A New Perspective on American Indian Linguistics [1]

KENNETH HALE
Massachusetts Institute of Technology

Appendix by
ALBERT ALVAREZ
Sells, Arizona

INTRODUCTION

Before I begin my discussion, I must point out that it is impossible for me to limit it to Pueblo languages or even to concentrate on them to any great extent. Indeed, if I am to have anything to say which is at all significant, I will have to draw very heavily from my recent experience with a non-Pueblo language—Papago of southern Arizona. Nonetheless, the import of what I have to say is as pertinent to the study of Pueblo languages as to any other. And, in fact, I think it is crucial to the study of Pueblo languages.

The point of view which will be central in this paper is that the future of American Indian linguistics (i.e., the extent to which it will advance significantly) will depend critically on how successful an effort there is to engage American Indians in the active study of their own languages—not as informants as in the past, but as linguists, philologists, lexicographers, creative writers, and the like. To put it another way, significant advances in the study of American Indian languages can be made, in

my judgment, only when a significant portion of the field is in the hands of native speakers of the languages concerned.

My position in this regard stems directly from my belief that the study of a language is essentially the study of the total range of linguistic knowledge which the fluent speaker has about the sentences of his language and that the proper data of linguistics are the kinds of knowledge (intuitions, if you like) which enable the fluent speaker to understand and create novel sentences belonging to his language. This view has been discussed at great length in the linguistic literature (e.g., Chomsky 1964), and I will not dwell on it here. In any event, it seems to me to follow from this view that the extent to which a linguist, grammarian, or what have you, can be successful in describing a given language will depend critically on his knowledge of it. That is to say, if the data are linguistic knowledge and if one cannot describe data that one does not have, it follows that the success of a linguistic description will depend on the linguistic knowledge which the describer possesses concerning the sentences of the language under investigation. It is just this dependency which has led many linguists to believe that an adequate description of a language can be made only by a grammarian whose knowledge of the language is essentially that of a native speaker.

To validate the point of this chapter, it is sufficient to accept the fact that, in the vast majority of cases, the people who know American Indian languages *best* are American Indians themselves. The fact is, however, that the point can be made much more strongly: with a very few (largely anecdotal) exceptions, the *only* people who possess a knowledge even remotely approaching that required for an adequate description of an American Indian language are people whose first language is an American Indian language. The condition which prevails in this country, nonetheless, and which must be changed, is that native speakers by and large are not the ones who are directly involved in the professions relating to the study of American Indian languages.

The new perspective mentioned in the title of this paper is not really new. There have been instances in the past when American Indians have written extensively about their own languages. I would like to see this be the prevailing state of affairs in the American Indian linguistics of the future, and I feel strongly that our future efforts should be directed toward this end—provided, of course, that Indians themselves agree that this is desirable. My relevant experience in this connection has not been

with a Pueblo language, but I see no linguistic reason why what I have to say cannot be applied in the Pueblo case. In what follows, I will state in rather general terms the role of a native speaker's knowledge in linguistic research, then, in more specific terms, the role of American Indians in linguistics and the work which Albert Alvarez and I did on Papago. Following this, I will attempt to relate the discussion to the concerns of Pueblo linguistics. Finally, I will include an Appendix containing excerpts from the work of Albert Alvarez on Papago.[2]

PREREQUISITES TO ADEQUATE LINGUISTIC DESCRIPTIONS

A view which can hardly be questioned, it seems to me, is that a grammarian who hopes to provide an adequate description of a language must have what amounts to native command of it; that is, what a grammarian knows about the sentences of the language he describes should agree in all essential respects with what a native speaker knows. But as matters now stand, linguists who work on American Indian languages begin their study as adults. Quite apart from the fact that the non-Indian linguist's exposure to the language begins too late, it is extremely unlikely that his exposure will ever be of a type that will enable him to approach even remotely native command of the language. It is quite true that the linguist is equipped with a battery of techniques which are designed to speed up the acquisition of a new language. But a moment's reflection will reveal that this is as much a drawback as it is a help. No set of techniques will ever be the right set—any list of questions to ask in the field will always be too short. The reason for this is as clear as it is important in its implications for linguistics. The point is, we do not know what the universals of language are. We might illustrate the point by means of an example. In English, we form questions of the type represented by "which man did you see?" by moving a questioned constituent (i.e., "which man") to the front of the sentence (in this example, it is fronted from object position, i.e., the position following "see"; compare "you saw that man," and "you saw which man?" in which the object is still, so to speak, in its "original" position). Now we notice that a questioned constituent can be fronted in this way from a considerable variety of positions: the subject of a subordinate clause is fronted in "which man do you think

