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The Critical Role of Group Studies in Neuropsychology: Comprehension Regularities in Broca's Aphasia[1]

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We reexamine the empirical record of the comprehension abilities of Broca's aphasic patients. We establish clear, commonly accepted, selection criteria and obtain a pool of results. We then subject these results to a detailed statistical analysis and show that these patients comprehend certain canonical sentences (actives, subject relatives, and clefts with agentive predicates) at above-chance levels, whereas comprehension of sentences that contain deviations from canonicity (passives, object-gap relatives, and clefts) is distinct and is at chance. That the latter is the case, and patients indeed guess at such structures, we show by comparing the distribution of individual results in passive comprehension to that of a model for such guessing—an analogous series of tosses of an unbiased coin. The two distributions are virtually identical. We conclude that the group's performance is stable, and well-delineated, despite intersubject variation whose source is now identified. This means that certain comprehension tests may not always be used for the diagnosis of individual patients, but they do characterize the group. It also means that group studies

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are not just a valid option in neuropsychology; they are a must, since demonstrations like ours indiciate very clearly that single-case studies may be misleading. As we show, the findings from any one patient, without the context of a group, may give a distorted picture of the pathological reality. Our conclusions thus promote studies of groups of brain-damaged patients as a central tool for the investigation of brain/behavior relations.

AN APPARENT CONTRADICTION

Repeated pleas for more precision in the examination of neuropsychological data have led to a split in the field. There are these who, following a long tradition of scientific practice, believe that clinically formed groups of patients are the right means for studying brain/behavior relations (cf., for instance, Zurif, Swinney, & Fodor, 1991); and there are those who, believing that functional lesions cannot be inferred on the basis of clinical category, commend single-case studies as the only valid method to investigate behavior subsequent to brain damage (Caramazza, 1986, and many related papers). Broca's aphasia has been at the heart of the attack on group studies. This syndrome has been argued to be epiphenomenal. The claim is that linguistic behavior, once examined at a sufficient level of detail, reveals vast inter-patient variation that defies generalization or inference to a theory.

This claim has already been challenged (e.g., Zurif, Gardner, & Brownell, 1991; Zurif et al., 1991; Grodzinsky, 1991). Here, we focus on one of its aspects and show that group studies are not just a legitimate strategy; they are a must. We discuss the debate on the interpretation of comprehension scores of agrammatic Broca's aphasics (and the ensuing diagnostic and methodological conclusions). At the heart of this debate lie scores obtained for the comprehension of the passive construction and the relation they maintain with those obtained for active sentences. For some reason, the active/passive distinction has captured the imagination of many, leading to a large number of experiments that test the comprehension of these structures by Broca's aphasics. We are thus fortunate to have a relatively broad database that gives a more reliable picture of the state of affairs than anywhere else in aphasia. There are two contrary claims concerning the data, in line with the two positions above. It has been claimed that the comprehension of actives and passives in agrammatic Broca's aphasia varies randomly across patients; at the same time, it has been argued that despite certain individual variation, a uniform pattern emerges: the patients perform at above-chance level for active sentences and at chance for passives.

Both views are data driven. In a field that purports to be governed by rational considerations, this situation is intolerable and must be corrected somehow. At the very least, one of the two interpretations of the data is false.

At issue is the relevance of traditional diagnostic categories to the study of brain–language relations and thus the value of group analyses. The resolution of this matter is crucial. Both Broca's and Wernicke's aphasia have lesion localizing value (with a grain size at least equal to, if not better than, that provided by fMRI data). Therefore, discovering syndrome-specific structural patterns will lead to significant generalizations concerning the neurological underpinnings of linguistic capacity. Indeed, we assume that this payoff is what impels the great amount of attention devoted to comprehension—and the active-passive contrast—in Broca's aphasia. However, if there is no reliable comprehension pattern for Broca's aphasia, there is no particular reason to continue to focus on this syndrome.

