are typically more difficult to solve at the present stage of our knowledge and have as much to do with premises as with conclusions--e.g., does the noun facilitation tendency just referred to reflect a damage to underlying systems of competence (knowledge, memory trace, etc.) or performance (use, access mechanisms, etc.)? (I am not entirely happy with the above terminology and realize that the distinction between the two types of problems includes many gray areas.)

As examples throughout this paper I will stick with noun facilitation in brain damaged persons as much as possible to maintain consistency with Bisazza (1985).

Of course, there are many other problems with obtaining NL data which I can not deal with in this paper. These include both ethical and practical problems and deserve discussion on their own in a separate paper.

1.   Methodological Problems--The Soft Underbelly of NL

1.1. Underdetermined Data

Underdetermined data are those for which no reasonable prima facie cause can be constructed. Such data are, by definition, unsuitable for NL purposes, since it is always dangerous to use performance facts to argue for or against linguistic theories when these facts are themselves not understood. Such incompletely understood data can, however, be extremely tempting to the linguist, be s/he neuro- psycho- or unhyphenated. Obviously, what often happens as soon as one bases a linguistic argument on such data is that Murphy's Law swings into effect, and an explanation for the behavior which makes the data simply irrelevant to the issue under discussion appears in a speech pathology journal.

As likely as this type of hitch may seem, there is more often a significant problem of a different type involved in trying to make use of NL data for linguistic purposes. Section 1.2.1. will deal with difficulty in using NL data due to just the opposite kind of situation--an overabundance of (at least) prima facie causal explanations.

Here, I am assuming that the enterprise of trying to use linguistic ideas to explain aphasic data is a special case of testing linguistic theories and constructs, since to the extent that linguistic ideas can explain any behavior--normal or pathological--they are further confirmed. Both using linguistic theory to explain aphasic data and using aphasia to test linguistic theory involve essentially the same type of problems from the point of view of this paper. In the case of using linguistic theory to explain aphasia, we have to be certain about the contribution of other factors to the behavior under study. In using aphasic data to test linguistic theory, we have to be sure that these data are linguistically constrained--i. e., that other, non-linguistic factors are not the only ones involved.

1.2. The Real Paradox of NL Data

The problem with most NL data is not that they are underdetermined, not that they have no plausible cause. On the contrary, the real catch is that a given aphasic symptom often has many possible causes. (For the moment, I will use the phrase "possible causes" to include both cases where more than one cause can be imagined, but only one really operates, and cases where many causes actually operate. Also, by the term "cause" I will be referring primarily to why one error type rather than another

occurs.) Typically, these possible causes include non-linguistic as well as linguistic possibilities. This potential multiple causality is a problem for neurolinguists who want to use such data as if they were linguistically caused or, at least, constrained. The following section will deal with this problem.

But the plethora of possible causes is not the only serious problem for NL. At the same time, NL data are extremely inconsistent. No two aphasic patients are exactly alike, and a single patient will behave differently from day to day, even from minute to minute. This problem will be the subject of Section 1.2.2.

These two problems with NL data--multiple causality and inconsistency--lead to an apparent paradox, since if different causes give rise to the same linguistic symptom we have little expectation that such a symptom will go away or change. Naively, more cause should have a more consistent effect. Of course, in the case of NL data, this is naive--how, I will try to show below. Still, as rationalists, we expect the paradox to be only apparent when all the facts are understood.

1.2.1. The Overdetermined Nature of Many NL Data

There are two senses in which multiple causes may be imagined for NL data:

(1) A symptom may have several necessary causal factors, none of which is sufficient to produce the symptom by itself.

(2) A symptom may have several possible causes, none of which is necessary, but each of which is sufficient by itself to produce the symptom.

The factors in both of the above cases could be all linguistic or all non-linguistic or mixed.

Case (1) symptoms usually pose no serious problem for neurolinguists--at least when the causal factors involved have been adequately demonstrated. By definition, in this case any linguistic (partial) causal factor would be necessary, i. e., its absence would result in no symptom. Thus, any linguistic arguments based on such a symptom would at least start off on the right foot. Many such cases involve performance modality conditions on whether a linguistic factor will operate to produce a given symptom--for example, some aphasics have trouble naming objects shown to them (production modality) but can understand the words involved when they hear them (comprehension modality).

Case (2) symptoms present neurolinguists with more danger of fruitless speculation. In this case, a given symptom can come from quite different causes on different occasions. Conversely, on a single occasion a given symptom may be produced by a

confluence of two or more, independently sufficient, symptoms. In such cases, the status of any one sufficient causal factor may be in doubt. We usually feel ill at ease in a case (2) situation; theoretically, there is always the possibility that some of the independently sufficient causal factors which appear to be involved could be eliminated or collapsed by more careful study. Minimizing case (2) factors is a highly desirable procedural goal, but in many cases multiple causality is a reality. An example of a case (2) situation follows.

