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Sign Language Research and Linguistic Universals

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sign language research

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linguistic universals

new england sign language society¹

Early research on native American Indian, African and Austronesian languages reflected the bias that Latin was the complete and perfect form of language. Consequently, grammars of these "new" languages attempted to describe the structure as it related to the Latin model, concentrating on the similarities and the differences. Research on sign language reflects a similar bias, only here the model is spoken language. In the United States the model is English, and American Sign Language is described (and evaluated) as it parallels and differs from English. Other sign languages are similarly viewed from the bias of the language spoken in their geographical or sociopolitical area.

As the linguistic notion that there is more to a language than the surface manifestations of its sentences developed, grammars of languages shifted emphasis to internal organization of syntax and morphology, and, eventually, as the notion of generative grammar grew, to the rules which were responsible for the internal organization. It

was then possible to compare and contrast languages in terms of their rules, and the search for universals of rules and rule types accelerated. The search for universals continues to this day. Unfortunately, research on manual languages has not transcended the bias of oral languages, leaving the rules and rule types of manual languages essentially undescribed.

We are concerned here with the difficulties which arise when the search for universals intersects with premature and biased descriptions of manual language. Specifically, we are concerned with three papers, Tervoort (1968), Schlesinger (1970) and Wasow (1973), which all deal inappropriately with manual language. We are particularly concerned with inadequate research precautions and questionable conclusions stemming from spoken language bias. At the same time these researchers seek to relate manual language to linguistic universals. We will deal with the papers in the order mentioned, as it reflects both the development of linguistic frameworks and the structure of the arguments related to linguistic universals.

Tervoort.

Tervoort (1968) represents research conducted within the structural linguistic framework, being primarily concerned with the sentence and word groups. Tervoort is seeking to corroborate, in manual language, Reichling's claim that basic word groups are determined on the basis of word order ("sequencing"), intonation pattern or morphological correlations. Tervoort claims that his research on American Sign Language provides evidence for word sequencing as the fundamental principle for word grouping.² Immediately we can point out that this reflects a bias from English, which has relatively fixed word order in clauses, or, if not entirely from English, since Tervoort also knows Dutch, at least from Germanic or Indo-European languages. In this section, we critically examine Tervoort's methodology and conclusions.

The framework in which Tervoort was operating led him to look for universals in terms of word order. His paper is concerned with determining the grammaticality of the sentences formed by taking all possible orderings of the words in the string YOU ME DOWNTOWN M-O-V-I-E F-U-N ?³ This original sentence was a string obtained from a conversation between a deaf student and his girlfriend in Washington, DC. The permutations were written down and presented to 120 student-teachers who were graduate students in a teacher-training program. Some of them were deaf, and all of them were familiar with the features of communication among deaf children. They were asked to judge "each permutation on its sequence of signs and the groups that might appear exclusively."

In conducting his experiment, Tervoort has displayed the bias of the framework he was working in by concentrating on the word groupings. He does this to the exclusion of other factors which are crucial to research on manual language. The fluency in ASL of his source for the original sentence is never documented and the initial sentence itself is suspect.

The signer used separate signs for YOU and ME, ignoring an ASL sign which could be glossed YOU-ME. The use of the separate signs YOU and ME is more English-like than the sign YOU-ME. Research on the language-contact situation of ASL and English (Woodward, 1973 a, b, c) indicates the existence of a language continuum with ASL at one end and English at the other. This is a diglossic situation in which the choice of lexical items and syntax is governed by numerous sociolinguistic and linguistic factors. These factors include whether a person is deaf or hearing, whether his parents are deaf or hearing, whether he is a native signer or not, and whether the conversational situation is formal, such as in a classroom, or informal, such as at a party. Certain syntactic rules such as Object Incorporation and Subject-Object Agreement (Kegl, 1976b) are features of ASL-dominated conversations but not English-dominated conversations. In the absence of any other data from Tervoort's sole source, it is not possible to determine whether the use of the finger-spelled forms M-O-V-I-E and F-U-N is due to the signer's unfamiliarity with the available ASL signs MOVIE and FUN, or to the somewhat more formal situation of making a direct request for a date or to dialect incongruence.

Ignoring the fact that a transcription of ASL in English removes most of the relevant details of the signs themselves (everything except some of the meaning and the order of occurrence), Tervoort asked for judgements based on his English glosses for the sentences and not on the signed sentences themselves. Furthermore, presenting the glosses in written form invites judgements to be made from an English viewpoint, since that is the language judges actually see. The final manifestation of the spoken language bias is the fact that Tervoort was willing to obtain judgements from native speakers of English who are not native signers. One could hardly defend an investigation which literally glossed Russian into English, presented these glosses to native speakers of English who knew "some Russian", and requested them to judge whether the sentences were grammatical in Russian. This is essentially what Tervoort has done.

In reporting his results, Tervoort omits any statistical analysis which would corroborate his conclusions. He concludes that "of all 120 permutations some were₄ more ambiguous than others but none was considered to be esoterically₄ ungrammatical by the majority," (p 464). Further he reports the word groupings which were felt to be the strongest by the judges. These include the groupings which contained YOU and ME with no intervening words (in English glosses, you me downtown movie fun, me you downtown movie fun, downtown me you movie fun, and 45 similar arrangements). Another group which was felt to be strong consisted of the remaining three words, DOWNTOWN, M-O-V-I-E and F-U-N, in their 24 uninterrupted permutations. Word groupings which were interrupted (you fun me downtown movie, downtown movie you fun me, and the remaining 46 permutations) were felt to be less strong. However, no numbers were given to allow the reader to know exactly how strong the various groups were felt to be.

