Linguistic Studies of Native Canada, ed. E. Cook and J. Kaye: A Review Article

ROBERT D. LEVINE

Linguistic Studies of Native Canada contains fifteen papers on various aspects of linguistic structure in a number of Canadian Native languages. Most of the essays are extremely informative and well written, although all are highly technical in nature and almost certainly inaccessible to the reader without linguistic training.

Linguistics, which in the Americanist tradition is an outgrowth of anthropology, is still nominally identified in many university bulletins as one of the four sub-fields of the latter discipline. In fact, however, linguistics has become almost incomprehensible to non-linguists, and even to many linguists who received their academic training prior to the mid-1960s. This increase in complexity reflects a fundamental change in research objectives within the field. As the papers in *Linguistic Studies of Native Canada* make clear, linguists are currently asking considerably more difficult questions of their data than they used to.

In order to put the essays in this volume in their proper context, it is essential to appreciate exactly what the linguist is trying to accomplish. Early grammars of Native North American languages were written almost exclusively by Christian missionaries for the benefit of other missionaries, so that the latter would be able to master the languages more quickly and thus proselytize more effectively. These missionary grammars tended to interpret the language according to the pedagogical structure of Latin grammars, with which the Catholic missionaries in particular were quite familiar. In the typical missionary grammar of a North American language, the Native tongue is displayed on a Procustean bed of paradigms, case-endings and the rest; elements which cannot be squeezed into the Latin mold are said to be meaningless, or are simply omitted. The notion that such languages might have structures of their own, and that these structures might have little or nothing to do with that of Latin, was utterly foreign to the missionary grammarians, and it is not until the Boasian era in North American anthropology that this view begins to guide linguistic research on Native languages.

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The massive *Handbook of American Indian Languages*, edited by Franz Boas (1911), contains a series of extended grammatical sketches of a large number of Native languages, each prepared by analytic methods designed to guarantee that

no attempt has been made to compare the forms of the Indian grammars with the grammars of English, Latin or even among themselves; but in each case the psychological groupings which are given depend entirely upon the *inner form of each language.* (Boas, 1911: p. 77; emphasis added)

In Boas' terms, the 'inner form' is the characteristic genius or structural plan of a language, and the 'pyschological groupings' represent classes of linguistic elements which function in parallel fashion. An example of such a grouping, using Boas' terminology, is the set of English auxiliaries *can, may, will* and *shall,* whose distributions are extremely similar. Furthermore, in the Boasian setting the linguist's job was recognized explicitly as the description of observed speech, not the establishment of prescriptive norms, so that when a linguist from this or a later period refers to rules of one kind or another, the term 'rule' does not identify a tenet of 'proper' usage. Rather, it denotes an observed regularity of pattern in the language in question.

The *Handbook* sketches represent for the most part an enormous improvement in both the accuracy and the insight with which Native languages were described, relative to the work of the preceding period. A later volume, undertaken on the initiative of Boas' great pupil Edward Sapir, which represents an even higher standard of excellence than the *Handbook* sketches, appeared under the title *Linguistic Structures of Native America,* to which the editors of the volume under review consciously referred in choosing a title for their own collection. Sapir had planned to dedicate the volume to his teacher. Boas, unfortunately, died shortly before it was published. In simple and moving terms, Leonard Bloomfield, another of Boas' students, summarized his mentor's contribution:

This volume, dedicated to . . . Franz Boas, was planned by Edward Sapir. . . . The dedication is as Sapir intended it, though now we must speak of memory. In our work we have thought of Franz Boas, the pioneer and master in the study of American languages and the teacher, in one or another sense, of us all. (Hoijer, 1944: p. 5)

The goal of describing languages 'in their own terms' became a fixed part of the ethos of Amerindian linguistic scholarship which passed from Boas to his two principal students Sapir and Bloomfield, and from these two to their own students and followers. It was these latter, third generation Boasians who in most cases oversaw the growth of linguistics in North America into a separate university discipline with departmental status.