We take this last possibility very seriously. Accordingly, we examine here all the relevant data, published between 1980 and 1996, of which we are aware. And to forecast the outcome of our examination, the data will strongly support a syndrome-based approach. That is, although we observe some variation in the patients' comprehension, this variation is statistically well behaved, forming a clear and explicable pattern. Moreover, our analysis indicates that single-case studies are highly misleading in certain circumstances. In what follows, we go through the steps leading to these conclusions.

EXPERIMENTAL OUTCOMES AND UNBIASED COIN-TOSS DISTRIBUTIONS

We begin by reflecting on possible outcomes of tests with binary-choice designs which have multiple tokens or trials (as do most syntactic comprehension tests in aphasia, in which a subject is asked to associate two semantic roles with two sentential positions—a binary choice). Even without information about the content of the tests (with *n* tokens, each with two possible responses, call them Nos. 1 and 2), we can easily determine that these tests allow exactly three possible outcomes: for any one subject, either response 1 is dominant in the scores or response 2 is dominant or neither is dominant; that is, both 1 and 2 responses are equally present in the subject's scores (give or take a bit).

We can thus distinguish two types of numerical patterns in the responses: (1) the ''sure'' response, in which subjects confidently and consistently make a choice, i.e, their performance differs from chance (whether their response is correct or incorrect from the experimenter's point of view is another matter);[2] and (2) the ''chance'' response, where subjects have no knowledge (or opinion) regarding the norm, hence no indication as to how to act and choose. Scores will reflect this lack of knowledge; i.e., subjects will vacillate and respond at random by guessing. In the absence of knowledge that will dictate No. 1 or No. 2 choice, they will, effectively, flip an unbiased coin before

[2] What matters to us is whether the subject responded in a manner different from chance. As a matter of fact, this response can be either above chance (''correct'' from the point of view of the accepted norm) or below chance (''incorrect'' or systematically reversed). For the present discussion, this distinction will be suppressed. We are effectively dealing only with performances that are above chance or normatively correct.

each response and follow its dictum (e.g., ‘‘heads’’ ⇒ response 1; ‘‘tails’’ ⇒ response 2).

These considerations force an interpretation of the results only in the context of groups of subjects. In the case of sure responses, having many subjects merely strengthens our belief in the reliability of our data, as we expect the majority to approach 100% sure responses anyway. The situation differs, however, in the case of ‘‘chance’’ responses. Here, each subject can be thought of as flipping a coin and using it as a guide for responding. Does that mean that we will get response 1 half the time and response 2 for the other half? If subjects consult an unbiased coin before responding, this will indeed be the case. For a large enough group of m subjects, and given a large enough n, the number of 1s and 2s evens out and, thus, of the total number of responses ($m \times n$) there will be about 50% 1s and 50% 2s. Yet does that mean that *each* subject (with a relatively small number of trials n) produces exactly $n/2$ No. 1 responses and $n/2$ No. 2s? The short answer is no. Such a result would only be obtained if we could guarantee a constant value for any (short) series of coin tosses; yet it is well known that a series of coin tosses of an unbiased coin distributes binomially around the mean. Throw a coin 10 times and you will get some ratio between heads and tails; throw it another 10 times and you will get, most likely, some other ratio. Over time, heads and tails will even out; yet the sets of 10 throws will have a binomial distribution around 5 heads and 5 tails. So, guessing behavior, which results in chance performance, cannot, and should not, be 50% correct per subject. Rather, it should be binomially distributed around the mean of 50% correct level. We can now see why results from multiple subjects are so important in this context: in such a response type, each subject flips a coin and uses it for responding to each experimental question. A single subject, then, cannot be used to discern the pattern, if there are experimental conditions that might result in chance performance. This is so because the score of this particular subject may be located anywhere on a binomial curve. We must examine the group in order to discover a pattern.

This is what the foregoing discussion leads to: for patients that are selected on independent grounds, we expect above-chance performance on active sentences; that is, the central tendency of the distribution of responses on this structure should be located close to 100%. We further expect a binomial distribution for the passive responses, with μ around the 50% mark. Finally, we expect the difference between the two results to be statistically reliable.