Judgements of relative strength or ambiguity were made in the absence of any context by people who were not native signers without actual

presentation of the signed sentences. We feel that these are serious methodological problems that therefore undermine the conclusions which are drawn from the experiment.

Schlesinger.

Schlesinger conducted his research utilizing principles of generative grammar, which, unlike Tervoort's structural framework, led him away from word groupings and surface forms, to investigate underlying rules and grammatical relations. In seeking the rules which generate the sentences of Israeli Sign Language, Schlesinger set up an experiment to elicit sentences which contained subjects and objects.

We will pursue discussion of Schlesinger's experiment according to the following outline: (1) presentation of the details of Schlesinger's preliminary and modified experiments and the claims that he makes on the basis of his findings; (2) criticism of his research design focusing on his subjects, his methods and his analysis of the data; and, (3) an examination of the assumptions which underlie his entire investigation.

Overview of Experiment.

Preliminary Method.

In the initial experimental situation each deaf signer was required to convey the content of a picture to another deaf signer who had to demonstrate comprehension of the message by pointing to the proper picture of a pair of extremely similar pictures. For example, the pair of pictures might show a black dog biting a white dog and a white dog biting a black dog. The experimenter provides the sender with the picture to be described and the receiver must rely on the sender's description for information leading to his choice.

Results.

Using this technique, Schlesinger found a dominant word order, with agent preceding object or goal of action, an order which parallels that of Hebrew. However, he also reports that about half of the subjects were inconsistent in their use of this order which led to confusion in getting the message across. From his preliminary findings, Schlesinger went on to investigate more complicated situations to see if an improved experiment would produce different results.

Revised Method.

The new experiment involved the same basic technique, but this time the picture required descriptions which indicated subject, direct object and indirect object, and the number of pictures that the receiver had to choose from was increased from two to six. In addition, Schlesinger made major changes in the content of his pictures. The earlier versions used

situations in which the direct object was an inanimate object such as an apple. The only confusion which could occur was between subject and direct object, that is, whether the boy gave the apple to the girl or the girl gave the apple to the boy. In the new pictures, he wished to elicit all three nouns explicitly and therefore used pictures in which either one of the three nouns could legitimately be the subject, the direct object or the indirect object. For example, one picture showed a man handing a bear to a girl, while another showed a man handing a girl to a bear. It is necessary to specify all three as either agent, recipient or affected element in order to unambiguously communicate the content of the picture.

Results.

He reports that no rule was consistently used by all the subjects, and that any individual might use several orders during the course of the experiment. The verb appeared most often after the second noun, less often after the first noun, and occasionally after the third noun.

Claims.

On the basis of the preliminary study, which indicated that the adjective modifier always followed the noun, Schlesinger concluded that "the claim that Israeli Sign Language has no syntax can now safely be dismissed." On the basis of the modified study, Schlesinger concluded that there was a rule which indicated where the verb may not appear -- namely, at the beginning of the sentence, -- but remarks that it is paradoxical that Israeli Sign Language has these particular two rules but not "more crucial" rules, eg, the "transformation rules for other relations, such as 'subject of', object of'" (p 115). Because of the importance of this claim to a theory of linguistic universals, we feel that the basis for it must be critically examined.

Research Design.

Subjects.

Like Tervoort, Schlesinger failed to determine the competency of his subjects in ISL or to ensure a homogeneous population. He readily admits that "our deaf subjects varied widely in their degree of mastery of this language [Hebrew]," (p 103). They covered the full range from good knowledge of Hebrew, probably associated with mastery of Signed Hebrew, to poor knowledge of Hebrew, probably but not necessarily associated with mastery of Israeli Sign Language. The comparable situation in the United States indicates that users of American Sign Language often readily comprehend sentences from Signed English but that users of only Signed English have great difficulty in understanding American Sign Language. This is primarily due to the fact that the education system insists on teaching English syntax to deaf children in school and they are consequently familiar with the syntax of Signed English. Educational institutions in the United States do not teach American Sign Language or its structure. The result is that deaf individuals may have a range of competency in ASL, and we suspect that the same holds in Israel with respect to ISL.

Schlesinger acknowledges the existence of this situation and confirms its effect on the results of his experiment by reporting comprehension rates. He defines a "full sentence" as one which contains a separate overt representation of the subject, direct object and indirect object, and then sorts his subjects into those who used full sentences (8 used them always and 12 sometimes) and those who never used them (of which there were 10). He further divided full sentence users into those who used the same word order all the time and those who varied word order. He reports then that comprehension of received messages was 74% correct when both members of the sender-receiver pair were in the full-sentence/same word order group, and that it dropped to 54% when one member of the pair was in the full sentence/same word order group and the other in the full sentence/variable order group. If both members were in the full sentence/variable order group, comprehension was still lower at 37.5%. Finally, if both members were in the non-full sentence group, comprehension was 44%, a result which surprised Schlesinger. The analysis of the results will be discussed below. The point here is that performance in the experiment was greatly influenced by the composition of the pairs of sender-receiver. One further factor which could have influenced the efficiency of a sender-receiver pair is the age of its members. Older subjects may have been immigrants who learned ISL late, possibly as a second or third language, while younger subjects could have been native ISL users. In addition, the length of time since immigration would make a difference in the fluency of the person's signing. Schlesinger provides no information on the ages of his subjects.

One can better understand our suspicion concerning Schlesinger's investigation by considering the results of a similar experiment done with users of American Sign Language. Bode (1974) conducted the same experiment as Schlesinger but with a group of native ASL signers and a hearing control group of native speakers of English. She found 86% comprehension for the signers and 95% for the speakers, the difference being non-significant. She concludes that the major flaw in Schlesinger's experiment is his choice of subjects.