Linguistics became increasingly formal, and linguists became increasingly meticulous in their elaboration of how, precisely, the categories corresponding to the 'inner form' of each language they worked on were to be defined and defended. Grammars were filled with various criteria for grouping words and parts of words into classes and stating the arrangements of these classes with respect to each other. Toward the end of the 1940s, it began to become evident to certain researchers, in particular Noam Chomsky, that a paradoxical situation had arisen: a set of highly specific and seemingly rigorously defined analytic procedures had evolved for linguistic description, while at the same time the languages being described were still theoretically supposed to be structurally sui generis. If each language were in principle grammatically autochthonous, what did it mean to say that certain formal procedures of analysis were required for all of them? As Robert Lees noted in a vigorously supportive review of Chomsky's epochal little book *Syntactic Structures*,

> when we compare a modern descriptive grammar with an old fashioned prescriptive grammar of a century ago, we are accustomed to dismiss the latter as unscientific, especially to the extent that it slavishly reproduces Latin and Greek grammatical categories in an effort to order the data of a non-classical language. But what more is our descriptive grammar than another reordering of the data — now, to be sure, according to a less traditional scheme of categories, but nonetheless according to an arbitrary set of descriptive labels which has become fossilized within linguistic description? Thus, without giving any internal linguistic justification, no reasons derived from a theory of language structure and behavior, the empirical data are organized in our descriptive grammar into chapters on Phonemes, Morphophonemics, Word-formation, The Noun, The Verb, Particles, and possibly Syntax, the whole intended from the very beginning to be just a classification of utterance fractions so that they may be successively mentioned from the first to the last page of the grammar in some manner other than randomly. (Lees, 1957: p. 377)

Even more serious, it appeared that the model of language on which these procedures were implicitly based was inherently unable to account for one of the most basic facts of language: that its users are able to construct sentences which they have never heard before, and to understand such sentences as well. The issue Chomsky and his followers raised may be stated as follows: if you collect a large but finite number of forms and apply various formal procedures to these data, you will wind up with a set of formulae which enable you to summarize your data conveniently. But if you insist that the formulae you arrive at correspond to the speaker's ability to create sentences and understand sentences, and to judge which are grammatical and which are not, then a convenient summary is thoroughly inadequate in itself. An analogy may be helpful: someone who has a butterfly collection can arrange the butterflies in his or her collection any way he/she wants to, on the basis of size, shape, colour, etc. The criteria which enter into the description of the physical structure of butterflies for such a collection will, however, almost certainly be insufficient for the purposes of an entomologist attempting to fit a certain butterfly into an evolutionary sequence based on current notions of speciation, natural selection and population genetics.

From the late fifties through the present, therefore, the linguist's perspective has gradually shifted. Researchers have increasingly attempted to devise grammatical models from which as many sentences of the language as possible can be predicted, or generated, from the operation of a relatively few rules. In stating these rules, the linguist employs a variety of formalisms, but unlike those of the preceding period, the formalisms used in current linguistic work are under constant review and evaluation in terms of how powerful they are. Since children are able, on the basis of their parents' and others' frequently incomplete, error-filled and ambiguous utterances, to which children listen, to become very capable speakers of the language by the age of six, in the view of Chomsky and his school it is entirely reasonable to suppose that human beings have some sort of innate linguistic capacity.

This capacity is conceived as a rather tightly organized filter of some sort which is able to 'strain' the defective utterances to which the growing child is exposed, and to derive from them the specific rules and patterns governing the parents' use of language, which then becomes the child's. The more powerful the descriptive device, the greater the number of hypothetical languages that can be constructed using the device. Since this outcome runs counter to the linguist's goal of providing a model of the language-learning capability so restricted that it would enable the child to arrive at the correct set of rules relatively quickly, linguists strive to use the least powerful set of devices possible which still permits an adequate account of the language.

The papers in *Linguistic Studies of Native Canada* generally reflect this shift in theoretical attitude. Many of the papers which discuss particular phenomena in a language attempt to evaluate certain descriptive conventions, devices and conditions which have been proposed elsewhere. For the most part the point of view taken in these articles involves an assessment of whether the phenomena under investigation support such devices or show them to be either too powerful or too restrictive. Linguistic facts are thus increasingly of interest in terms of their implications for the theoretical vocabulary used to state such facts.

Before considering some of the specific papers in the volume, it is important to explain briefly the critical notion of a derivation, which has guided most contemporary work in linguistics. Based on certain properties of syntactic behaviour, a linguist will assign a given sentence an abstract representation, and will apply a variety of rules to this representation which eventually transform it into the original sentence. Both the abstract representation and the rules which are applied to it must be independently justifiable, and both must have certain specific formal properties, depending upon which theoretical framework the linguist is working in. The process of deriving the sentence in question from its abstract 'ancestor' is referred to as a syntactic derivation. (A similar process involving the phonological form of words, and certain intonation features of sentences, is called a phonological derivation.)

