Algorithmic and Heuristic Processes Revisited

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0. INTRODUCTION

In this article we reexamine one of the most commonly accepted claims in the study of aphasia: that some aphasics (mainly those of the Broca’s variety), though unable to utilize syntactic, rule-based processes during the comprehension of input sentences, are still able to bypass this deficiency via the use of heuristics; and since in most situations in life the amount of nonformal cues and clues accompanying a sentence is quite sufficient for the aphasic to extract a correct interpretation, the comprehension deficit in Broca’s aphasia is noticeable only under highly specific experimentally induced constraints: Only when all semantic and pragmatic cues had been removed do Broca’s aphasics perform poorly in comprehension tasks.

This claim was originally made in a well-known paper by Caramazza and Zurif (1976). In an experiment they conducted, they found that while semantically “nonreversible” sentences were easily and correctly interpreted by Broca’s aphasics, “reversible” ones yielded poor performance. This finding motivated the claim that these patients “are unable to use syntactic-like algorithmic processes,” and that “at least for the Broca’s

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aphasics, brain damage affects a general language processing mechanism that subsumes the syntactic component of both comprehension and production.” (Caramazza & Zurif, 19/6, p. 581).

We are going back to this decade old claim not just to point out a historical curiosity. Its influence on the thinking of aphasists has been immense. Similar claims are found in many subsequent publications on the subject of comprehension in Broca’s aphasia or agrammatism, reflecting, in our view, a commonly held belief concerning the comprehension abilities of Broca’s agrammatic aphasics. What we would like to argue here is that there is no evidence whatsoever in support of this claim. even though it seems to us intuitively correct. So, while not necessarily rejecting the conclusion, or at least its general spirit, we show that there are no data supporting this claim, and that its impressive prevalence is due to its intuitive appeal on the one hand, and to incorrect readings of some experimental findings on the other. In order to make our case, we will have to go into a short methodological digression concerning experimental techniques and the interpretation of findings, in light of which we later examine the relevant literature.

1. TYPES OF SENTENCES, TASKS AND RESPONSE SHEETS

One point about language comprehension that was demonstrated many years ago is that the semantics of a sentence may be instrumental in speeding up its on-line analysis. As Slobin (1966) showed, semantically “nonreversible” passives (1a) are understood faster than “reversible” ones (1b):

(1) a. The flowers are being watered by the girl (nonreversible passive)
    b. The cat is being chased by the dog (reversible passive)

This fact motivated a distinction between rule following, formal processes, and heuristics that the language comprehension device uses. The difficulty created by the noncanonical arrangement of constituents in the passive construction, it was argued, may be bypassed by appealing to semantic heuristics. This method was used in the experiment under discussion. Yet there was one difference: In experiments like Slobin’s, the dependent variable that was measured was reaction time, reflecting order of difficulty of processing among the sentence types, which is indeed influenced by the reversibility property of the string in question. What Caramazza and Zurif did, by contrast, is to test the reversibility property of sentences against level of aphasist performance in an interpretive task, where errors are counted as the dependent variable. This difference plays a crucial role in our analysis below. Prior to that, however, we turn to experimental tasks.

1 But see Forster and Olbrei (1973) for a later development.
The task commonly used for testing the interpretive abilities of brain-damaged patients is the "sentence-picture matching" task. In this task, subjects hear (or read) a sentence, and then they are confronted with several pictures, only one of which depicts the meaning of the stimulus sentence. The dependent variable here is error rate, since usually performance on this task is not timed. Given that the task is a forced-choice one, the most important manipulation is the choice of pictures. Even if the syntactic structure of the stimulus sentence is kept fixed, there are, as most experimenters realize, many possible error types, and the solution space can clearly influence the outcome. Take, as an example, the sentence in (2), and the possible answer sets (each representing a set of pictures) in (3) and (4):

(2) The tall boy kissed the girl
(3) a. 'The boy kissed the tall girl'
    b. 'The tall boy kissed the girl'
(4) a. 'The tall boy kissed the girl'
    b. 'The girl kissed the tall boy'

For (2) as the stimulus sentence, with an answer sheet with the pictures whose content is represented in (3), all it takes for giving a correct response is to know who among the two characters is tall. In (4), however, one needs to know who did the kissing. We can see, then, how the choice of pictures to define the solution space may influence the outcome: If our patient is impaired in a way that enables him to compute adjective-noun relations, but does not enable him to compute, say, subject-predicate relations, he might do well when the choice he has to make is among the options in (3), but perform poorly in (4).