Bode's experiment strongly suggests that either something is very wrong with Israeli Sign Language or something is very wrong with Schlesinger's experiment. Given what we know about the manifestation of grammatical relations in American Sign Language (Kegl, 1976b), we are not surprised that Schlesinger's methodology and analysis failed to reveal grammatical relations in ISL. Extended to ASL, his approach would probably fail to find grammatical relations in ASL. His entire analysis is based on the incorrect assumption that grammatical relations will exist in a linear sequence, rather than in simultaneous realization as, for example, in ASL. We will continue by pointing out those aspects of Schlesinger's experiment which, when added to the problem with his choice of subjects, serve to bring his claims regarding grammatical relations in ISL into considerable question.

Method.

Another of the factors of Schlesinger's experiment which makes his claims extremely suspect is the artificiality of the experimental situation itself. It must be kept in mind when evaluating any experiment that there is a strong possibility that what a subject can do in an experimental situation is not necessarily what he does do in real life. This is especially true in language research, where the very fact of being in a controlled experimental situation greatly limits communication, particularly because of the removal of contextual cues. There is a real question here as to the use or non-use of context in sentence comprehension. This will be discussed later.

Aside from the general problem inherent in controlled experimental settings, Schlesinger's experiment was designed to specifically exclude pragmatic clues by the construction of the pictures used. In these pictures, unusual situations are depicted, eg, a man handing a bear to a monkey. The reason for this unusualness is the necessity for each of the figures to be potential subjects, direct objects or indirect objects. Schlesinger indicates that "the fairy-tale-like quality of the situations described did not seem to disturb our subjects," (p 104). He does not consider its effect on their performance, except to mention that "the experimental task placed a heavy load" (p 114) on the subjects, and he attributes their poor performance to "their getting confused by its complexity," (p 114). The fact that the highest percentage of correct comprehension was only 74% strongly suggests the interference of task variables with linguistic factors.

Analysis of the Data.

Our concerns with Schlesinger's data analysis focus on two aspects -- what he chose to analyze and how he chose to analyze it. It is here that the bias of spoken language is most apparent. He assumes that grammatical relations will be displayed in a linear sequence, which may have led him to ignore crucial data and to conclude that ISL has no means of indicating "subject of" and "object of".

To begin with, Schlesinger does not provide statistical analysis for his results. He gives no standard deviations for his means or totals for his analyzed sentences. The comprehension percentages he reports are above chance (17%) but it is not clear that all of them are significant. The differences on comprehension scores based on sentence form and word group are also not analyzed for significance. Another problem is that Schlesinger collected data from 30 subjects, but his comprehension results leave data from 6 of them unaccounted for (2 in the full sentence group and 4 in the non-full sentence group).

More important than the absence of formal statistical analysis are the decisions Schlesinger made concerning what to analyze. Schlesinger states that "some of the utterances were repetitive and complicated so that it was rather difficult to decide where a sentence ended and a new one

began," (p 108). Consequently, he finally chose to analyze only "full sentences", those signed utterances which contained separate overt representations for the subject, direct object and indirect object and the verb in linear sequence. He excluded from analysis those signed messages which followed a manual language device of setting up positions in space for each noun. This device allows signers to then utilize the points in space as points of agreement for pronominal clitics which are simultaneously realized as part of the verb (see Kegl, 1976a for further discussion of agreement in ASL). He mentions that this type of device which introduces the nouns first then indicates the action, was used. However, his general bias towards spoken language prevented him from considering the possibility that "full sentences" defined on the basis of inclusion of all three nouns are not the only way that a complete sentence can be produced. Schlesinger did not realize at the time that repetition, for example, marks continuity and stress and that such factors as body shift, facial expression, eye contact, decreasing rate of production down to the last sign, and pauses may mark the end of a sentence (Baker, 1975; Covington 1973a, b; Grosjean and Lane, 1976).

Schlesinger admits that there is "interference from the Hebrew in studying the syntax of ISL," (p 103), but he fails to take this into account in the analysis of the data. His definition of the "full sentence" is exactly that which would occur in Hebrew if all three elements were semantically intended. Only 20 of his 30 subjects actually used full sentences, so immediately he is led to exclude one-third of the data which he obtained. Of the 20 who used "full sentences", only 8 did so consistently, causing some of the data from the remaining 12 to be excluded as well. His confidence in his definition is not shaken even when he reports that for 9 subjects who used the "full sentence" form, only 33 of the 49 messages included in the analysis were actually understood correctly. This information, taken together with earlier reported comprehension results, which indicated that those people that did not use the full form understood each other 44% of the time, strongly suggests that the mechanisms by which ISL communicates "subject of" and "object of" are not to be found utilizing a Hebrew or English definition of sentence.

However, Schlesinger considered the results to indicate that ISL did not have any mechanisms for indicating grammatical relations and that the "full sentences" which occurred were borrowed from Hebrew. He is forced by his assumptions to maintain this conclusion even though it is inconsistent with his own observation that "ISL is an adequate vehicle for everyday give and take of the deaf," (p 115). He explains the inconsistency by saying that in everyday situations "there is absolutely no need for rules expressing the grammatical relations discussed here," (p 115). If this last claim were true, then it would be difficult to explain why the grammatical relations which Schlesinger is searching for would be so important that they would be linguistic universals. One would expect instead that something which was necessary everyday would make a better candidate for a universal, and that a universal would be so prevalent that it would appear in "everyday give and take." We will turn now to a critical examination of the assumptions that led Schlesinger to adopt this contradictory position.

Assumptions.