A grossly oversimplified example of part of a syntactic derivation can be constructed as follows. Notice that in English we can say *What did John put away?* but not †*Did John put away?*[1] Furthermore, we can have indefinitely long sentences of the former types, such as *What did Mary hope that Tom would expect . . . that John had put away?* Verbs like *put* (*down*, *away*, *aside*, etc.) are inherently transitive; that is, they must have direct objects. Only in the constructions of the type we are considering here, and related ones, do we find 'gaps,' where such verbs have no direct object. One way of capturing the fact that *put*-type verbs, though inherently transitive, do not have a direct object in such constructions would be to suppose that, in the abstract structure we are setting up for such sentences, *put away* does in fact have a direct object, which for simplicity we can continue to refer to as *what*.

It can furthermore be shown that the structural description of English questions becomes unwieldy and without insight unless the abstract representation of questions contains the same word order corresponding to declarative sentences. A very rough approximation of the deep structure of our original question is then something like *John did put away what*; for the longer sort of sentence we would have *Mary did hope that Tom*

[1] A cross (†) identifies an ungrammatical sentence.

would expect . . . that John had put away what. In addition to this abstract structure, we need a rule which will move *what* indefinitely far from its original position leftward, and a second rule which inverts the subject and its auxiliary, e.g., *did* in this case. Evidence from other parts of English syntax indicates that the rule moving *what* to the front of the sentence must apply to the abstract representation first, and then the rule inverting subject and auxiliary word order applies. We now have an abstract representation and two rules applying in a specific order, with independent evidence for all of these elements of the analysis, giving rise to the sentence in question. In other words, we now have part of a syntactic derivation.

Even in this highly schematic description, certain points of considerable theoretical interest are involved. In the first place, the rule which moves *what* and other relative/question pronouns does not seem to be restricted in how 'far' it can move such pronouns to the left. However, there is considerable evidence from the syntax of English and other Indo-European languages, and also from some non-Indo-European languages, that rules which move (or remove) elements in derivations are frequently not open-ended in this way, but are rather more local. Some linguists believe that this localness is true of all such rules, and that the apparent unboundedness of the rule involving *what* is illusory.[2] Another point is that in Standard English people usually reject sentences like †*I wonder what did John put away* (with no break after *wonder*), and insist that the sentence be phrased *I wonder what John put away.* In other words, the rule inverting the position of auxiliary and sentence subject cannot apply just anywhere. This restriction means that we have situations in which a rule may be able to apply in a certain part of the derivation but not in another part. Now, a derivation does not represent a model of speech production; it is rather a formalized model of a speaker's knowledge of what is and is not grammatical in his or her language. However, our understanding of how language works is seriously affected by the answers to questions of whether or not rules which move elements, e.g., *what*, can move them indefinitely far, and whether or not it is valid to suppose that the application of such rules depends on where in the derivation the rule is supposed to apply. Depending on how we answer these questions, we will characterize the language learning 'filter' alluded

[2] Readers who wish to pursue these questions further will find a good introduction to the necessary background in Culicover (1976); key papers in this question of wh-movement are Chomsky (1973) and various papers in Culicover, Wasow and Akmajian (1977), especially Chomsky (1977) and Bresnan (1977).

to earlier in significantly different ways, because each combination of answers to such questions admits a certain class of structures as possible human languages and excludes others.

An interesting example of a formal device which is subject to an empirical test appears in *Linguistic Studies of Native Canada*, in Pat Shaw's essay "On restricting the power of global rules in phonology," using data from Dakota, a Siouan language. In Dakota, a rule exists which has the effect of altering the k-like sounds in the abstract phonological representation of words, when the sound following the k is a front vowel, that is, *i* or *e* (where these have continental pronunciation). Such rules are very common and represent a modification of one sound (k) under the influence of a following one, with the effect that the preceding sound assimilates in some respect to that which follows. In English, we have an example of this same sort of rule in the pairs *operate/operation*, *pollute/pollution*. However, things are not completely straightforward in Dakota, for while k sounds are modified so that they are pronounced like *ch* in *church* whenever they precede *i*, they only undergo this modification preceding *e* under special conditions. Shaw is able to show that the particular *e* vowels which induce this change in the 'abstract' k's are just those *e*'s which themselves have been created by a still earlier rule converting abstract *a* (corresponding to a vowel pronounced like the *a* in *father*) into *e*. Again, Shaw provides good documentation for all of these points. The problem, then, is that part of the rule which modifies the pronunciation of k in Dakota does not apply mechanically to a given form, for when k precedes *e*, the rule has to 'know' whether or not the *e* involved derives from *a* by an earlier rule, or is a 'bona fide' *e*, for in the latter case k will not undergo any change.