Similarly, the syntactic properties of the sentences presented can determine performance level. One can imagine a syndrome where patients can comprehend some syntactic constructions, yet they fail on others. So, if a patient is given one sentential type to interpret, success or failure would not necessarily indicate full intactness, or total disruption, of the language faculty. Thus, the choice of syntactic construction is another major determinant of the aphasic performance level in the sentence-picture matching task.

We have thus seen that the choice of both pictures and sentential types may influence the outcome of a comprehension experiment. There is, however, one obvious reservation: This influence is possible only if the problem the patient is suffering impairs him only partially. If the whole language faculty is wiped out or, alternatively, fully intact, then the

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2 There have been some efforts to time the performance on this task (see Ansell & Flowers, 1982). We are, however, unclear about the usefulness of these data, because their theoretical interpretation is far from obvious.
patient is expected to perform very poorly—or perfectly—in untimed interpretive tasks such as the one we are discussing.

As it happens, the comprehension abilities of agrammatic aphasic patients appear to be impaired only partially, and we think that the characterization of this pattern is syntactic (see Grodzinsky, 1986a, for a proposal of a descriptive generalization over agrammatic comprehension data). If this is indeed the case, then the choice of materials—both sentences and pictures—deserves careful attention in the analysis of experimental results concerning agrammatic comprehension. We do just that next.

2. THE CARAMAZZA AND ZURIF EXPERIMENT

The experiment we wish to criticize, it should be said, was conducted in a context quite different from the present one. That is, it was widely believed then that Broca’s aphasia, and agrammatism, are both associated almost exclusively with production problems and that the comprehension of these patients is generally intact. Against this background Caramazza and Zurif set themselves to test the comprehension abilities of the aphasics. What they wanted to find out was whether the relatively good comprehension performance of these aphasics was due to the use of “heuristics,” or whether the aphasics “have retained the capacity to use algorithmic procedures in the comprehension of language?” (p. 575). To this end, they devised a comprehension test consisting of four conditions. Three had center-embedded object relative clauses, and the fourth consisted of active sentences. Among the former, one type was “nonreversible,” where the predicate in the relative clause was agentive, and the actors were represented by one inanimate and one animate NP. The matrix (main) clause had a predicate adjective that could, in principle, be predicated of either NP. Such a sentence we find in (5a).\(^3\)

The next condition, called “reversible,” featured sentences in which the two animate actors in the relative clause were both equiprobable to carry out the action encoded by the verb, from a semantic point of view (5b). A third condition involved “improbable” sentences, describing an improbable event happening to two animate NPs (5c). The fourth “control” condition had “nonreversible” active declarative sentences (semantically similar to the first condition). One of the NPs was modified by an adjective which could, in principle, also modify the other NP (5d):

(5) a. The apple that the boy is eating is red (nonreversible)
   b. The boy that the girl is chasing is tall (reversible)

\(^3\) In this condition, the authors incorrectly claim that the adjective can modify only one of the NPs. If so, then how come one of the possible answers in front of the patient is a depiction of the adjective wrongly modifying the other NP? Here they are clearly contradicting themselves.
c. The boy that the dog is patting is fat (improbable)
d. The boy is eating a red apple (control)

The results of the experiment (for the Broca’s aphasics on whom this note focuses) were straightforward: The patients performed well above chance on the “control” and “nonreversible” conditions, and at chance level on the other two (that is, “reversible” and “improbable”). The authors, as we have already pointed out, took this finding to indicate that semantic cues are heuristically interpretable by the patients, and when these are removed the patients’ comprehension is poor. Thus, there is a dissociation between heuristic and algorithmic processes in language comprehension, as the name of their article would suggest.