We think it is crucial at this point to indicate a fundamental difference between a traditional notion of grammatical relations and the generative notion of grammatical relations, as defined in Chomsky (1965). The traditional subject, for example, is defined on the basis of meaning; it is the agent, the do-er. It can be determined only by reference to the meaning of the whole sentence. The generative notion of the subject is defined in Chomsky (1965) as an NP directly dominated by an S. Other nodes are irrelevant to the determination of subject. The generative notion of object is defined as an NP directly dominated by a VP, and again the rest of the sentence is irrelevant. These two notions are structurally defined in the base. Transformations which move elements may destroy the original node structure which defined the grammatical relations, as happens when rules like Passive apply. Furthermore, in order to claim that a particular noun serves as the grammatical subject, one must provide syntactic evidence that that noun behaves as a subject (cf Li, 1976, for discussion of syntactic properties of subjects). Schlesinger looks for surface order regularities, but does not do a syntactic analysis of his data according to any formal notion of syntax.

In reality, Schlesinger has pre-defined "subject", "direct object" and "indirect object" on the basis of the action in the pictures. He has confused semantic relations and structural definitions in such a way that given the English sentence "The truck was hit by the car", he would have to record the order as direct object - verb - subject, rather than subject - verb - direct object as it appears in the base. Thus, if any of the different word orders which he reports represented passives or other moved structures, he would be unaware of their existence. In fact, although Schlesinger claims to be looking for a consistent order of subject, direct object and indirect object, what he is really looking for is a consistent order of the mentioning of each of the nouns represented in his pictures. He expects the noun which is doing the giving to be mentioned first, followed by either the element being given or the noun being given to. In other words he is looking for a consistent cognitive strategy for presentation of salient information, which is surely separate from the order of syntactic elements.

There are other aspects of Schlesinger's study which can be criticized (such as his statement about transformations which define grammatical relations), but we feel that it is not necessary to do so. Many of our comments are hinted at by Schlesinger himself, as he finds that he must offer qualifications and explanations for his counter-intuitive results and claims. For example, he mentions the following unexpected set of results (Schlesinger, 1970, p 114):

"There were 3 pairs in which neither of the partners ever used the full form. It is of interest that they achieved a somewhat better degree of comprehension than the 'inconsistent' pairs, namely, 44%. Apparently, their

more primitive structures worked better than the inconsistent use of full sentences."

Despite the fact that he recognizes the contrary nature of what he is forced to do here, he avoids the obvious conclusion -- namely, that something is wrong with the experiment, -- and instead continues to argue on the basis of his already weakened and qualified data. We feel that this is the strongest manifestation of bias in linguistic research, when one continues with a particular line of argument even when one is aware of the holes in the argument. Schlesinger's study has been widely circulated and publicized. We hope to have indicated the number of possible counterarguments one can raise to his position. If someone had taken the trouble to investigate this study prior to now, our discussion of the next paper would not be necessary either.

Language Universals and Grammatical Relations.

Several experiments have been reviewed and criticized above. Even when experiments were designed with a plausible goal in mind (for example, the universal existence of grammatical relations), they were foiled by a lack of background knowledge and poor experimental procedures. In spite of the fact that the results of these experiments stand on very shaky ground, many surprisingly strong conclusions have been drawn on the basis of them. The strength of these conclusions and arguments based on them is questionable.

The issue of universals is one in which experimental determination of grammatical relations in manual languages plays a large role. Underlying the desire of linguists to have a consistent theory of grammar rather than series of descriptions based on haphazard organization of facts is a recognition on their part of the need to move from studying facts about individual languages to outlining the characteristic properties of natural language itself. Jackendoff (1976, p 1) summarizes the issue as follows:

"In particular, we take it to be essential to separate those aspects of a speaker's knowledge that are universal from those that are language-particular, under the hypothesis that the language-particular features must be learned by the speaker, but the universal parts may be innate, ie, determined by the structure of the human organism itself. Success in separating universal from language-particular components enables us to make interesting claims about the nature of the mind."

According to Wasow, the structure of the theory of transformational grammar (Chomsky (1965)) contains a basic assumption about the universality of grammatical relations. Chomsky (1965, p 71) proposed structural definitions of "Subject-of : [NP,S]"; "Predicate-of : [VP,S]"; "Direct object-of : [NP,VP]"; and "Main verb-of : [V,VP]". These notions are

referred to when discussing selectional restrictions and semantic interpretation. Wasow claimed that this intricate involvement of grammatical relations in the theory of transformational grammar makes the prediction that all languages will have grammatical relations. However, it is not necessarily the case that all languages have grammatical relations structurally defined in precisely this way. For example, Bell (1976) points out the problems of the structural definition with reference to VSO languages (ie, the difficulty in using [NP,VP] to define the object).

The universal nature of grammatical relations can be considered either as strong or weak linguistic universals. Schlesinger, on the basis of the results obtained in his experiments, felt justified in claiming ISL to be a counterexample to a strong theory of linguistic universals (Schlesinger, 1970, pp 116-117):

"This particular version [the strong version] is extremely vulnerable because it claims that whatever appears in the universal base appears in the base structure of every language. Now, what appears in the base structure of a language must be expressed somehow also in its surface structure. All that is required to refute the hypothesis, therefore, is one language where one of the grammatical relations is left unexpressed in the surface. The conclusion arrived at by the present investigation that ISL is such a language thus invalidates this version."

Schlesinger's actual arguments as to why grammatical relations are a weak rather than a strong linguistic universal are very confused. He argues that there are no formal transformational rules linking the base structure and therefore there is no reason for a formal linguistic base structure. He felt that all one might conclude is that the cognitive structures of users of ISL are the same as those of speakers of other languages. Schlesinger draws this conclusion from the fact that deaf people "perceived differences between drawings in the same way that we do."