In the remainder of this paper we examine the empirical record along these lines. The critical role of an independent diagnosis leads us to begin with a brief discussion of diagnostic principles that must be applied for the definition of the group. We then review the experimental record and show that the circumstance we have just described is correct: the comprehension performance of Broca’s agrammatic aphasics on active sentences is around 100% correct, whereas their performance on (agentive) passives produces a binomial distribution (or rather, a variant thereof, that fits our specific situation), with a mean around 50% correct, that is, moreover, reliably different from the distribution of the actives. We further present an analysis along the same lines of all the available studies on comprehension of subject- and object-gap relative clauses and clefts, comparisons that are claimed to bear on the same linguistic distinction as that between active and passive constructions. And the outcome is the same. This set of findings indicates that the comprehension of Broca's agrammatic aphasics is well behaved and follows a model of comprehension according to which actives and subject-gap relatives and clefts are properly comprehended by the patients, whereas passives and object-gap relatives and clefts are guessed at. Finally, we draw some conclusions regarding the relevance of these data for the diagnosis of Broca's aphasia.

EMPIRICAL RECORD

Subject Selection and Diagnosis

As a first point, we again note that the claim of there being no discernible comprehension pattern for either actives or passives has long been associated with the so-called ''single-subject-only'' position (e.g., Caramazza, 1986; Badecker & Caramazza, 1985), according to which group studies are illicit in neuropsychology. This claim has most recently been resurrected in a review of empirical studies titled ''Comprehension of Reversible Sentences in ''Agrammatism'': A Meta-analysis'' (Berndt, Mitchum, & Haedinges, 1996).[3] Of particular relevance for the present discussion is the fact that of the 64 cases which Berndt et al. reviewed (culled from 15 studies reported in the literature between 1980 and 1993), only 36% had a pattern where active sentences were performed better than chance level and passive sentences were understood at a level no different from chance. Of the remaining 64%, about 30% of the patients performed above chance in both conditions whereas 34% exhibited chance performance also in both conditions (Berndt

[3] Berndt et al.'s title notwithstanding, it is a misnomer to call this review a ''meta-analysis.'' A meta-analysis is defined as a compilation of studies (be them published, unpublished, or both) intended to corroborate the evidence behind specific theoretical claims utilizing inferential statistical analyses on the aggregate data. The main purpose of a meta-analysis is thus to allow evidence to count which otherwise would be considered too unreliable (due to a low number of subjects or other such methodological limitation). In Berndt et al.'s review, however, no statistical analyses were carried out beyond binomial tests on each patient's performance per condition and calculation of groupings of patterns. Furthermore, and contrary to normal practice in the elaboration of meta-analyses, all the studies reviewed were inappropriately given the same weight. This ignored the fact that the sample size (of sentences tested) differed widely across studies. These factors make the work presented by Berndt et al. not a meta-analysis, but an incomplete review.

et al., 1996, p. 295). This apparent randomness in performance is taken to support the single-case only claim.

This presentation of the data, however, is misleading. Findings such as Berndt et al.'s are, in general, a reflection of the application of the wrong criteria for patient selection. Specifically, data lose clarity when there is a failure to circumscribe subject selection to patients with agrammatism associated only with Broca's aphasia, that is, only with the short, telegraphic, and syntactically simplified and incomplete utterances that figure importantly in the classic Broca's profile. And, this stricture is important: after all, it is the localizing value of Broca's aphasia (and the manner by which it contrasts with Wernicke's) that connects the analysis of aphasic syndromes to neuroanatomy and establishes the different functional commitments of different brain regions.

This issue of patient selection, along with the statistical considerations presented in the introduction, led us then to the following criteria for inclusion, which we applied to the available data that was published between 1980 and 1996:

(a) Subjects must be Broca's aphasics.[4] Therefore, it is necessary that each patient tested be classified in terms of the classical taxonomic lines yielded in standardized aphasia batteries and that their lesion sites, if known, be compatible with the neuroanatomical characterization of this syndrome.