Yet in light of the methodological discussion above, it is advisable to question this interpretation. If we consider the solution space for each condition, quite a different story emerges. Caramazza and Zurif used four different distractors for each sentence type. Yet here we focus on one of these, since it is the most interesting one to us as well as to these authors, who illustrate it in their Table 2. There, it turns out that only in conditions involving reversal of thematic roles (subject-object reversal) did the patients perform abnormally (that is, at chance levels). The discussion below therefore focuses on this condition.

Consider, first of all, the “nonreversible” sentences. The reason for testing the aphasics on such sentences is to see whether semantic (or “real world”) knowledge would improve their performance. That is, whether they would use nongrammatical knowledge to bypass potential hurdles that their impaired language faculty puts in front of them. The patients were thus asked to choose between a correct picture and a distractor. In “nonreversible” sentences this means that they were presented with one picture of a boy eating an apple, for the example above, and another for an apple eating a boy, and they had to choose between these two. Now, knowledge of the world should enter into play here, and aid the patient. But how could it?

Giving such a pair of pictures changes the rules of the game. “Non-reversibility” is defined modulo a given universe of discourse, and a depiction of an event that is impossible in the natural universe clearly modifies the world against which the subject has to decide on his response. The procedure in question amounts to telling the patient: “Now we are in a new universe, where apples can indeed eat people.” In such a universe, the common, everyday heuristics clearly cannot work, and the patient cannot be expected to invoke them to solve the problem, because the knowledge on which they are based is simply contradicted by the current situation. So, real world knowledge is useless in imaginary worlds where apples eat people. This experimental problem, we should add, is not specific to this study, but is inherent to this design. If the sentence
to which the patient is supposed to match a picture is strange, then there is at least one strange picture in the answer sheet.4

Thus, "heuristics" as Caramazza and Zurif intended cannot be operative here. In other words, cues that are valid for real world situations cannot be used in imaginary settings, and hence could not be used by the patients in this experimental condition.

However, the fact of the matter is that the patients did well on this condition—they made few mistakes on the "nonreversible" sentences where the distraction was subject-object reversal, and performed at chance on the "reversible" ones (see Fig. 3, p. 580). How can we explain this performance pattern? Having rejected the "heuristic versus algorithmic processes" explanation, we are back to square one. What we have to account for at this point is the patients' chance performance on the "reversible" and "improbable" sentences, and their above chance performance on the "nonreversible" and "control" ones, where the distractor in all cases is subject-object reversal.

In Grodzinsky (1986a), it is proposed that traces of movement are deleted from S-structure representations for agrammatics, and that their incomplete syntactic analyses are augmented by a heuristic of the N-V-N type. Since traces mediate θ-role assignment, there are some NPs which lack a θ-role, and the N-V-N strategy assigns them the thematic role of agent, if they are clause-initial. This is how chance performance on passives, object relative clauses, and object clefts is derived and contrasted with the constructions where above-chance performance is observed. All these considerations hold, naturally, for situations where semantic and pragmatic cues are controlled for. We can immediately derive from this hypothesis three of the four findings of Caramazza and Zurif's study: The good performance on "control" sentences is predicted, because no transformational operation is involved in the derivation of this construction, and hence, no deficit should be observed; the chance performance on "reversible" relatives is predicted, because it is exactly for this construction type that the Trace-Deletion account was proposed5.

4 This is probably the reason why Slobin (1966) used the opposite paradigm, which we might want to call picture-sentence matching: He had a single picture, depicting a possible, nonreversible situation, and then he presented the subjects one of two types of sentences: Either they would get the correct nonreversible sentence (the girl is watering the flowers in the example above), or they would get the inverted thematic role version (the flowers are watering the girl). This way he could get exactly the desired experimental effect, where only possible situations are depicted, but impossible sentences are presented.