This is a highly undesirable situation, for a rule 'smart' enough to know the difference between a bona fide *e* and a derived *e* is an extremely powerful device, and as such is highly suspicious for reasons discussed above. More generally, one of the principal constraints which linguists have imposed on the operation of rules in derivations is that such rules must apply whenever they encounter a string of symbols which meet the conditions for their application; no rule may have access to information about whether or not a particular rule applied at an earlier point in a derivation. Rules which do have access to such information have been labelled global rules, and most linguists at present are convinced that no genuinely global rules exist. However, if one accepts the interpretation of the data that Shaw presents, it seems difficult to avoid the conclusion that Dakota does indeed have a global rule.

As it happens, there is an alternate interpretation possible. One of the virtues of Shaw's paper is that she clearly points out data which represent a complication or an obstacle for the hypothesis she advances; this straightforward acknowledgement of difficulties appears in a number of the other good papers in the volume. There is some reason to believe that the rule which affects k before *e*'s of the 'right' sort is a completely different phenomenon from the rule which affects k before *i*. The former would have to be global, while the latter is quite mechanical; furthermore, the former operates far more idiosyncratically than the latter — so much so that it is difficult for me, at any rate, to consider it an actual process, to be accounted for by a rule. Instead, we could describe the shift of k before *e* in the following terms: when a k (of the right type) precedes *a* (associated with forms of a certain restricted class in Dakota), the sequence *ka* as a whole becomes pronounced *ce*, where *c* represents the *ch* in *church*. Without going into detail, such an interpretation is quite tenable and eliminates the need for a global interpretation of the Dakota rule in question. The advantage gained is that, as noted above, we are thus able to reject the use of a grammatical device of almost unlimited power.

Two other papers in which a rule similar to the sort considered by Shaw plays a role are Glyne C. Piggott's "Some implications of Algonquian palatalization" and Jonathan Kaye's "Rule mitosis: the historical development of Algonquian palatalization." Both papers are devoted to a comparison of the operation of this rule in the Algonquian ancestral language (known as proto-Algonquian) with its operation in several daughter languages, especially Ojibwa. Proto-Algonquian seems to have had a rule which affected the proto-Algonquian sounds (marked by asterisks) *t and *θ when these sounds preceded *i* and related vowels, in very much the way that *i* affects k in Dakota. In both instances, the rule is said to *palatalize* the various consonants involved, because these consonants become articulated much closer to the centre of the mouth, in the area of the hard palate, than they usually are. Piggott shows that the rule responsible for palatalizing *t was, on the face of it, the same rule which palatalized *θ. However, in Ojibwa, which is descended from proto-Algonquian, it is possible to demonstrate that the rule which palatalizes the 'descendants' of *t must be a different one from that which palatalizes the 'descendants' of *θ. Piggott's argument here is based on the fact that, as noted earlier in the discussion of derivations, rules may apply to abstract forms in a specific order which, if altered in any way, gives rise to incorrect outputs. In modern Ojibwa, it seems quite clear that there is a rule involving vowel-loss which must follow the rule palatalizing the descendants of *t but which must precede the rule palatalizing the descendants of *θ. This means, of course, that the two palatalization rules must be stated separately from each other in modern Ojibwa, and therefore are distinct from each other.

Thus, if we assume that the proto-Algonquian palatalization process represented a single rule, the Ojibwa evidence forces us to conclude that what was originally a single rule has split into two parts. The alternative is to assume that proto-Algonquian also had two distinct palatalization rules, and that these merely give the appearance of having been unified. Piggott argues that the second description of the situation is in fact the more probable. In the first place, there is good evidence that in at least one more Central Algonquian language, Cree, there are also two palatalization rules, and thus, if the first alternative were correct, we would have to suppose that just by coincidence the same split happened in two of the daughter languages. What makes this suspicious is the fact — in itself a second strong piece of evidence for Piggott's point of view — that a split of that sort required represents a very unusual phenomenon. The actual formal devices by means of which such a rule would be stated of proto-Algonquian imply that it represents a unified process, and hence imply that any breakup of the rule into two parts would be a somewhat unnatural development.