(b) Each data entry must be independent. Whenever a subject contributes more than one measurement for the same condition, an averaged performance must be entered.[5]

Actives and Passives in Broca's Aphasia

In Fig. 1 below, we pooled the results separately for actives and passives, using the ''*Mathematica*'' computer program. We drew the histogram of the recorded performance levels (bin size, 10%). That is to say, we looked at

[4] Incidentally, we note that in Berndt et al.'s (1996) review, of the 42 subjects considered, only 21 were reported to be Broca's aphasic (as classified by the BDAE or some other standardized test battery). This we take to be the main source of the wide heterogeneity in patterns that emerged in their findings.

[5] This point is straightforward and very important. In fact, one serious methodological flaw in Berndt et al.'s analysis was that each instance of a performance of a subject reported was entered as an independent case, whether or not that subject had contributed more than one measurement for the same condition. Thus, if subject FM, for instance, participated in four different studies of active and passive sentences Berndt et al. counted those data as four different measurements, in fact, as four independent data points. This oversight had the direct consequence that in several instances throughout their review, Berndt et al. counted the same subject more than once. Berndt et al. acknowledge that there was multiplicity of measurements per subject but fail to recognize the problem it presented for the validity of their conclusions. The consequence of this mistake was to inflate the database in ways that made their results difficult to interpret. This we believe is another source for the heterogeneity revealed in their findings.

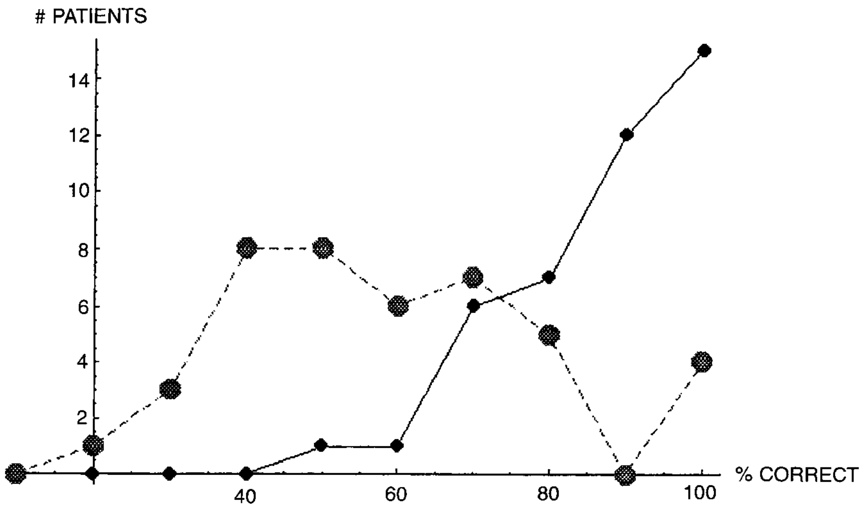

FIG. 1. Number of patients vs. performance level in actives (full line) and passives (dashed); scores are for 42 patients (6–48 trials each).

the clustering of subjects around each level of performance, for both the active and the passive constructions. The figure thus shows the number of patients in each performance level.

We further analyzed our results numerically. Measures of central tendency for both the active and the passive condition distributions show that: (a) the active condition is negatively skewed and clusters around a performance level of 86% correct (mean = 83.14, median = 85.5, and mode = 100); and (b) the passive condition, by contrast, clearly clusters around the 50% mark (mean = 55.3, median = 55, and mode = 50). (In both conditions the number of sentences per patient varied between 6 and 48.) Moreover, χ^2 tests on the active and passive conditions show that whereas the average performance for active sentences is significantly different from chance ($\chi^2(1,n = 100) = 43.56$, $p < .0000$), the average performance for passives is not ($\chi^2(1,n = 100) = 1.59$, $p > .05$) (see Appendix 1 for detailed data on each subject's performance on each condition). A repeated-measures *t* test was performed, to reveal a difference between the active and passive conditions ($t_{(41)} = 9.54$, $p < .001$). Actives, then, are well above chance and reliably different from the passives, which are at chance levels.

Notice that the criteria for patient selection are independent of the interpretation that can be given to these patterns. It could well have been the case that implementation of the right criteria for inclusion would have yielded total heterogeneity across subjects. Therefore, the clear active–passive distinction increases our expectations as to the potential of this kind of evidence to inform theories of the normal case—in the sense, that is, that *stable* breakdown patterns are required if they are to bear upon theories of normal language organization.