5 One additional provision has to be made. In the "reversible" condition the patients could arguably perform well, because they could use the relation between the predicate adjective and the matrix NP to reach a correct solution. So, in sentences like (5b), the Trace-Deletion hypothesis claims that the relation between the boy and tall is intact, and this alone should have been sufficient for getting at the right answer. Yet, the patients performed at chance here. The way out of this puzzle is by pointing out that according
next, if our above argument is correct, and "nonreversibility" does not provide the patients with valuable cues for interpretation in this condition, then the same considerations are true for the "improbable" condition as well, and especially when sentences such as "the boy that the dog is patting" are at issue. These sentences thus have the status of the "reversible" ones, and the chance performance follows from the Trace-Deletion account. But relying on this account buys us an obvious problem: Now we have to explain why the patients performed so well on the "nonreversible" sentences, even though we argue that the semantic cues are worthless in this situation.

In fact, when we look at the results carefully, there is a strange discrepancy between the results of the "improbable" and "nonreversible" conditions: In both cases the solution space consisted of a thematic role inversion foil that depicted an imaginary situation—a dog patting a boy, or an apple eating a boy; yet while in the former case performance was at chance, in the latter performance was above chance. The explanation we have to that lies in the fact that there is a contrast between these two groups of sentences which is perfectly confounded here: The "nonreversible" ones all contain an inanimate character, whereas the "improbable" ones contain an animate character that is unlikely to be agent in any situation in life. This contrast may account for the difference in performance in that the thematic role reversals depicted for the "nonreversibles" (an apple eating a boy) were so bizarre that the patients chose to ignore them altogether, thereby consistently giving the right answer despite their syntactic impairment. It is our contention that if the authors really controlled for this oddity, and would give their subjects bizarre sentences, such as "the apple is eating the boy", and not just pictures, then the subject would realize that they actually have to pay attention to the odd picture, and performance would plummet to chance level on object relatives.

What we are claiming, in short, is that the patients' good performance on the "nonreversible" sentences is not due to their ability to use the "nonreversibility" to relate the actor/acted upon to their appropriate surface positions in the sentence. Rather, the oddity of the "nonreversible" pictures barred them from being candidates for response altogether. So, from the point of view of interpretive processes, the nonreversibility of the sentences is irrelevant.

Let us summarize what has been shown so far: first, that in the ex-
perimental paradigm Caramazza and Zurif used the "nonreversibility" of sentences did not make them easier to interpret. What it made the patients do instead was something unrelated to the sentences at issue: They were simply forced to ignore weird pictures, irrespective of the sentence type they heard. Second, we have attempted to provide an alternative explanation to the results, in part by alluding to the Trace-Deletion hypothesis that one of us proposed. This hypothesis seems to have many other predictions that are borne out. It correctly predicts, for instance, agrammatic good, above-chance performance on reversible actives, subject relatives, and clefts (see Grodzinsky, 1986a, 1986b).

The above critique, we believe, shows that the conclusion reached by Caramazza and Zurif had little to do with the data they had at hand. In all fairness, we hasten to add that the conceptual framework within which they operated could hardly direct them toward an interpretation of the type we offer here. Our criticism is directed less toward their original claims than to the way they were adopted uncritically later. In one respect, though, they were clearly off the mark: Their conclusion that Broca's aphasics were "syntactic" was unsubstantiated at all. From the one syntactic construction on which the patients' performance was imperfect, one can hardly reach such a radical conclusion. But this is somewhat beside our point. What we would like to do next is show that in the literature subsequent to the article we have just discussed, there is still no evidence supporting the distinction between algorithmic and heuristic processes in comprehension, despite its intuitive appeal.