Kaye's paper, which considers exactly the same problem as Piggott's, also presents a good deal of evidence for the fact that palatalization in Ojibwa involves two rules, but comes to the opposite conclusion. Since, as Kaye notes, "all the evidence that indicates that the Ojibwa [descendants of the proto-Algonquian palatalization] rule involves two processes can be shown to have developed in the post P[roto-] A[lgonquian] period," we have no actual evidence in our reconstruction of proto-Algonquian for the existence of two separate rules of this type. Kaye does not appear to consider the Cree evidence to support unambiguously the case for two separate proto-Algonquian rules. He concludes then that there has indeed been a rule split in a way which violates the formalism used to state the rule, and that we must therefore not rely purely on such devices to indicate what sort of historical changes are or are not natural. The issue would thus seem to be very much an open one.

A fourth paper which deals with palatalization rules, E.-D. Cook's "Palatalizations and related rules in Sarcee," is in a general way devoted to the same problem as in Piggott's and Kaye's papers, i.e., what can we infer about historical change in a language on the basis of the rules we employ to describe the language in its present form? Cook first considers a set of data in Sarcee, an Athapaskan language of western Alberta. There are three possible derivations which plausibly account for these data, which Cook considers in some detail. He then demonstrates that two of these derivations, each of which has some particular feature to commend it as a solution, violate the picture of Athapaskan historical development which we are able to establish on the basis of comparative Athapaskan evidence. Cook concludes that while the history of a language should not necessarily influence our descriptions of the language's structure at the moment, by the same token we cannot rely on notationally elegant or seemingly natural derivations to automatically give us insights into historical developments in a language. Cook considers various other implications of palatalization rules for Athapaskan prehistory, but these pertain to specific details of comparative Athapaskan phonology and need not concern us.

I have so far discussed papers devoted solely to phonological topics; for the most part the papers in *Linguistic Studies of Native Canada* are devoted to phonology. There are, however, several papers concerned with word formation and syntax. A particularly interesting example of syntactic work with an Amerindian language is Donald Frantz's "Copying from complements in Blackfoot." In Blackfoot, an Algonquian language spoken in an area immediately to the south of Sarcee-speaking territory, there are two types of sentences which will translate *I want my son to work*. The first would translate literally as *I want* [*my son might work*]; the second would translate *I want him* [*my son might work*]. The structure enclosed in brackets is generally referred to by linguists as the *complement* of the verb *want*, and the clause containing *want* is called the *matrix*. Frantz's point is that, in deriving the Blackfoot sentence corresponding to the second example above, with *him* as the object of *want*, it is necessary to regard the Blackfoot suffix corresponding to *him* as the result of a kind of copying process, in which the person (first singular, second plural and so on) of the subject of the complement sentence — *my* son, which is third person singular — is introduced into the matrix clause and appears as the object of *want*. In other words, *I want* [*my son might work*] corresponds to the abstract representation of the Blackfoot sentence, but a rule exists which allows us, as an option in the derivation, to copy the person of *my son* onto *want* in the form of an object suffix, corresponding to *him*.

Now, in English we encounter sentences of the form *I expected John to be here*. Certain linguists have analyzed this sort of sentence as having an abstract representation *I expected [John to be here]*, with *John* the subject of the infinitive within brackets. Since it is clear that in sentences like *I expected him to be here* the word following *expected* is the direct object of *expect*,[3] we must conclude that in *I expected John to be here*, *John* is a direct object of *expect*. Since, in this hypothesis, *John* starts out as the subject of the complement and winds up as the direct object of the matrix verb, there must be a rule which moves *John* out of the complement and into object position following the matrix verb *expect*. This process is called *Raising*, and looks very much like what seems to be going on in Frantz's Blackfoot examples discussed earlier.

One of the major points of Frantz's essay, however, is precisely that what we encounter in Blackfoot is *not* a Raising phenomenon. Some of Frantz's evidence is based on the fact that, as already illustrated in the English literal translation, forms like *my son* in the above examples remain in subject position in their complements; instead of such elements actually moving, an extra suffix is added to the matrix verb. The other piece of evidence is based on the fact that in languages all over the world it seems to be impossible for any element to be affected by a movement rule — a rule changing its position — when the element in question is part of a phrase consisting of forms linked by *and*, *or* or other conjunctions or their equivalents in other languages. However, if the rule in question is not a movement rule, but is instead a copying rule, the restriction just noted does not apply.