Wasow.

Misuse of the shaky conclusions based on Schlesinger's experiment go beyond Schlesinger's paper. Wasow (1973) used Schlesinger's paper and a paper by Rosemont (1972) concerning the absence of grammatical relations in Archaic Chinese⁵, to argue that any language which is not spoken cannot be a counterexample to a theory of universal grammatical relations. He claims that non-spoken forms of communication cannot be considered natural languages.

Without the slightest reservations, Wasow accepted the results of Schlesinger's experiment at face value. He did, however, take Schlesinger's conclusion to task. Wasow points out that, if grammatical relations truly is a weak universal, then it should be cognitively based and therefore present in any form of language. Schlesinger uses strong vs weak universals

in two ways in his paper. For him, the notions linguistic vs cognitive and strong vs weak are not synonymous. By weak universals, Schlesinger is not referring to a feature common to all cognitive processes, but rather to the following (Schlesinger, 1970, p 117):

"According to this version, there is a universal set of grammatical relations out of which each language selects a subset for its base (cf Bach, 1968). Thus, all those relations the linguist discovers belong to this universal set, but it is not the case that each language includes every one of these relations in its base."

Instead of taking Schlesinger's data and method to task, Wasow points out the inconsistency in Schlesinger's conclusion and uses it to his own advantage (Wasow, 1973, p 49):

"If, on the other hand, grammatical relations are strong linguistic universals, then there is no reason to expect non-linguistic communicative systems to express them (although there is no reason to exclude the possibility that they might be expressed in some such systems). Hence the existence of a non-linguistic communicative system which did not express grammatical relations would constitute crucial evidence for the conclusion that these relations are strong linguistic universals."

Notice from the statement above that Wasow has foreshadowed his method of argumentation. He will attempt to prove that any counterexample to the theory of universal grammatical relations is a non-linguistic communicative system. Wasow (1973, p 50) outlines his alternatives as follows:

"There are two possible conclusions to draw from Schlesinger's experiments: either ISL is a counterexample to the claim that grammatical relations are linguistic universals, or ISL is not a natural language, in which case grammatical relations are strong linguistic universals. But there is good reason for thinking that grammatical relations are in fact linguistic universals, and hence we naturally lean toward the latter alternative."

Wasow seems to be ignoring another possible alternative -- namely, that there is a flaw in Schlesinger's experiment.

Wasow presents several points of evidence which he feels gives "overwhelming" evidence "linking the spoken medium with the expression of grammatical relations."

1. There is something special about the spoken modality.

In his argument for the special involvement of the spoken medium with grammatical relations, Wasow brings three things to bear as evidence for

his claim. First, he compares human behavior with chimp behavior. He draws his information from the studies of wild chimpanzees done by Jane van Lawick-Goodall (1971). Wasow (p 44) points out her comments about the "remarkable likeness of the nonverbal communicative behavior of chimpanzees and humans." He also quotes her pointing out the major distinction between man- and chimp-speech (van Lawick-Goodall, 1971, p 251):

"One of the major differences between man and his closest relative is, of course, that the chimpanzee has not developed the power of speech. . . . Verbal language represents a truly gigantic stride forward in man's evolution."

First of all, when van Lawick-Goodall points to the similarity between the gestures of humans and chimps, she is speaking of natural, not systematic, abstract manual languages. It must be noted that there is as yet no proof that any chimp possesses or is capable of learning a language as complex as one of the human sign languages. The notion referred to above as "verbal" language should not necessarily be connected with speech. If one believes that there exists a difference between chimps and humans which is a special capacity for expressing complex, abstract forms of language, then one should believe that any human, whether born deaf or hearing, must innately possess this capacity. And, if this is some special innate linguistic capacity, separate from other cognitive abilities, one should not expect these abstract, universal conditions on language to be affected by the modality of production or perception. We have seen as the result of experimentation with another non-spoken language, American Sign Language, that the language acquisition process for this manual language is similar to that of spoken languages (Bellugi and Klima, 1972; Wilbur and Jones, 1974), and that there are selectional restrictions and grammatical relations (Bode, 1974; Fischer, 1975; Friedman, 1976; Kegl and Wilbur, 1976; Wilbur, 1976). As a result of the research on ASL and our belief that the results of Schlesinger's experiment could not possibly serve as proof either for or against a theory of language, we strongly doubt Wasow's claim that innate linguistic capacity is bound to the spoken modality.

2. There are other non-spoken systems which do not express grammatical relations.

Wasow's second point of evidence for the special nature of the spoken modality is his claim that any interference with the spoken medium may entail interference with general language structures. Consider the following statement (Wasow, 1973, p 51):

"Notice that this claim is more than mere wordplay. I am asserting that the use of the spoken medium is an integral element of the human linguistic structures and that interference with this element may well affect language in other ways which are not obviously connected. If the innate linguistic mechanisms of humans are as

complex as Chomsky suggests, then it is not surprising that modification in one aspect (in this case, the medium used) can have nontrivial repercussions (eg, the failure of grammatical relations)."

Why should shift of modality drastically alter a deep-seated linguistic universal? To assume that something innate could be modified by something as physical as a shift in modality seems a strange claim.

Chomsky (pc) has suggested that perhaps the innate linguistic mechanism is simply not triggered by a manual/visual modality of communication. Perhaps this type of communication is handled by other cognitive structures.

