

Panel: Aphasia With and Without Adjectives
Babies, Bathwater, and Monkeys

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In a spirit of cooperative effort, I'd like to begin with a few quotes whose authors you might try to identify:

1. "Correlational analysis of their (aphasic) test scores will yield high intercorrelations among all language performances and factor analysis will yield an overwhelming first factor produced by degrees of severity." This is not Hildred Schuell or Aaron Smith speaking, but rather Goodglass and Kaplan (1972, p. 15).

2. "Many dimensions of impairment resulting from brain damage are identifiable, and need to be studied, in addition to the common or general dimension of language deficit." This is not Goodglass, Helm-Estabrooks, or Howard Gardner speaking, but rather Schuell and Jenkins (1961, p. 299).

3. Finally, in the immortal and prophetic words of George Carlin (1973), "Your name can be spelled S-M-I-T-H and you can pronounce it 'Jamofsky' if you want to."

These quotes (ignoring the third) weren't intended to argue that Goodglass and Kaplan don't believe that types of aphasia exist, or that Hildred Schuell was a closet follower of Norman Geschwind. They do suggest, however, that both sides of the aphasia with and without adjectives issue often argue their points while recognizing legitimate points made by the other, and that many--although certainly not all--of their differences are related to different goals, different methods of evaluation, and different samples of patients who command or demand our interest or attention (Duffy and Ulrich, 1976). With these things in mind, I'd like to examine some practical methodological issues that bear on the validity of a with-adjectives approach to the study of aphasia.

At the core of the aphasia with or without adjectives issue is whether aphasia varies only along a severity continuum, or whether there are separate and distinct types of aphasia which reflect breakdowns of different aspects of language as a function of lesion locus and how language is organized in the brain. As far as I can tell, nobody denies that variations in severity play a very strong role in producing variations in performance across a wide variety of language measures. If we accept this potent role of severity in explaining variations in performance among patients, then the shortcomings of those using a no adjectives approach center around errors of omission--or failure to recognize or look for variations in aphasic performance not due to severity. Somehow or other, errors of omission are hard to criticize without proving the theoretical validity or clinical utility of that which has been ignored. The burden of proof thus shifts to those attempting to prove and describe the existence and usefulness of aphasia typologies, and it's much easier to focus criticism on the tangible aspects of those attempts. The fixing of a label with an adjective is a sitting duck for general criticisms like "when knowledge is lacking, a name comes to take its place." And it's not until we get on the road toward trying to understand a problem that someone can say, "Going the wrong way? Don't worry, you're making great time."

For the next several minutes I'd like to ignore theoretical issues and focus on some relevant facts that describe how we've been approaching the

study of aphasia as a function of severity and type. The data base for this comes from a review of 59 articles from five sources; the 1982 C.A.C. Proceedings, the 1982 issues of the Journal of Speech and Hearing Disorders, the 1982 issues of the Journal of Speech and Hearing Research, volumes 15-16 of the 1982 Brain and Language, and the 1981 issues of Neuropsychologia (1982 was unavailable to me). The 59 articles represent all data-based studies whose primary focus was on patients with aphasia. Abstracts, studies of nonaphasic problems or of the spouses or families of patients were excluded, as were studies of non-English speaking patients, unless familiar standardized measures were used. Seventy-one percent of the studies were primarily descriptive or theoretical in nature, 20% were treatment studies, and 8% emphasized localization issues. I think reviewing some characteristics of these studies can help focus on what we've been doing with and without adjectives. They also identify what seem to be some important shortcomings of studies that seek to identify differences among patients as a function of aphasia type.

First, how much attention are we paying to aphasia severity and aphasia type in our studies? Sixty-nine percent of the studies reviewed described severity and 63% described type. Of those describing severity or type, only 38% described both, suggesting that in many cases investigators are interested in only one or the other factor. Thirty-one percent described severity but not type, and slightly less (22%) described only type. Only 8% described neither type nor severity; this is about 8% higher than it should be.

An important issue relative to reliability and replicability of research relates to how well we describe severity and type. There shouldn't be many excuses for not using a standard exam or rating scale these days--we have several of them. In fact, operationalizing the definition of types of aphasia by tests like the Boston Diagnostic Aphasia Examination (Goodglass and Kaplan, 1972) and the Western Aphasia Battery (Kertesz, 1982) provides us with the mechanism for at least understanding--if not agreeing with--the quantitative meaning of terms like Broca's and Wernicke's aphasia.

We find that 85% of the articles that described severity did so with a formal rating scale or standard test -- 15% simply stated severity and left it at that. Only 61% of studies describing type indicated that a formal scale or test was used to determine type. This leaves us with 39% in which we can't be exactly sure who's been studied as far as type is concerned, unless we're personally acquainted with the authors or the patients themselves. One has only to peruse the range of texts crossing the disciplines of speech and language pathology, psychology, linguistics, and medicine to learn that words used to describe types of aphasia have multiple meanings, some of which are more acceptable than others. Take, for example, the concept of Broca's aphasia as involving intact comprehension with impaired speech. Some have claimed that this definition is a straw man set up by those with a unitary view of aphasia, and that nobody ever really believed that Broca's patients have normal verbal comprehension. A glance at definitions of Broca's aphasia in many current medical texts, however, suggests that this definition of Broca's aphasia must be one of those myths that never happened but always are. It's also interesting to note the number of psycholinguistic studies in recent years that have examined Broca's aphasia and suddenly "discovered" the existence of comprehension deficits. If we're talking about straw men, it's clear that the straw can be cut both ways.