An example of this difference between movement and copying rules in English is the following: in certain dialects of American English, given an abstract representation like *John likes apples for breakfast*, we can move the object of the verb to the very front of the sentence as a means of emphasis, resulting in *Apples John likes for breakfast*. If, however, we have another item linked with *apples* by *and* in the abstract representation, as in *John likes apples and oranges for breakfast*, the rule moving *apples* cannot apply; no English speaker will accept †*Apples John likes and oranges for breakfast* (with no pause or break after *likes*). This is an example of the restriction noted in the preceding paragraph. However, if *apples* leaves some sort of copy of itself behind, such as the pronoun *them*, we get sentences which are quite generally accepted by speakers of this particular set of dialects: *Apples, John likes them and oranges for breakfast*. Thus, what is not possible for a pure movement rule is permitted for a rule which involves copying.

[3] Notice, for example, that we have *him*, the object pronoun not *he*, which only identifies subjects, in this sentence.

Precisely the same sort of pattern is found in Blackfoot: when the subject of the complement contains a series of items, linked by the Blackfoot equivalent of *and*, it is possible to copy a reference to a single one of these items into the matrix verb, in a manner identical to that illustrated above, as in the Blackfoot equivalent of *I want him* [*you and my son might work*]. Frantz argues that if what were going on in Blackfoot were Raising (which is a movement rule) rather than copying, the restriction we have seen illustrated above for English would prevent a reference to a single item from being moved out of a series. Since, as just noted, we do find instances of this latter possibility, we must conclude that copying, not Raising, is responsible.

Frantz's paper also discusses other aspects of Blackfoot syntax, but the core of his argument is contained in the line of reasoning I have outlined. It is quite persuasive on the whole, but there is an alternative explanation which deserves serious consideration. Perhaps, instead of having a reference to the complement subject copied onto the matrix verb, this 'copy' has been present all along; that is, it may be present in the abstract representation itself. Specifically, instead of deriving *I want him* [*my son might work*] from an abstract sentence *I want* [*my son might work*], we might have a separate abstract sentence *I want him* [*my son might work*]. Such a possibility actually has some serious empirical consequences which differ from those predicted by Frantz's analysis.

One very important prediction this alternative hypothesis makes is that we ought to be able to get sentences in Blackfoot of the form *I want (of) you* [*my son might work*]. Since there is no 'you' form in the complement, there is no source from which it could be copied to appear as the object of *want*. Thus, Frantz's analysis predicts that such sentences do *not* exist. Instances like this one illustrate the tremendous frustration one often encounters in doing syntactic research with languages of which one is not a native speaker and which have been little studied; for when we check the two very different predictions the two analyses in question make vis-à-vis what speakers of Blackfoot will and will not accept, we find that, as Frantz reports, some speakers do accept the Blackfoot equivalent of *I want (of) you* [*my son might work*] (in a context, for instance, where the person being spoken to has 'full authority over the son') while other speakers reject such sentences absolutely. In terms of currently held views on the nature of permissible syntactic derivations, the fact that some speakers will accept such sentences strongly implies that, for these speakers at least, the hypothesis offered as an alternative to Frantz's is probably the correct one.

Frantz's paper raises another issue, one which involves many of the other papers in the volume. Like Shaw, Frantz also appeals to global rules to handle certain kinds of problems, and in general presupposes a theoretical framework (called generative semantics) which virtually no linguists doing theoretical work (including, according to my impression, Frantz himself) employ any more. One finds other bits of largely obsolete theoretical apparatus elsewhere in this collection. The reason for this is that between the assembling of the manuscripts for the volume and their eventual publication as *Linguistic Studies of Native Canada*, something like six years intervened, due to the financial collapse of one of the original publishers and the difficulty in finding funds for a subvention to support publication elsewhere. Linguists are fortunate that the volume finally did appear, and owe considerable thanks to the editors for overseeing what must have been a particularly exasperating job in finally making these papers available. But the inevitable effect of the long delay was that those papers dealing with theoretical issues current at the time the papers were written were deprived of their opportunity to make a contribution to the ongoing debate which has been such a conspicuous part of the field for the past several decades.