Consider the noun facilitation tendency reported in Marshall and Newcombe (1966), Marshall, Newcombe and Marshall (1970) and elsewhere. Brain damaged persons with this tendency more easily produce nouns in various tasks; they find it easier to read aloud nouns than verbs and sometimes read verbs as related nominals. Many brain damaged persons, including those who show such a noun facilitation tendency, have an easier time producing and understanding concrete words. This is, in fact, probably one of the most common effects across different brain damage syndromes. Thus, it is natural to speculate that concreteness (i. e., the imageability or tangibility of a word's referent) is the causal factor in noun facilitation. This speculation makes intuitive sense; brain damaged persons are disoriented, and, therefore, concrete words and ideas should facilitate their performance by giving them something like a cognitive anchor.

Now, some noun facilitation patients' "nominalizations" (e.g., "conceal" read aloud as "concealment" (Whitaker 1972:67)) seem to result in less concrete words, suggesting that noun facilitation is at least partly determined by some aspect of the category NOUN besides concreteness. However, even the case of less concrete nominalizations might still be squeezed under the concreteness explanation. How can we prove that such paralexias are not the result of a general concrete quality associated with the category NOUN? In other words, how can we rule out the possibility that even non-concrete nouns are facilitated in cases of noun facilitation since nouns as a category are perceived as concrete? Such a speculation, while not very appealing, gains from the fact that non-concrete nouns, such as that just cited above, seem to be in a clear minority.

Thus, the neurolinguist starts off a little dangerously if s/he wants to use these nominalization and other noun facilitation data as a basis for linguistic arguments because of a feeling that a linguistic factor is involved. And yet the noun facilitation symptoms are tempting; a purely linguistic category--NOUN--does seem to be directly implicated...

The situation becomes more complicated if we consider other patients and patient types. For example, in the agrammatism of Broca's aphasia a facilitation for nouns is also observed. Agrammatics often have a preponderance of nouns in their speech, which tends to lack function words and verbs, though I do not think they tend to nominalize verbs. (Recall that Marshall and Newcombe's (1966) patient was a dyslexic with mostly parietal,

and some temporal, lobe damage. Broca's aphasics have damage to the frontal lobe of the dominant hemisphere.) The neurolinguist has to ask whether agrammatic noun facilitation is the "same" as that discussed by Marshall and Newcombe (1966). That is, despite its different neurological etiology and partly different symptoms, could agrammatic noun facilitation reflect the same linguistic constraints as dyslexic noun facilitation?

I am aware of no study explicitly comparing the apparent noun facilitation in agrammatism with that in some dyslexics. Such a study is very necessary but is going to begin with an additional uncertainty: Although Broca's agrammatics often show a facilitation for concrete words, they also can be analyzed as favoring stressed words. The latter observation is the basis of the "stress-saliency" hypothesis of agrammatism (Goodglass 1973), as well as of Mary Kean's more recent "phonological" theory of agrammatism (Kean 1978, etc.). Of course, concrete words are stressed words, but not always vice versa.

Now we must ask whether noun facilitation in agrammatism is related to concreteness, stress or other linguistic/non-linguistic factors, and whether the same holds for dyslexic noun facilitation. Neurolinguists who want to make something out of either type of noun facilitation (as I did in Bisazza (1980)) must first consider these questions.

There is also the possibility of more than one linguistic explanation for noun facilitation (see Bisazza 1985:242). Marshall et al. (1970:417) note that

Linguistic theory provides an embarrassment of variables that may be relevant to reading performance. Words are characterized by one or more category membership symbols, by a structured list of intrinsic syntactic and semantic features, and by selectional restrictions of various kinds.

In Bisazza (1980) I concluded that noun facilitation of the dyslexic type--including the nominalizing tendency--was a symptom with a linguistic cause. That is, I demonstrated that the fact that derived nominals (e. g., "nomination") have fewer obligatory arguments than their associated verbs was the basis of this symptom. (I was unable to treat agrammatic noun facilitation due to unavailability of typical patients.) Thanks to this demonstration, I was able to provide the missing link to Whitaker's (1972) argument against the generative semantic (transformational) treatment of derived nominals by showing that the reason for noun facilitation in the first place was sufficiently related to the issue of his paper (see Bisazza 1985 for summary). I was also able to make some other purely linguistic conclusions based on my demonstration of the cause of dyslexic noun facilitation. However, before reaching this point it was necessary to rule out many potential non-linguistic and linguistic causes for dyslexic noun facilitation. In the end, the Japanese noun-verb stimuli I chose for my experiments were such that they allowed factors like concreteness to be evaluated

along with a test of the effect of optional versus obligatory arguments. Despite all of this I can not say that a factor like concreteness has no effect in dyslexic noun facilitation; I simply demonstrated that concreteness and many other factors are not necessary factors. They may even still be sufficient factors. In any case, the purely linguistic factor 'of number of obligatory arguments is (also?) a sufficient factor.