As a next step, we sought to characterize the distribution of the passive scores, in order to evaluate the claim that the performance levels of the patients are analogous to flipping a coin. This leads to a model of a series of unbiased coin tosses, namely, a model for a group of m ''subjects,'' each having n ''trials'' with $p = .5$ chance for success. The resulting distribution of the number of successes per subject should thus behave like a binomial distribution $B(m,n)$. However, in our particular case there is a slight complication: in the actual data, the number of trials used in different studies is unequal, varying from 6 to 48 test tokens. As a consequence, the data are presented in terms of *percentage of success* for each patient. In such a case the binomial model does not apply strictly speaking. To get around this problem, we simulated this process, rather than devising an analytical formula for such a variation on the binomial function. We built a computer simulation (again using ''*Mathematica*'') in which an unbiased coin was tossed repeatedly for each of our 42 subjects, the length of the runs being equal to the diverse number of trials in the tests (between 6 and 48 times). To be faithful to the actual data, we further computed for each subject the percentage (rather than number) of successes. Finally, we pooled the results of many such simulations in order to get a good approximation of the overall distribution of the levels of success in such a situation.

The similarity between the data and the simulation is striking (Fig. 2). They are both symmetric with a mean around 50%, and they are unimodal. Most crucially, they are both open to an almost identical extent, that is, the range of possible performances is as broad in both graphs.

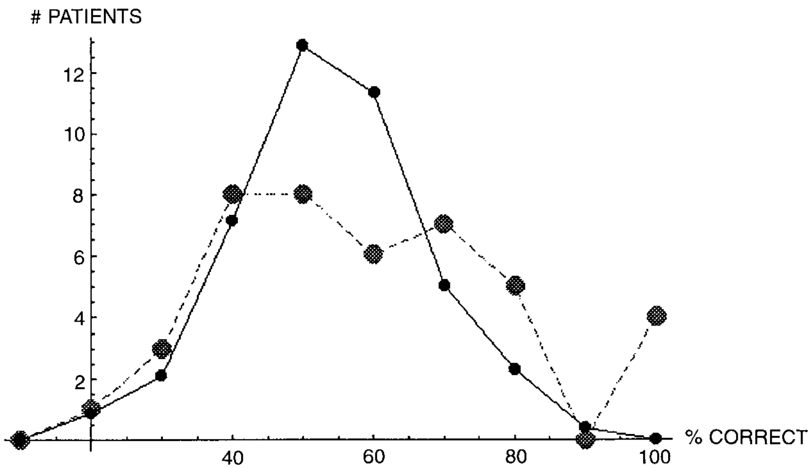

FIG. 2. A comparison between the passive data (dashed) and the simulation (full line).

Concluding, then, chance performance is equivalent to flipping a coin; a model of the distribution of coin tosses (corrected as our particular case requires) is similar to the actual data from passive; individual variation, thus, is a reflection of this distribution. Broca's aphasics, we can safely conclude, perform at chance levels on comprehension tasks of the passive construction.

Subject and Object Relatives and Clefts in Broca's Aphasia

Having established the overall reliability of the active–passive contrast in Broca's aphasia, it seems reasonable to pursue a linguistic path in our evaluation of patterns of sparing and loss connected to this syndrome. One important linguistic factor in the active–passive contrast is that the passive, but not the active, involves deviation from canonical order. As it happens, there are other widely investigated contrasts that also feature this difference, namely, subject vs. object relatives and subject vs. object clefts. The choice of this contrast is not accidental: it is analogous in many ways to the active/passive contrast. These pairs of constructions may, therefore, form a natural class within linguistic theory. A result for relative clauses that is analogous to the one obtained above, will thus be of great value for the proper description of the deficit in Broca's aphasia. Figure 3 reveals that performance in the subject condition (comprising performance for subject relatives and clefts), which like the active preserves canonicity, clusters well above 50% with a mean of 84.11 and a median and mode of 85. By contrast, the distribution of the object condition (comprising performance for object relatives and