Unfortunately, therefore, a number of papers which might well have had considerable impact had they appeared reasonably soon after being written will probably give the reader at the end of the 1970s a strong sense of déjà vu, and create a somewhat stale impression which, based on their intrinsic merits, they do not at all deserve. On the contrary, most of the papers in *Linguistic Studies of Native Canada* contain much valuable information and analysis, even when some of the theoretical principles they appeal to have been discarded, or at least placed in serious doubt. Both James Fidelholtz's "Micmac intransitive verb morphology" and Philip Davis and Ross Saunders' "Bella Coola Syntax" present very extensive treatments of the phenomena their titles announce. Fidelholtz's article contains a wealth of material on word formation in an Algonquian language, with a very detailed sketch of rules required in Micmac phonological derivations. Davis and Saunders offer a carefully developed interpretation of certain types of syntactic derivations in Bella Coola, a Salishan language of the British Columbia coast. Davis and Saunders' syntactic framework is interesting; it is what we might call a distribution of information approach. Sentences are assigned an abstract representation based on a distinction between parts of the sentence presenting 'old' information (information which the speaker and hearer in some sense share, which Davis and Saunders call the Topic), 'new' information (which is the statement the speaker is making about the Topic; such information is called the Comment), and what we may think of as peripheral information, called the Adjunct. Using this model, Davis and Saunders give a comprehensive account of certain phenomena which in English would be described as relative clauses. They rely on various rules of the sort we have considered to derive the sentences of Bella Coola from the abstract representations of these sentences; one rule in particular requires that two elements in different parts of several abstract representations be identical, in which case one of the elements is deleted. Use of this rule, however, seems to entail recourse to global rules later in the derivation, a device which, as noted several times in preceding discussion, is no longer considered legitimate in formulating hypotheses.

Furthermore, the deletion rule just mentioned has also come under very heavy fire during the past three or four years on the basis of English data; these data create problems of a sort which can be shown to exist in any language when two elements are required to be identical in order for a rule to apply, and when in addition an ungrammatical sentence will result *unless* the rule applies. Again, it must be made clear that both global rules and deletion rules of the sort considered were regarded by many linguists as perfectly justified at the time this paper was written. The vast quantity and range of data Davis and Saunders offer makes it possible for the enterprising reader to try to come up with alternative formulations which avoid the problems such deletion rules entail.

All of the papers I have discussed so far are exemplary, worthy of the widest circulation and professional discussion among linguists regardless of their areal or theoretical specializations. Regardless of whether or not they ultimately turn out to be 'right,' as the field judges such things, they genuinely illuminate some interesting feature(s) in the languages they cover. Unfortunately, not all the articles in the volume are up to this standard; the fact that so many papers are of good quality is actually somewhat unusual for a lengthy anthology. But it seems worthwhile discussing some of the problems with the less satisfactory contributions, since in some respects these problems will not be immediately obvious.

In Terry Klokeid's paper "Surface structure constraints and Nitinaht enclitics," there are two principal difficulties: on the one hand, a confusion between rules governing word formation and rules governing syntax, and, on the other, a seemingly unwarranted interpretation of Nitinaht sentence structure. Nitinaht is a Southern Wakashan language spoken on the west coast of Vancouver Island. Like other Wakashan languages, it expresses information about tense, the person of the sentence subject, etc., by means of an elaborate set of suffixes, which can be attached to the verb under certain specific conditions.

Klokeid's paper is concerned with specifying the conditions under which these suffixes can appear with respect to each other, but Klokeid does not consider the elements in question to be suffixes. As he observes with respect to earlier Wakashanists like Boas, Sapir, Haas and Swadesh, "these scholars based their distinction [between types of suffix] on purely phonological criteria, while the present study is concerned with syntax as well." Klokeid treats the Nitinaht suffixes in question rather as syntactically free elements which come to rest, as it were, at the end of a word and so give the appearance of being suffixes. Regardless of what Klokeid's paper is or is not concerned with, however, he presents no data whatever to support his notion that certain types of syntactic device are needed to correctly predict the form of Nitinaht verbs. Most linguists currently accept the idea that there is a serious distinction between the component of a grammar which is responsible for word formation and the component responsible for word permutation, and Klokeid fails to justify his assumption that in Nitinaht the latter component is what governs the distribution of the suffixes in question.