Even if we consider some triggering device necessary to activate this linguistic mechanism, why should we assume that this device is exposure to spoken language? Exposure to any systematic, abstract language, spoken or manual, seems to trigger the innate linguistic mechanism. If this is not the case, then why do children of deaf parents seem to acquire and master sign language at at least the same rate as hearing children acquire spoken languages (Bellugi and Klima, 1972; McIntire, 1974)? Why do these children far surpass deaf children of hearing parents in their command of English (Brasel and Quigley, 1975; Boyes-Braem, 1973; Lacy, 1972; Moores, 1974; Wilbur, 1976)? Our suggested explanation here is that the deaf child of deaf signing parents has been exposed to language (manual/visual) at an early age and can then easily learn English as a second language (a bilingual situation) -- for substantiation, see Charrow and Fletcher, 1973; Charrow and Wilbur, 1975. On the other hand, the deaf child of non-signing, hearing parents has been deprived of language prior to the age at which he begins formal schooling. English teaching in school assumes prior exposure to language and builds upon and refines generalizations which the child has already made about the nature of English syntax (Quigley, Wilbur, Power, Montanelli and Steinkamp, 1976). If the child comes in with no prior language generalizations, he is severely handicapped. This same child, when the opportunity arises, can pick up sign language as a result of natural, consistent exposure to its use by his peers on the playground or in any social environment (H. Schlesinger and Meadow, 1972). This exposure and mastery of sign language often aids in his academic progress with English (Brasel and Quigley, 1975).

One of the major arguments for a special innate linguistic mechanism is the rapidity with which a child learns language. This rapidity of learning, as well as various other linguistic properties, seem to be shared by both manual and spoken languages. There is no need to posit two separate, terribly similar mechanisms to handle them. Doing so would considerably weaken the claim that there is an innate language faculty.

Another argument of Wasow's is the obvious preference for the spoken modality, (Wasow, 1973, p 50):

"Hidden in this argument of Schlesinger's is the assumption that there is nothing special about the spoken

medium, that is, that if ISL is a natural language, then it would appear to be a coincidence that sign is the medium for language only among the deaf. That is, if natural languages may utilize either speech or sign, why, then, is there such a marked preference for speech? The natural medium for language is clearly speech, and, as a consequence, language transferred to another medium is not natural."

Our reply to this statement is that the spoken modality is not the only natural medium for language, but rather the most practical one. It is not a coincidence that sign language is the medium for language primarily among the deaf. First of all, the evolutionary development of man seems to have revolved around the freeing of the hands. The adoption or development of an oral form of communication would be advantageous in achieving this goal. Also, it is difficult to sign in the dark, at extreme distances, behind obstructions, or, to be more contemporary, over the telephone. The spoken/heard modality is a practical medium for the language of hearing people. The manual/visual modality is the practical medium of language for deaf people. The marked preference for speech can be correlated with the number of people for whom its use is more practical. However, in many cases when the situation dictates its use, even hearing people may use manual languages. For example, there was a widespread use of manual language among various groups of the Plains Indians of North America. It served as an extremely rich interlanguage between mutually unintelligible spoken languages (Mallery, 1881; Tomkins, 1969; Kegl and Nigrosh, 1975). Australia has many sign languages which are used in a variety of situations -- among the deaf, as interlanguages, and while observing taboos of silence in connection with certain initiations and rituals, (Meggiot, 1954; Mountford, 1938; West, 1963). In other words, it seems that Wasow has not proven that speech is somehow important in the definition of a natural language, but rather that it is often the practical choice as the modality for the expression of various languages.

What we have referred to as Wasow's second argument, the one linking grammatical relations to speech and therefore speech to natural languages includes evidence that there exist other non-spoken communicative systems that do not express grammatical relations. In light of our conclusions about Schlesinger's experiment, the word "other" is extremely questionable here. Wasow draws his supporting evidence from Rosemont (1972).

In this paper, Rosemont claims that classical Chinese developed independently from any spoken language and never existed in a spoken form. Rosemont makes the claim that grammatical relations, as well as several other proposed linguistic universals, are lacking in this language. Wasow (1973, p 51) summarizes the conclusion of this paper as follows:

"Rosemont's position is that since classical Chinese is not a spoken language, and since during its formative period it was strongly influenced by non-linguistic factors, there is no motivated reason to believe that it is a natural language. Thus he concludes, as we did above,

that a nonspoken 'language' cannot be used as a counterexample to a linguistic universal."

Wasow, by way of Rosemont,¹⁰ is taking a tremendous leap when he puts manual languages and written languages into the same category. He gives no justification for this assumption beyond the fact that this contrived similarity gives him a handy rug under which he can sweep supposed counterexamples to strong linguistic universals. What about the characteristics that signed and written systems do not share, such as rapid acquisition by children with no formal training or on the spot production and processing in conversation? Rosemont himself uses these properties to argue the naturalness of spoken languages. The validity of the claim that written communicative systems are not natural languages because they violate strong linguistic universals is irrelevant. We feel that the extension of this conclusion to all nonspoken languages is unjustified.

Furthermore, aside from the actual framework of Wasow's argumentation, there are several places where his premises are weak. These are crucial points in his overall argument.

If one accepted his line of reasoning, the fact that he might have proved that Israeli Sign Language is not a natural language does nothing further than claim that that particular communicative system cannot be considered a full-blown language. It cannot be extended to all other manual languages. Nor does a parenthetical remark such as the following reduce the existence of grammatical relations in other manual languages to sheer coincidence (Wasow, 1973, p 49):

"(although there is no reason to exclude the possibility that they might be expressed in some such system)."

The existence and surface realization of grammatical relations in American Sign Language is evidenced in many places in the literature (Friedman, 1976; Fischer, 1975; Kegl, 1976a, b; Wilbur, 1976). These stand as counterexamples to Wasow's general claims.