What is true for this example of noun facilitation is true for many language behavior symptoms which result from brain damage. It is often possible to imagine several, sufficient causal factors for a given symptom. If one of these is a linguistic factor, we have to make sure it is really operative before using it as a piece of linguistic evidence. This determination is often extremely difficult for the following main reasons.

1.2.1.1. Reason 1 for Difficulty in Assessing Linguistic Factors in Aphasia

To begin with, non-linguistic factors like concreteness often have a more assured empirical status than hypothesized linguistic factors in studies of aphasia. That is, cognitively prime factors like concreteness have been recognized and studied in aphasia longer than many, more complex, linguistic constructs. Their effects in language problems due to brain damage are not in serious doubt. Linguistic factors are often both more complex and less apparent, making it all the more difficult to establish their roles in aphasia. Thus, a quality like NOUN may be paradigmatically associated with concrete objects, but its syntactic definition is more complex. The factors which enter into this definition or follow from it (such as optional or obligatory subcategorization features, etc.) are difficult to control and vary systematically in language performance experiments, partly for the following reason.

1.2.1.2. Reason 2

To separate, eliminate, etc. different possible causal factors in aphasia, linguistic test materials are necessary which make possible the systematic variation and control of the many factors potentially involved. And a given language does not always provide an unlimited, endlessly varied set of such test materials. For example, in Bisazza (1980) I wanted to test an English-speaking noun facilitation patient's reading aloud of verbs with a single argument (e. g., "disappear"--X disappears) versus verbs with multiple obligatory arguments (e. g., "put"--X puts Y on Z) relative to nouns. However, in the end, I could not find enough single-argument verbs to make the desired test. English single-argument verbs usually have homophonous (sometimes less common) transitive readings (e. g., "cease"). Others are orthographically category ambiguous (e. g., "hit"--both verb and noun). Still others are just too uncommon to be used in such a

test (e. g., "perish"). This test was, however, possible in Japanese where transitive and intransitive verbs are explicitly marked, and that is the language I did my experiment in, with the results summarized in Bisazza (1980, 1985). But Japanese may not be the answer for other types of problems involving test materials. Obviously, different languages will present different problems in this sense, and Murphy's Law will often assure the maximum possible difficulty!

1.2.1.3. Reason 3

Finally, as will be made clear below, aphasic language behavior is inconsistent, making the clarification of the role of different causal factors all the more difficult. When the data themselves are inconsistent, it is hypothetical linguistic causal factors which become suspect first for the kinds of reasons outlined in Section 1.2.1.1.

1.2.2. The Inconsistency of NL Data

Again, here I am thinking primarily of aphasic data--the main type of NL data. What follows does not necessarily apply to other types of NL data, such as language performance data from normal persons undergoing cerebral blood flow monitoring.

It is true that, in terms of broad characterizations, aphasic language performance is consistent. If it were not, there would be no point to studying it. Thus, for a given patient, there will be things which s/he can never do--e. g., understand a reduced relative clause. I am not sure whether there will be things which a given patient can always do; one of the characteristics of aphasia seems to be that at times even simple tasks are blocked--e. g., a patient without particular word-finding difficulties may not be able to produce his/her name on a given occasion. But this much is, after all, a characteristic of normals as well.

However, within the broad limits of their performance capability, aphasics demonstrate more variation in their language performance than normals. Aphasics do not always perform the same linguistic task in the same way. For example, a dyslexic with a noun facilitation tendency asked to read aloud a verb may respond correctly one time, respond with a nominalization several other times, produce the verb with some phonological distortion another time, make no response another, and so on. And I am not even considering variation from patient to patient.

This inconsistency is a salient feature of the language problems that result from brain damage and should perhaps be viewed in a slightly more positive way. For, in fact, it is in large part a result of the interaction of different systems, modules, etc., which is a characteristic of higher cognitive functions. If an interaction of cognitive modules is necessary

to produce linguistic (or any) behavior in the first place, one can imagine many possible scenarios along the following lines as a basis for the kind of inconsistent behavior just listed. A correct performance, where normally an aphasic error would be expected, could be the result of the intermittent compensation of a spared module. Different types of errors could be the surfacing of effects from different, damaged modules--which effect surfaces at a given time being the result of other complex interactions. No performance, where some is called for, could represent a failure of all contributing modules. And so on.