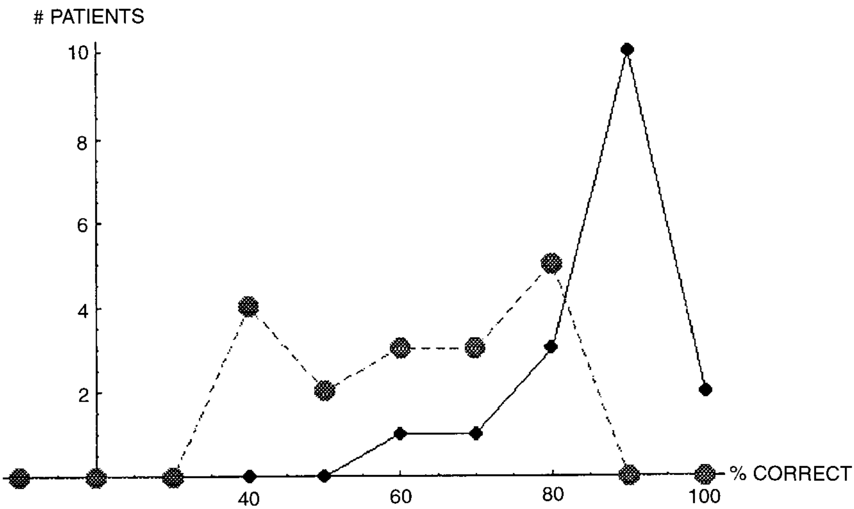

FIG. 3. A comparison between subject- (full line) and object-gap (dashed) relatives and clefts (17 patients at 10–40 trials per case).

clefts) which, like the passive, is noncanonical—shows a clear clustering around 50% with a mean of 58.9, a mode of 80 and a median of 60.

χ^2 tests on both the relative and the cleft constructions show that whereas average performance for subject clefts and relatives is significantly different from chance ($\chi^2(1,n = 100) = 46.24$, $p < .005$), average performance for object clefts and relatives is not ($\chi^2(1,n = 100) = 4$, $p \gg .1$) (see Appendix 2 for data on each subject's performance on each condition). A repeated-measures *t* test was performed, which discerned a difference between the subject and object relatives ($t_{(16)} = 7.59$, $p < .001$).

Again we emphasize that the existence of this pattern is only a result of selection criteria independent from any kind of descriptive generalization or interpretation. The existence of this clear contrast, just like the active–passive one, reflects a stable and solid pattern that is most apparent once we move beyond any one subject and look at a set of subjects with syndrome-related characteristics.

BROCA'S COMPREHENSION—IMPLICATIONS OF ITS REGULAR NATURE

In light of these analyses, it is clear that if we are to seek generalizations concerning brain–language relations, group studies are crucial. Indeed, our analyses point to instances where single-case studies are misleading: in every instance where patients' performances may come in distributions (e.g., chance and perhaps others) and do not converge on single value, one can never know where exactly in the distribution an individual patient's performance falls.

This last conclusion has two important consequences. First, it casts doubts on claims that in Broca's aphasia, the speech production deficit may manifest without an accompanying comprehension problem.[6] That is, cases in which a comprehension problem in passive and object relative and cleft seems absent may be mere distributional artifacts: the patients may have performed at chance, yet their scores happened to be on the higher end of the distribution. In such cases, repeated testing is advisable, because if a deficit exists, the probability of exposing it increases with repeated testing. By the same token, our conclusions weaken the diagnostic value of the active–passive comprehension contrast—it can be used just for a positive, yet not for a negative diagnosis of an individual as a Broca's aphasic, even though it is part and parcel of the overall behavior of the group. That is to say, if a patient performs at chance on passive and object relative clauses, and above chance on actives and subject relatives, we can use these scores for a positive diagnosis; yet the opposite is not true: the diagnosis is not ruled out by other results. What emerges is an interesting situation, in which individual and group char-

[6] Whether or not the two deficits fall under the same description is another matter.

acteristics do not necessarily overlap: aphasia manifests certain clinical signs in every individual, yet there are other pathological phenomena that surface clearly only when a group is examined. The diagnosis of each individual case must be done, thus, through the use of other means, most notably, the speech production patterns of the patients and their neuroimaging data.