3. OTHER EXPERIMENTS

After its publication, the Caramazza and Zurif study has had many followers. Most of them tested aphasics comprehension in reversible sentences only (Schwartz, Safran, & Marin, 1980; Heeschen, 1980; Liebarger, Schwartz, & Safran, 1983; Wulfeck, 1984; Grodzinsky, 1985; Caplan & Futter, 1986). Only two have tested both reversible and non-reversible sentences (Deloche & Seron, 1981; Ansell & Flowers, 1982; Kudo, 1984). In the former series there is obviously no evidence whatsoever pertaining to the algorithmic/heuristic distinction. All these studies chart aspects of the aphasic limitation in comprehension. Still, they all repeat the Caramazza and Zurif position. The three studies that actually tested reversibility are impossible to interpret in the present context: Deloche and Seron used only active sentences in a speeded task, with exposure time of 300 msec per sentence. The differences they got are thus not

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6 This experiment included a "nonreversible" condition, but oddly enough, the sentences were such "only in a less strict sense," for example, the recruit shouts at the officer. Thus the fact that there was a picture for this interpretation in the response sheet makes it perfectly "reversible." Also, what was actually investigated in this study was patients' sensitivity to case inflections and not their use of strategies.
germane to the present discussion, since agrammatics have no particular problems with interpreting reversible actives. Ansell and Flowers (1982) do not report what the foils were in their experiment. Finally, Kudo (1984) says in his methods section just that “the two pictures in each item differed from each other in a single detail.” (p. 209). We cannot know what this detail was.

So, a review of the relevant literature shows that although Caramazza and Zurif’s claim is virtually universally accepted and, in fact, has stimulated a fair body of research, no data supporting it exist.

4. CONCLUSION

We have shown how a careful analysis of an experiment may have surprising results. Our moral of this story consists of several points: First, an analysis of a performance must take into account both the syntactic construction used and the solution space provided for a forced choice task. One cannot just take “syntax” to be a single, unanalyzed variable, as has been done often in the past. Similarly, “sentence-picture matching” is just a part of a task description. Only an analysis of the response type and the solution space, coupled with the syntactic construction in question, constitutes the actual “task analysis.” This, we believe, is the only way to assign a coherent interpretation to data from the comprehension of brain-damaged people. This, of course, is against common wisdom in neuropsychology, where tasks are always equated with activities such as reading, writing, repetition, etc., and syntactic form and response options are lumped together. We are thus putting ourselves in opposition to many interpretations of aphasic performance (see, for example, Caramazza & Berndt, 1985; Caramazza, Berndt, Basili, & Koller, 1981), and many, many others.

Second, we note that the hypothesis Caramazza and Zurif put forward concerning the syntactic abilities of Broca’s aphasics was too extreme and that their data did not warrant such a powerful claim. Again, if one believes that “syntactic ability” is an unanalyzed concept, then aberrant performance on any task involving syntactic analysis would lead to the conclusion that a patient group is “asynaptic.” Yet if syntactic ability is seen as being underlain by a complex of algorithms and a rich knowledge base, then there are very many imaginable manners by which such a system can be disrupted, and the evidence necessary to motivate claims about the total destruction of the syntactic processor is much more than Caramazza and Zurif presented. In fact, the correct characterization of the syntactic abilities in agrammatic Broca’s aphasia maintains only a partial impairment. As a consequence, these patients have access only to incomplete syntactic representations, which explains why their differential performance correlates with specific types of syntactic constructions.
Finally, if we have recounted this historical tale accurately, we can see how strange the course of scientific progress may sometimes be. Even though Caramazza and Zurif misread their findings, they still stimulated a lot of research and attracted much attention to their study. This is, we believe, because they simply had the right intuitions. It seems very plausible that the aphasic deficit, observed only when semantic cues are lacking, can be bypassed by heuristics, and that this is the reason why Broca’s aphasics strike the clinician as having good comprehension. Yet this point has never been experimentally demonstrated, and in light of this story we are not at all sure that it needs to be shown. If everyone agrees, then so be it. Yet this interpretive error remains, and so will the lesson it teaches us, we hope.

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