In fact, if the mind is characterized by modularity, it seems likely that higher cerebral functions are characterized by greater modular interaction. Such "inconsistency" as aphasic language performance demonstrates may be nothing more than a natural symptom of this high degree of modular interaction; we just do not notice this interaction in the case of error-free performance because the medium is usually not the message. Importantly, it also seems to be the case that highly consistent error performance on the part of brain damaged persons is probably the result of a lower-level deficit in terms of the cognitive hierarchy. For example, dysarthric problems result in sound distortions that are more consistent than those of Broca's aphasics (with damage to the anterior cerebral cortex) and are not usually considered "phonological" in nature--the linguistic designation being reserved for the higher cognitive deficit. A dysarthric, such as the type resulting from cerebellar lesions, will more often mispronounce a difficult word in the same way. The most consistent of all errors are produced by damage to the speech organs themselves, at the furthest remove from the cerebral levels associated with linguistic competence.

The first step in finding the consistency we suppose underlies the inconsistency of aphasic performance is to recognize what type of inconsistency we are facing. Some basic types follow.

1.2.2.1. The "Pure" Type of Inconsistency

This is basically variation over time; one minute (literally) it is there, the next, it is not. A patient may nominalize a verb when trying to read it aloud, then later produce it correctly. Or vice versa, or any combination of sequences. Think of it as something like a light with a faulty connection--it can blink on and off, then remain on for a while, flicker rapidly, and so on. And, as with an electric light, fatigue is not always a factor. That is, it is not always the case that a rest will give rise to more correct or more consistent behavior. Attention, too, sometimes plays a role here--often defacilitating performance, as it can do in normals. That is, sometimes the more an aphasic or non-aphasic tries, the more chance there is of an error.

Of course, the general tendency is most important: Is there

a predominant type of error? It is often surprisingly difficult to answer this question, since brain damaged patients can not be tested for long periods, since they are constantly changing due to the process of natural (even if slight) recovery, and since-- unlike a light bulb--a patient's behavior can come out in more than just the three ways mentioned above. These other ways include the following.

1.2.2.2. Red Herrings, Contradictions, Violations of
          Hierarchies and Other Puzzles

     If neurolinguists were only faced with the type of inconsistency just described, there would perhaps be no great problem in identifying real underlying tendencies and causes. A light bulb with a faulty wire is either on, off or flickering. A parallel noun facilitation patient might read a verb correctly, give no response or produce a nominalization of the target verb. But imagine a light which changes color in addition to flickering due to a faulty connection! Unfortunately, but interestingly, this is the type of problem aphasic verbal inconsistency often presents us with.

1.2.2.2.1. Red Herrings

     Suppose we are testing a brain damaged person who shows a noun facilitation tendency in the reading aloud of verbs. Some verbs are read correctly, others have no response, and some are nominalized. So far, no puzzles. But suppose a few items are read aloud with phonological errors, a likely possibility with any type of brain damage. In theory, we want to ignore these if they can not be shown to depend on the lexical category VERB, so we test to make sure that the same numbers and types of phonological errors occur for both nouns and verbs. This much is perhaps only troublesome.

     However, some individual errors will be sure to remain ambiguous, perhaps even some crucial to a linguistic argument. An example of such an ambiguous case would be if a noun facilitation patient responded [ɪntɛnt] for the target "intend". Would this be a case of a devoicing error involving the final consonant, but not changing the lexical category of the target? (Such devoicing errors are quite common in both anterior and posterior aphasias.) Or should it be regarded as a nominalization, perhaps caused by the linguistic factor discussed above (Section 1.2.1.)?

     I am calling such problems of ambiguity potential "red herrings", since on a one-by-one basis they may throw a neurolinguist hunting for a different effect off the scent, resulting in the rejection of a useful example. In the worst case, such red herrings may cast doubt on the effect of a crucial factor in general. Such a worst case might come about, for example, if phonological errors were very numerous and if another

unrelated factor like concreteness were also demonstrably involved, effectively masking a third effect of more theoretical interest.

1.2.2.2.2. Intra-Subject Contradictions

Suppose a brain damaged person shows a noun facilitation tendency to the extent of reading nouns aloud more often correctly than verbs and by nominalizing some verbs. So far, an ideal case for NL speculation. Of course, such a patient need not read all nouns correctly or all verbs incorrectly--the relative statistics being the basis of the noun facilitation attribution. But suppose that this same patient reads a couple of nouns as verbs! These errors would then appear to be "contradictions" to the noun facilitation tendency, which was attributed to the patient on the basis of statistically more errors on verbs. If there were very many such "verbalizations", we might have to declassify the patient in terms of noun facilitation. Even two or three such verbalizations might pose a problem depending on the type of argument about the structure of linguistic competence we wanted to make based on the noun facilitation data. For example, in Bisazza (1980) I wanted to claim that nominalizations are easier for dyslexic noun facilitation patients, because their arguments are optional whereas those of verbs are not. (We can say "Books make nice gifts" but not *"I give" in the sense of "hand over".) However, if a dyslexic noun facilitation patient produces "give" for the target "gift"--even once--my claim is thrown into doubt.