Another point that Wasow (1973, p 48) makes, is that "it seems most plausible that the expression of grammatical relations is a weak linguistic universal" except for one fact that cannot be accounted for.

"That is the fact that grammatical relations are expressed even when there is no possibility of ambiguity."

This is a very strong claim to make. Wasow is claiming that grammatical relations must be marked (either by word order or by inflection or separate marker) even when no ambiguity would arise if they were left unmarked. That is, languages have regular means of indicating grammatical relations whether or not there ever exists an ambiguity problem which would require some linguistic indication. In fact, in ASL, there are some cases in which this claim is violated. These violations can be found in sentences in which the verb does not incorporate subject and object or is non-directional.

Consider the following sentences:

1. BOY BELIEVE STORY (with or without body shift to agree with subj)
[The boy believes the story]
2. BOY STORY BELIEVE
[The boy believes the story]
3. STORY BOY BELIEVES
[The boy believes the story]
4. * STORY BELIEVE BOY
[The boy believes the story]

Sentences (2) and (3) are interpreted on the basis of selectional restrictions on the verb. In these cases there is no apparent overt marking of subject and object. (1) is the preferred form for these sentences. ASL has a relatively free word order. There are some constraints upon it as can be seen in the example (4) above. In verb final cases where the choice of subject and object is ambiguous a conjunctive reading is preferred.

5. BOY DOG SEE
[The boy and the dog see]

In cases where the verb is clearly transitive, the conjunctive reading is preferred and continuation of the sentence is expected. But an ambiguity is also tolerated on a second reading, favoring the initial NP.

6. DOG MONKEY BITE
 - a. [The dog and the monkey bite . . .]
 - b. [The dog bites the monkey]
 - c. [The monkey bites the dog]

In some cases the reading is reciprocal.

7. BOY LOVE GIRL
[The boy loves the girl]
8. GIRL LOVE BOY
[The girl loves the boy]
9. BOY GIRL LOVE
GIRL BOY LOVE
[The boy and the girl love each other]

It is important to remember that sentences in which there is no incorporation or agreement for subject and object marked on the verb are less common cases of ASL utterances. Usually there is much more going on

syntactically.

10. GIRL₁ INDEX₁ PRO₁ → HIT → PRO₂ BOY₂ (INDEX₂)
 girl - ref₁ pro -agree₁ -(S) hit - (0) - M pro₂ boy - (ref₂)
 [The girl hit the boy]

" → " marks a dominant vs non-dominant relation of the NP to the movement of the verb. These mark subject and object.

(Dominant) → V → (Nondominant)
 Subject Object

INDEX₁ indicates the relating of a referent (newly introduced) to a unique position in the signing space which will be used for purposes of agreement to make further references to the NP.

As Wasow (1973, p 52) points out, ISL and classical Chinese are supposedly in worse shape than ASL:

"In classical Chinese as in ISL, the difference between subject, direct object and indirect object is not expressible directly, but must be inferred from the context."

However, there exists a spoken language, namely Papago,¹¹ which has free word order and the same marking for subject and object. If, as Wasow claims, a language must explicitly mark the surface forms for differences in grammatical relations, even in cases which would allow no ambiguity anyway, then the following Papago sentences (12) and (13) would require special explanation:

11. Question: Sáacu ?at múa g ?ó?odham.
 [What did the man kill ?]
12. Answer : Júđum ?at múa g ?ó?odham.
 [The man killed the bear] (lit: bear-AUX-killed man)

If you simply said (13), you would have to probe very hard to get informants to give you the reading [The man killed the bear] even though the surface structures of sentences (12) and (13) are identical.

13. Júđum ?at múa g ?ó?odham.
 [The bear killed the man]

This language tolerates ambiguity and often depends on context to distinguish subject from object. This language would appear to constitute a counterexample to Wasow's argument.

Summary.

We have concentrated heavily on the methodological and theoretical flaws which we feel invalidate the claims of these three papers. Lest the reader infer that this represents an unnecessary vendetta on our part, we would like to briefly illustrate two other arguments which make universal claims on the basis of inappropriate use of sign language research.

Scholes (1974) attempts to relate cerebral dominance to the primary language modality (speech). As part of his argument, he states that "some humans fail to develop syntax even though they do develop cerebral dominance," (Scholes, 1974, p 3). This comment refers to the deaf, and his evidence that they fail to develop syntax comes primarily from Schlesinger (1970). The following quote from Scholes (1974, p 3) indicates exactly how carefully he read Schlesinger:

"Communication via International Sign Language (ISL) has been studied extensively by I.M. Schlesinger (Schlesinger 1970)," (emphasis ours).

Remember that Schlesinger's experiment concerned Israeli sign language

In order not to misinterpret Scholes, we would like to clarify the point of his paper with the following quote (Scholes, 1974, p 3):

"My claim would be not that signing is syntactically inadequate, but that the deaf communicator is syntactically inadequate."

The other argument is that of Rosemont (1974). He is concerned with the empiricist vs rationalist position on language acquisition and linguistic universals. He discusses Wasow's arguments and includes the following description of Schlesinger's work:

"Some work in Israeli sign language shows that it, too, lacks this grammatical feature. In a detailed experiment, two children who knew only signing were each given pictures; . . . what puzzled the investigator was that the first child could not communicate - without context - which picture he was viewing. In other words, Israeli sign language does not unambiguously distinguish grammatical relations such as subject-object."