Elsewhere in the paper, Klokeid assumes the existence of a separate rule which moves 'prepositions,' as he labels a certain class of forms, into the front position in sentences, along with the 'nominal' which follows them. But in such cases it is quite clear that the so-called prepositions are in fact verbs, and the nominals which follow them are the subjects of the verbs. Much of the argumentation Klokeid provides to show the superiority of his formulation to other conceivable alternatives is more or less beside the point, but in this instance a serious error would seem to be involved: sentences which consist of two separate clauses are in effect described as though they contained only a single clause. Since the sentences in question do contain two clauses, either clause can appear at the front of the sentence in the abstract representation, and hence no movement rule of any sort is required for such cases. Elsewhere, Klokeid comments that Sapir and Swadesh (1939) "erroneously refer to as 'Nootka'" the Tseshaht band north along the west coast from the Nitinaht. It would seem that Klokeid is unfamiliar with the introduction to the volume he cites, in which the authors clearly state that

> the term "Nootka" is somewhat of a misnomer. It is locally used only of the Indians of Nootka Sound but in ethnological literature it has been extended to cover a number of culturally and linguistically related tribes living on the west coast of Vancouver Island.... The particular dialect illustrated in this

volume is that of the Indians of Barkeley Sound and Alberni Canal. (Sapir and Swadesh 1939: p. 10)

Given the fact that Sapir and Swadesh were following the nomenclature current at the time, and that they duly informed their readers of the actual situation covered by this nomenclature, it is difficult to understand how their usage could possibly be described as 'erroneous.'

The problems with Th. R. Hofmann's paper, "Equational sentence structure in Eskimo," are somewhat different. Much of Hofmann's material is genuinely interesting and important; what mars his article is its turgid and frequently unclear writing. The gist of Hofmann's paper is that in Eskimo, the abstract representations of sentences consist of essentially unordered collocations of words, all of which are either predicative or potentially predicative. For a given abstract representation, then, the elements in that abstract representation can appear in all possible word orders, with no actual movement rules involved. At the time Hofmann wrote this paper, such languages had not made much of an impression on linguistic theory; nonetheless, Hofmann's claim that "if half the arguments I have presented about Eskimo stand, then syntactic theory as it presently stands is seriously inadequate" is a little strong, since current linguistic theory has, without undue discontinuity, been able to incorporate models for describing such languages insightfully.[4]

The principal difficulty in this paper is simply that it is very difficult to read. Hofmann attempts to develop a comparison between the structure of Eskimo sentences and the structure of algebraic equations of the form $A=B=C$, but the analogy is rather laboured and does not really aid in presentation of the purely linguistic argument; it is actually often distracting.

Another serious distraction to the reader is Hofmann's use of '&' in place of 'and,' and '1' in place of 'one,' '2' in place of 'two' and so on, and his spelling of 'another' as 'an other.' These orthographic idiosyncracies are Hofmann's own; the editors explain in a footnote that they are leaving them intact at Hofmann's request, and attempt to explain Hofmann's motivations in using them — "he argues that there is no advantage for an international publication language to follow all the idiosyncrasies of a spoken idiom, & that written English is more useful as an international language with the adoption of these pasigraphic symbols." This justification makes no sense to me at all; the fact is that in a number of places Hofmann's peculiar conventions interrupt the flow of

[4] See, for example, K. Hale, L. M. Jeanne and P. Platero (1977) for some discussion of such languages from the theoretical viewpoint.

reading quite unnecessarily and in a most irritating way, with no compensating benefit that I can see. My own belief is that in such cases the editors have a responsibility to the reader to ensure that the latter's burden in assimilating unfamiliar and complex content is not aggravated by having to cope with authors' personal abbreviatory preferences.

With these exceptions — and it must be said again that Hofmann's paper does have much interesting content, though the manner in which this content is presented tends to make it much more inaccessible than necessary — the papers in *Linguistic Studies of Native Canada* are thoroughly lucid and rewarding. This is true of J. K. Chambers' paper "Dakota accent," James Hoard's "Obstruant voicing in Gitksan," J. Massenet's "Une conspiration en Eskimo," David Pentland's beautiful exercise in Amerindian philology, "Proto-Algonquian *sk in Woods Cree," Gregory Thompson's "The origin of Blackfoot geminate stops and nasals" and Christopher Wolfart's "How many obviatives: sense and reference in a Cree verb paradigm," which space limitations prevent me from discussing; they are fine examples of the linguist's craft. Linguists have reason to hope that similar anthologies will continue to appear in the future, for scholarship needs them: they provide not only a good deal of mutually illuminating pieces of information, but a convenient summary of the state of the art at a given moment in the field's history.

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