Marshall and Newcombe's (1966:173) patient produced one such contradiction--i. e., one verb in response to a noun stimulus (exact example not given)--out of a total of 50 noun stimuli. The same patient read 18 out of 20 verbs as nouns, showing the relative statistical difference we would expect for a real noun facilitation tendency.

OH, the dyslexic with a noun facilitation tendency discussed in Bisazza (1980), did not produce any such contradictions in my tests, but he could have for all sorts of reasons which would not really have contradicted my explanation of nominalizations in noun facilitation. These include contamination from other test stimuli, disproportional familiarity for the verb of a derivational pair (e. g., the familiarity of "to take" versus that of "a take"), etc. Such contradictions are an inescapable aspect of aphasic verbal performance, but with careful checking and re-testing it is sometimes possible to separate out those performances which reflect one underlying tendency from those which reflect another. The point is that going through this checking is another one of the potential delays in using NL data. Unfortunately, in combination with the other problems discussed in this paper it can sometimes also be decisive. In any case, to do NL we must assume that all such contradictions-- as well as those to be discussed next--are only apparent.

1.2.2.2.3. Inter-Subject Violations of Expected Linguistic
           Hierarchies

Suppose we have an unambiguous noun facilitation patient who
nominalizes verbs when reading them aloud. But imagine we also
have another patient who appears to show the exact opposite
tendency--i. e., facility for verbs and trouble with nouns. The
existence of such a patient would be as problematic for the
linguistic conclusions I wanted to make in Bisazza (1980) as the
apparent contradictions discussed in the previous section; in
both types of contradiction the status of noun facilitation would
be in doubt. Perhaps more so in the case across patients, since
any conclusions about linguistic competence derived from NL data
should apply at least across all speakers of a language, and
perhaps across all human beings if a linguistic universal is
involved. The goal of linguistics is to describe the competence
which all speakers of a language have and those aspects of
competence which are common across languages. My argument
regarding the facility of nouns predicts that no person will find
verbs easier than nouns as long as s/he speaks a language similar
to Japanese and English (and most other languages as far as I
know) in having mostly optional arguments for derived nominals.

But the fact is that a claim for the existence of such a
"verb facilitation" patient has been made. It is this type of
situation that I call an "inter-subject violation of an expected
linguistic hierarchy". Strangely enough, in this case the claim
comes from Whitaker and Whitaker (1976). They claim (1976:269-
70) that "it is not possible to conclude that NOUN is
hierarchically more important or significant than other
categories" on the basis of noun facilitation phenomena due to
brain damage, since--they claim--the selective impairment of
nouns exists as well. But recall that Whitaker (1972) argued
against the generative semantic theory (e. g., Lakoff 1970) which
generates derived nominals from their root verbs, since such
nominals should not be expected to be spared in brain damage
under this theory which regards them as more derivationally
complex than verbs. Whitaker and Whitaker (1976) do not state
whether they view their claim as a change of mind regarding
Whitaker (1972), but it comes close. If they maintained both
their (1976) claim and that of Whitaker (1972), they would be
saying that noun facilitation can be an argument against the
generative semantic transformational treatment of derived
nominals (Whitaker 1972), but that it can not be used for other
arguments! Presumably, they would not want to allow "verb
facilitation"/"noun defacilitation" as an argument for the
generative semantic treatment of derived nominals (see Section
2.1.).   Thus, I think I am right in stating that if "verb
facilitation" existed it would be an embarrassment to both me and
Whitaker (1972).

Here, the problem for me or any lexicalist is to show that
such an inter-subject contradiction is either (A) only apparent;
or (B) the result of an impairment along a different dimension or
hierarchy from that of noun facilitation (a generative

semanticist would presumably want to show this too to rebut Whitaker's (1972) argument!). These two alternatives would be the choices for the neurolinguist in other cases of inter-subject contradictions as well. Of course, doing either (A) or (B) can add tremendously to the work of NL. In Bisazza (1980), I think I was able to convincingly do (A)--at least for the data which form the basis of Whitaker and Whitaker's (1976) claim. That is, I showed that as far as these data are concerned the "verb facilitation" is only apparent. In addition, just in case more convincing "verb facilitation" data could be found, I also had something to say about (B). As examples for dealing with inter-subject contradictions, the details of my arguments follow.

As for (A), consider the following data from Konorski (1967:248), which are the basis of the above quotation from Whitaker and Whitaker (1976) but which they do not quote in full.