Note that Schlesinger's three experiments were done with 30 people (whose ages are never mentioned, although discussion of political activities implies that they are adults) with varying competencies in Signed Hebrew and ISL. Many were bilingual. Even if the experiment had been done with only two children, as Rosemont claims, it is hard to see how such strong conclusions could be drawn based on such a limited number of subjects.

As Rosemont sees it, Schlesinger's argument has the following form: linguistic universals must hold for all languages, Israeli sign

language is a language but does not have grammatical relations, therefore grammatical relations cannot be universals. Rosemont contests the assumption that Israeli Sign Language is a language:

"If Israeli sign language is a language, grammatical relations cannot be linguistic universals. We then have the remarkable coincidence: all spoken languages express grammatical relations; two of the small number of non-spoken languages that have been studied (and there may well be others) do not express grammatical relations. If this coincidence seems to require explanation, the empiricist position on this point must be abandoned and a different account offered, namely that the natural medium for language is speech, and thus language transferred to another medium is not natural," (Rosemont, 1974, p 408).

It is an oversimplification on Rosemont's (and Wasow's) part to treat all non-spoken languages as though they constituted a single homogeneous class. Written languages are not used in conversational settings which require on-line processing for comprehension and production; they exist in some permanent form such that they can be reconsidered several times in cases of ambiguity. Sign language is used in the same type of conversational situations as spoken language, requires on-line processing, suggesting a possible fundamental distinction between it and written languages.

Rosemont's larger argument, that the rationalist position is to be preferred over the empiricist position, does not crucially depend on the argument related to Israeli Sign Language, and is thus not invalidated by the fact that the argument concerning ISL is unsupported. Nonetheless, it is disconcerting to see the same study being used (and misquoted) over and over without consideration of the possibility that there is some other explanation for Schlesinger's findings.

Conclusion.

We have critically reviewed several papers which demonstrated the effect of oral language bias on manual language research. In some cases, the researchers formed their expectations on the basis of oral languages familiar to them and then set out to fulfill those expectations with data from manual language. When their expectations were not met, they were confused and puzzled. Attempts to explain the discrepancies resulted in contradictory and confused explanations. Having once accepted the data as valid, they then used it to build broader arguments regarding linguistic universals.

We hope to have highlighted those areas in which oral language bias can do the most damage. We would like to stress several important

prerequisites to good sign language research:

1. Fluency in the language.
2. Thorough background in the sign language literature, and in the various disciplines involved in its analysis.
3. Heavy reliance on native users of the language, and data from natural conversational situations.
4. Careful experimental procedures with regard to
 - a. Choice of subjects;
 - b. Task variables;
 - c. Design;
 - d. Analysis;
 - e. Interpretation.
5. Presentation of enough information in writing to allow other researchers to duplicate the experiment.

These considerations will lead, we hope, to many more productive papers in sign language research and will eliminate the need for papers like this one.

Footnotes.

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² For a different approach to the issue of word order in ASL, see Fischer (1975).

³ Capital letters represent a broad transcription of a signed utterance.

⁴ This is Tervoort's term for a sentence which is ungrammatical within the sign language as opposed to the signed equivalent of the spoken language. In the latter case, the sentence may be said to be exoterically

ungrammatical if its word-for-word translation in the spoken language is ungrammatical.

5 Rosemont characterizes "classical" Chinese as a written language that was never spoken. In a variety of cases, structuralists have used classical Chinese as a counterexample to certain features of language (one being grammatical relations) claimed by transformationalists to be universal. Rosemont is attempting to eliminate this counterexample.

6 The frustrating aspect of reviewing this argument is the realization that at least on the basis of other research, on another sign language, namely ASL, one is led to believe that Schlesinger is mistakenly misrepresenting the facts in ISL. Research on ASL indicates the existence of grammatical relations. Oftentimes these relations are expressed through pronominal forms which incorporate into the verb, reflecting their grammatical relations as a function of dominance (subject) vs non-dominance (object) with respect to the verb. In some cases the relations are expressed in terms of word order.

7 The best evidence here would be a replication of Schlesinger's experiment with ISL. This has not as yet been done. The experiment has, however, been done with ASL (Bode, 1974) with extremely different results.

8 Wasow mentions the possibility that interference with the spoken medium could affect language. However, he doesn't really give strong evidence of the actuality of this situation. His only evidence comes from Schlesinger.

9 However, in the cases of the use of manual language by hearing people, either as an interlanguage or in connection with taboos on speaking, there is not much evidence that this is an independently arising language. These sign languages are generally learned late -- usually in adolescence.

We are not making any argument one way or the other. We are merely pointing out the fact that the relation of these languages to previously learned languages may be derivative. For a discussion of the influence of a previously learned language on a later learned sign language, see Woodward and Markowicz (1975).

10 Rosemont (1972, p 18) does make the following claim:

"Indeed, as long as linguists equate natural with spoken language, classical Chinese will not be an appropriate example for any linguistic theory regarding natural languages."

However, Rosemont never really discusses the manual language cases. All of his arguments support only the claim that written languages are not natural.

The one property that Rosemont frequently attributes to written languages is the ability to offer a series of readings on a given sentence. Written language is not transient. This property crucially distinguishes written from spoken language. If the meaning of a sentence is not immediately clear, it can be reanalyzed over and over again until an acceptable interpretation is achieved.

This would allow him to argue that classical Chinese is not a natural language without forcing him to include manual languages as well. This would seem a preferable approach to the problem since manual languages and written languages conflict in many important respects. Manual languages are learned without formal education. Written languages must be taught. The degree of dependence upon manual languages seems more comparable with that of spoken languages than with written ones. A person can easily survive with minimal literacy.

¹¹ We would like to thank Ken Hale for providing the data from Papago.

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