At any rate, it seems apparent that studies describing type of aphasia will go a long way toward improving their credibility, replicability, and basic contribution to knowledge if they consistently apply some operational criteria

for identification of types. The means for doing it are available and they should be taken advantage of.

Having established that a fair number of studies describe severity and type of aphasia, we can ask how many show an interest in actually examining the effect of severity or type on dependent variable performance. Of the 59 articles, 25% examined the effect of severity and 31% examined the effect of type of aphasia on dependent variable performance. These figures may seem low, but studies in which no such comparisons were possible are included here--for example, single case studies.

When severity was described, 37% of the time its effect on performance was examined, and when type was described 49% of the time its effect was examined. This suggests that more than half of the studies used descriptions of severity and type simply as a vehicle for describing the characteristics of patient samples and not because of an interest in their influence on dependent variable performance.

Of special interest are the number of studies in which severity or type was found to influence the dependent variable(s). Severity was found to affect performance in 80% of the studies in which the effect of severity was examined. This is not surprising in light of the known potent effects of severity on performance on many language tasks.

What is somewhat surprising is that type of aphasia was found to affect performance in 78% of the studies in which effect of type was examined. This is surprising because of the controversy surrounding typologies and the inconsistent or unknown criteria used for establishing type.

Why is this figure so high? First, it could be because the effects of type are real and relatively pervasive. Or, they could result from an artifact of subject selection, experimental design, or data analysis. Given the strong effect of severity on language task performance, it would seem very important to control for the influence of severity when examining the effect of aphasia type on the dependent variable(s). For example, if we compare the performance of a group of Broca's and Wernicke's patients (however they might be defined) on a measure designed to assess comprehension of reversible sentences, and we do this in an effort to discover if breakdowns in comprehension are different in Broca's and Wernicke's aphasia, it seems that we'd want to be sure that any differences we find aren't attributable to something other than aphasia type (namely, severity), or that failure to find differences weren't due to a masking effect of severity. Severity in this case could be controlled by matching Broca's and Wernicke's subjects on the basis of some index of overall aphasia severity, or by ensuring that the two groups do not differ significantly as a function of overall severity. Another control could be statistical; that is, when groups do differ in severity, those differences could be controlled for by partitioning out the severity factor in analyses of covariance or partial correlations.

Returning to the survey, we found that the effect of severity was controlled for in only 39% of those studies in which the effects of type were examined. Even more revealing is that when differences as a function of type were found, 79% of the time severity had not been controlled for. This means that in a large percentage of studies that find type of aphasia to affect performance on some measure, that effect might be attributable to differences in severity between the groups which were compared. This is a significant shortcoming of some (perhaps many) of the studies that seek to improve our understanding of aphasia by comparing performance as a function of aphasia type. However, it is one that can be remedied through careful subject selection or description and/or in data analysis. It should also be recognized that

controlling for severity doesn't mean differences as a function of type will never be found. For example, differences among types of aphasia in this survey were found 43% of the time when severity was controlled. Also, it is possible that failure to find differences among types also may be due to differences in severity, and that controlling for severity may unmask differences between types that otherwise would have gone undetected.

One comment regarding a different and rarely used approach to grouping patients. It's very possible that more can be gained by grouping patients post hoc rather than a priori. This notion of post hoc grouping in place of reliance on categorizing by traditional typologies was explained quite well by Caramazza, Berndt, and Brownell (1982). They said: "Although grouping by clinical profiles can be extremely useful, it also can be misleading. That is, the theoretical basis for the clinical classification is unlikely to be sufficiently detailed to allow the fine differentiation in performance typically sought in experimental investigations. Thus, indiscriminately averaging patients' performance in clinically determined groups can result in severe distortions of individuals' abilities. A reasonable alternative is to examine patients' performance on the experimental tasks to determine the extent of individual differences and to base subgroupings on experimental data. This way, emerging subgroups reflect the extent to which the task taps processes and structures that may be differentially available to the patient group tested" (p. 172). This approach to grouping is one that might be profitably followed by those who do and don't use adjectives with aphasia. It recognizes that typologies are just lists of parts and that from that we need to move toward an understanding of process.

To conclude, Carl Sagan (1977) has described science as paranoid thinking applied to nature, in which we look for natural conspiracies and connections among apparently disparate data. Perhaps those without adjectives for aphasia do too much denying of the conspiracy, and those with adjectives point fingers too easily or to too many places where they don't belong. For an awfully long time this whole with and without adjectives issue has been a monkey on the back of our attempts to come to grips with a clinically meaningful and theoretically sound understanding of aphasia. It seems that part of that monkey is an approach to studying aphasia that too often has neglected objective indices of performance and ignored adequate experimental controls. We shouldn't be ready to throw the baby out with the bathwater, but it'd sure be nice to do some things that'll help us get rid of that monkey.

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