(3)   object shown   patient's naming attempt

       mirror   I know this, this is...
       pencil   I know, this is for writing.
       comb   I know this, its name is...
       finger   These are my hands, one hand.
       mouth   This is my...not hand...my body.
       ear   This is my head, my head
                                composed of two parts.

Many of Konorski's patient's responses (i. e., "name", "hand", "body", "head", "parts") in this object naming task are nouns, although not the correct nouns. In fact, the absolute number of verbs in (3)--counting "is...composed" as one--is nine, which is equal to the number of nouns--not counting pronouns, but counting "writing" since it occurs after a preposition. Counting the number of different verbs and nouns, the number of nouns is far greater--even if we count "is" and "are" as different and "composed" as one. Although noun facilitation patients such as that discussed in Marshall and Newcombe (1966) tend not to produce verbs as error responses to verb stimuli, this patient of Konorski's has many noun errors in response to noun stimuli.

Continuing to assess this data as "verb facilitation", the verbs produced by this patient--apparently with correct inflections, although this is not clear since the original data are in Polish and these are not provided (nor could I find a Polish version of the text)--are of the highest level of familiarity. This fact fits Wepman, Bock, Jones and Pelt's (1973:222) finding from a large statistical study that nouns are curtailed in anomia more severely than other parts of speech as a result of a frequency effect; that is, there are many more infrequent nouns in language than verbs. Anomia, a word-finding difficulty resulting from brain damage, presents the same symptoms as Konorski's patient. Note in particular the number of times "I know" and "This is"/"These are" alone re-occur; if it were not for these nearly automatized sequences there would be little basis for a claim of "verb facilitation" here.

Next, the "-ing" forms, which occur both in the data from the above patient and in Konorski's few other examples mentioned in connection with naming difficulties, would presumably be verb-derived nominals in Polish (again, if the English translation is accurate). Such forms in Polish are more clearly nominal in nature than gerundive nominals in English, since they must be inflected with the nominal portmanteau suffix for gender, number and case. We know that the "-ing" forms in Konorski's translation of his Polish data must be this type of verb-derived nominal--rather than, say, the infinitive form of the verb (cf. "I like to swim" and "I like swimming", which have parallel Polish constructions)--since in the type of examples Konorski cites (1967:248-9) <u>only</u> the nominal form would be grammatical. That is, in the "This is for..." frame, verb-derived nominals must be used and inflected for neuter gender, singular number and accusative case. Note that in English *"This is for to write" is also ungrammatical.

From the above, it would appear reasonable to conclude that it is doubtful whether patients like Konorski's represent true cases of "verb facilitation"/"noun defacilitation". If, for example, such patients' problem is with nominal quality per se, why do they frequently resort to a nominal form of the verb or an incorrect noun when trying to name an object?

There is still another consideration which militates against Whitaker and Whitaker's (1976:269-70) statement that "it is not possible to conclude that NOUN is hierarchically more important or significant than other categories" and their claim of equivalent status for noun facilitation and "verb facilitation" in aphasic syndromes. Namely, I know of no finding with <u>normal</u> subjects which could be taken as an analog of the postulated "verb facilitation" which appears, Whitaker and Whitaker claim, as a result of brain damage. Experimental results with normal subjects in tachistoscopic and other tasks point to a generally greater processing ease for nouns than for verbs (Holmes, Marshall and Newcombe 1971; Bisazza 1980; etc.). Children tend to acquire nouns before verbs as well (Bloom 1970). Thus, the hierarchical significance that Whitaker and Whitaker seem hesitant to apply to an explanation of noun facilitation as a result of brain damage appears to be necessary to explain normal processing anyway. All other things being equal, we want to posit the same hierarchies of difficulty in normal performance and in brain damaged persons' performance. The possibility of "verb facilitation" really being a part of any aphasic syndrome is thereby lessened, unless it were to occur as part of possibility (B) described above.

Since in Bisazza (1980) I was able to show that Whitaker and Whitaker's (1976) "verb facilitation" data were only apparent (=alternative A for dealing with inter-subject contradictions), it was not necessary for me to do (B). However, the following points are what I would say along the lines of (B) if more convincing "verb facilitation" data suddenly turned up.