To conclude, then, proponents of the single-case approach have done an important service to the field by pointing to variation among patients. Here, we take the next step: we show how variation (in this case—in comprehension of structures that deviate from canonicity) can be understood and identify its source. It is a well-defined statistical property of chance performance. Viewed thus, this syndrome presents a very clear comprehension pattern: above-chance performance for active sentences and chance for passives. Moreover, a similar pattern is discerned for the two types of relative clauses and clefts we looked at. As matters stand, this pattern can be captured by the abstract terms of syntactic theory and tied to a disruption to cortically localizable processing resources.

APPENDIX 1

Scores of all Eligible Subjects on the Active and Passives (% Correct)

Subject	Study	Active	Passive
AB	BHPP	80	40
AK	SCGT	79	42
AT	LSS, SLSP	67	35.5
B	G	100	29
BL	SSM	71	29
D	G	86	64
DM	SS, GPM	79	71
EB	LSS	67	54
ED	SCGT	71	29
EG	L	79	33
EM	SS	96.5	76
ER	BHPP	90	40
ES	GPM	85	35
FA	BG	70	70
FC	SS, BG, HA	85	73.25
FM	OS, SLSP, BSMB, BNC	52.6	56
GV	BHPP	90	50
HO	H	83	50
HR	SSM	96	42
HT	SSM	50	54
JG	BHPP	100	40
JR	SSM	67	71
LD	SS	88	63
LS	LSS, SLSP	83	57.5
MB	H	67	33
ME	OS, SLSP	92	95
MS	H	83	17
NF	H	100	55
PJ	OS, SLSP	100	90.5
POE	KvG	100	100
RB	GPM, G	100	50
RD	SS, GPM, HA, G	100	72.38
ROO	KvG	90	45
SL	H	67	33
SP	CF	100	50
SY	H	100	67
TS	H	83	67
VS	SSM, LSS, SLSP, BSMB	86.8	59.75
WF	BG	100	100
YM	BHPP	80	50
YO	H	100	67
YY	H	100	66
	Mean:	84.85	55.28

Note. Scores of patient who participated in more than one study are averaged.

BC, Badecker, Nathan, & Caramazza (1991); BG, Balogh & Grodzinsky (1996); BHPP, Beretta, Hartford, Patterson, & Pinango (1996); BSMB, Berndt, Salasoo, Mitchum, & Blumstein (1988); CF, Caplan & Futter (1986); G, Grodzinsky (1995); GPM, Grodzinsky, Pierce, & Marakovitz (1991); HA, Hicock & Avrutin (1995); H, Hagiwara (1993); KvG, Kolk & van Grunsven (1985); L, Linebarger (1990); LSS, Linebarger, Schwartz, & Saffran (1983); OS, Ostrin & Schwartz (1986); SCGT, Shankweiler, Crain, Gorrell, & Tuller (1989); SLSP, Schwartz, Linebarger, Saffran, & Pate (1987); SS, Sherman & Schweickert (1989); SSM, Schwartz, Saffran, & Marin (1980).

APPENDIX 2

Scores of Performances of All Subjects on the Subject- and Object-Gap Relatives and Clefts (% Correct)

Subject	Study	Subject relative	Object relative
AB	BHPP	75	45
AT	LSC	85	65
DR	LSC	75	55
DT	LSC	80	40
EM	G	90	75
ER	G	85	60
ER	BHPP	90	60
GV	BHPP	60	40
JG	BHPP	85	75
LD	G	85	35
MJ	LSC	90	80
RD	G	90	35
SP	CF	100	66.67
SP	LSC	90	65
VM	LSC	95	80
VP	LSC	85	80
YM	BHPP	70	45
	Mean:	84.11	58.92

Note. BHPP, Beretta, Hartford, Patterson, & Pinango (1996); CF, Caplan & Futter (1986); G, Grodzinsky (1989); LSC, Lukatela, Shankweiler, & Crain (1995).

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