Alternative (B): In Bisazza (1985) I discussed my (1980) demonstration that noun facilitation is most plausibly due to the effect of number of obligatory versus optional arguments associated with lexical items; that is, nouns (including derived ones) are easier than verbs because they tend to have fewer obligatory arguments. This demonstration removed the remaining uncertainty about Whitaker's (1972) arguments against a transformational treatment of derived nominals, since it confirmed that the cause of the kind of noun facilitation he discussed is indeed relevant to the generative semanticist-lexicalist debate. The generative semantic theory claims that a derived nominal starts out as its related verb, and with its obligatory arguments, and thus predicts no greater processing ease for nouns based on optionality of arguments (see Bisazza 1985:236-42). (Recall that Whitaker (1972) had not even speculated on the cause of his noun facilitation data.) Given my argument, it seems unlikely that there would exist another type of patient for whom more obligatory arguments--and greater syntactic complexity--would be easier, since this would be tantamount to different cognitive principles for different human beings. Thus, if there do exist "verb facilitation" cases, they would have to result from an impairment along a different dimension from that involved in noun facilitation--a different dimension which hierarchizes verbs before nouns (in terms of ease of performance)--and which is irrelevant to both noun facilitation and my conclusions from it, if my (1980) argument is correct.

So much for examples of the (A) and (B) alternatives for dealing with the kind of inter-subject contradictions which can arise when we want to make use of aphasic data for NL purposes. Other examples of such contradictions in the NL literature often do not have resolutions even as clear as that described above.

1.3. Back to the Paradox of NL Data

To recapitulate, NL data from aphasia present two seemingly paradoxical features. These are both salient features of aphasia, the main type of NL data, and can be summarized as follows.

(4)   A single error behavior may have different causes,
       at different times or on a single occasion; however,

(5)   many of these error data when taken together appear
       to contradict each other in the various ways just
       outlined.

The bottom line for neurolinguists is that, individually, (4) and (5) both have the net effect of casting into doubt the empirical status of any one cause of an aphasic performance, and of hypothesized linguistic causes in particular. Together, (4) and (5) are bad news indeed. Neurolinguists can rarely be certain that what they are looking at has a linguistic cause or which

linguistic cause. This, in turn, makes it tough to use NL data as an argument for or against any aspect of linguistic theory.

This, then, is the paradox: NL data often lend themselves to various (at least) prima facie explanations (=4); but it is also often difficult to spot with certainty the general error tendency associated with any given causal factor due to apparent inconsistencies among aphasics' errors (=5).

The reasons (4) and (5) come to be salient aspects of NL data require a dissertation on their own, but any such <u>complete</u> explanation is unlikely--that would solve all the problems!

2. Interpretive Problems--The Unguarded Rear Flank of NL

Noam Chomsky (1982:44-5) has often claimed that the way modern, mainstream linguistics does business--as a branch of cognitive psychology--is not fundamentally different from the so-called hard sciences. (I assume that what he has written in this regard has equal application to NL, which I have argued is a bona fide branch of linguistics (Bisazza 1983).) Thus, the process of proceeding from unproven premises to testable hypotheses and so forth is common to both the mental and physical sciences; it is <u>not</u> the case that the cognitive enterprise is any less "realistic" than other sciences.

Nonetheless, a lot of people do not seem disposed to accept this point of view. Of course, intellectual habits and prejudices come into play here. However, it also has to be admitted that the type of evidence used and the number and type of premises which must be made to do cognitive science (including linguistics and NL) do seem to create a superficial difference in kind from the non-cognitive sciences. Chomsky (1980:197) admits this much.

These types of issues--e. g., the necessary premises for cognitive science--come to a head in NL especially, since here it is least possible to say "Let's assume for the sake of argument..." NL is the front line of cognitive linguistics, where it is often difficult to be neutral about pre-theoretical premises. In NL, perhaps more than in the initial formulation of the competence grammar, such premises can have a crucial effect on the interpretation of results.

2.1. Competence and Performance with a Vengeance

The competence-performance distinction, although by now well-worn, still seems capable of generating a variety of opinions and no small amount of confusion. However, the necessity of the distinction to linguistics can be measured by the fact that, despite these confusions, it refuses to go away.

Think of a stereo system; only the record is analogous to

competence, and everything else--static or dynamic--is performance. Of course, we just have to remember the greater complexity of the real competence--especially the fact that it somehow provides for an infinite variety of performances.

In formal linguistics, this distinction helps to define the field of inquiry as those aspects of language knowledge which are outside time, neutral for performance modality, unconstrained by memory limitations, etc. In neurolinguistics, on the other hand, we attempt to test the theories of linguistic competence thus defined using, principally, language performance data from brain damaged persons. Here it is difficult to ignore the issue of competence and performance, since the obvious question is whether competence itself is impaired as a result of brain damage. In fact, the competence-performance distinction itself can be used to reject the relevance of disconfirming NL data. Thus, if a given set of NL data seems to contradict a competence theory, a disgruntled linguist merely has to claim that the NL data reflect some aspect of the performance system and not competence structure. This can also happen with purely formal data which appear to contradict a cherished theory, but it is perhaps natural to expect the neurolinguist to have something more concrete to say on the matter.

A hypothetical example of the above competence-performance catch applied to NL data was mentioned in Bisazza (1985:241-2). Recall Whitaker's (1972) argument against the generative semantic theory which derives nominals like "nomination" from verbs like "nominate". He argued that, since there exist brain damaged persons who can produce the former but not the latter, the generative semantic theory is implausible, because it would entail that brain damage spared the derivationally more complex form. However, Whitaker does not demonstrate the cause of his data. Therefore, as I pointed out in my 1985 paper, a generative semanticist could claim that these facts reflect only some aspect of damaged performance, and that, in fact, such patients' tendency to nominalize verbs when asked to read them aloud shows that they have intact derivational rules relating such verbs and nominals--thereby actually confirming the generative semantic theory of competence in its treatment of derived nominals.

The only way to resolve such opposed views of the same NL data is to make a complete determination of their cause(s), as discussed in Bisazza (1985), because there is still no reliable way to determine whether competence or performance, or both, is impaired in brain damage. This remains a major goal for NL.

Some serious NL thought (e. g., Weigl and Bierwisch 1973) leans toward viewing competence as undamaged in most cases of aphasia for reasons like spontaneous recovery, spared linguistic ability in certain modalities, etc. But even if competence is never damaged in aphasia, we are left with a similar problem: Namely, we must still decide what aspects of damaged performance systems mirror the competence system, and which do not. Weigl and Bierwisch (1973:15) note that many aspects of the damaged

performance systems in aphasia are describable in terms of the competence system, making it possible to hold their view that brain damage spares competence and still do NL. Again, it is crucial that NL develop criteria for deciding when brain damage symptoms mirror competence structure and when they do not.

A key point always of importance in speech pathology also comes into play here: the question of whether aphasic symptoms represent a loss or simply a damage of function. When the term "loss" is used in the speech pathology literature, I have the impression that it is often the loss of an underlying knowledge/competence which is meant. Conversely, when "damage" is used it seems to refer to damage to mechanisms for accessing and implementing language knowledge--damage to performance mechanisms, in other words. And the term "damaged" (or, synonymously, "impaired") seems to be in more common use than "loss".

Of course, "loss" and "damage" need not parallel competence and performance, respectively; both could occur to either competence or performance mechanisms. But the status of a certain area of competence for which the appropriate performance mechanisms have been lost is not so obvious; hence, there is a tendency to doubt its survival (see Chomsky 1980:50-1).

The problem for neurolinguists in this is to avoid the general tendency to assume that a total loss of a certain area of language behavior necessarily entails a damage to competence substructures. Even in such cases of total loss there is still the possibility that competence itself is unaffected, as Chomsky (1980:50-1) points out. Unfortunately, if this possibility exists, then there is also the possibility that any damage to performance mechanisms which we can observe does not reflect anything about the structure of competence.

The conclusion seems to be that the neurolinguist, like the formal linguist, always has to be on the lookout for principled ways of distinguishing competence from performance data. Even more so than the formal linguist--everyone would like the buck to stop somewhere!

3.   Conclusion--Still Some Hope

This paper might seem very pessimistic, though it was not my intention to scare linguists off NL data. I simply have left qualifying points for other papers. The advantages of NL data were discussed in Bisazza (1985). Other qualifying points could also be mentioned.

For example, a paper could be written about the dangers of relying exclusively on formal data to construct a competence model. Such works have been written, tending to be reactionary criticisms of Chomskyan linguistics (e. g., Moore and Carling 1982). But there is also a more neutral sense in which such an

assessment could be written, and Chomsky himself (e.g., 1980:197) admits the problems involved with relying only on formal data. It is not necessarily the case that it would be safer, or even as safe, to do linguistics without NL data.

Another qualifying point for another paper is that ways are constantly developing to deal with the problems discussed in this paper. To mention four:

First, some of these techniques are coming just from the fact that neurolinguists are gaining experience. For example, using cross-linguistic aphasic data is helping clarify some issues, like noun facilitation. Other cross-linguistic data may be similarly useful in other issues.

Second, other techniques are coming from medicine and speech pathology. Increasingly sophisticated accounts of aphasia from the clinical point of view can only benefit the field of NL. Partly by showing what is not linguistic in origin, but also in other ways.

Third, the ongoing pycholinguistic study of normal language performance--including language acquisition--will benefit NL by providing a context for the interpretation of aphasic symptoms. An example was given in Section 1.2.2.2.3. of such a case involving apparent "verb facilitation".

Fourth, even the notorious competence-performance problem shows some signs of mutating from a pre-theoretical issue to a research question. With a greater understanding of the biochemical basis of memory, it should become more and more possible to speak of a distinction between damage to competence and to performance mechanisms in concrete terms, though probably no one should hold their breath!

In short, there is no reason to be overly pessimistic, or to label NL as irredeemably speculative. To do that would entail rejecting the possibility of a cognitive linguistics.

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