will come?" (cf., "you think which man will come?"), the object of a subordinate clause is fronted in "which man do you think you will see?" (cf., "you think you will see which man?"), and the object of a preposition in "which man did you come with?" (cf., "you came with which man?"). But it is not the case that a questioned constituent can be fronted from *any* arbitrary position. Thus, while there are English sentences like "you saw the man who shot that deer" and even "you saw the man who shot which deer?" the following is ungrammatical in English: "*which deer did you see the man that shot?" A questioned element cannot be fronted from within a relative clause. In other words, the rule of English grammar which effects the fronting of a questioned constituent must be constrained in a particular way. What we do not know is whether this constraint is universal. Now, to know English is to know, among thousands of other things, that this constraint (or a more general one under which it is subsumed) exists. This is certainly so, since every native speaker of English can recognize the ungrammaticality of the sentence cited above. It may well be that to know *any* natural language is to know this constraint, but we do not know that much about languages yet. If the constraint *could* be shown to be universal, we would be that much closer to a general theory of human language, i.e., we would, to that extent, be able to restrict the theory which characterizes the notion "possible human language." It is rather important, therefore, to know whether or not a particular language we are studying relaxes this constraint. Now consider the linguist who is learning a totally unfamiliar language, and suppose, for the moment, that the language he is studying relaxes this particular constraint under some or all conditions. Bear in mind that to know this hypothetical language is to know, among other things, that it has well-formed sentences which are essentially of the form represented by English "*which deer did you see the man that shot" and "*who did you see the snake that bit?" The question is: How would the linguist ever learn that fact about the language? The answer is: by accident or not at all. Except in the extremely unlikely event that he had precisely *that* question on his list. But if his list included that question, would it also include, say, the question as to whether the language has sentences like "he likes the horse that John was saddling," in which, as is *not* the case in English, "John" and the pronoun "he" refer to the same person? The fact is, we do not know enough about language in general to know

what questions to ask in attempting to learn all of the essential facts about any given new one.

This being the case, there is an unknown quantity of crucial data for any given language which the linguist, who is not also a native speaker can learn only by *accident*, e.g., by noticing them in texts, in side remarks on the part of an informant, or the like. By definition, however, these very data form a part of the native speaker's knowledge of his own language. It is not at all unreasonable, therefore, to assert that the native speaker has the competence which is the sine qua non for a satisfactory linguistic description of his language.

It is, of course, quite correct to point out that a native speaker's command is not a *sufficient* prerequisite to satisfactory linguistic descriptions. It must be combined with interest and skill in linguistic analysis; people who describe languages must, in some sense, be linguists. This presents a dilemma: for the majority of the world's languages, there are no native-speaking grammarians.

Traditionally, a solution to this dilemma has been sought in the pairing of trained linguists with native-speaking informants to form teams of which one party has all the questions and the other has all the answers. I have given the basic reason why I believe that this is not the correct solution—the fact that we simply do not know all the questions; we do not know, in effect, what a linguist should do when he works with an informant to insure that he obtains the data relevant to the native speaker's knowledge of his own language. In attempting to describe what a fieldworker does in the field, one is forced, in the final analysis, to resort to some statement like "he tries to learn the language by hook or by crook." That is, one is forced eventually to admit that a lot of the data which one obtains about a language in the field is obtained accidentally rather than by the application of some method.

To be a linguist means to have significantly more consciously formulable and theoretically relevant questions about language than the layman has. It also entails belonging to a profession that is constantly generating new questions which are disseminated among its practitioners. The linguist is expected to find out the answers to these questions for the language he is investigating and to advance the field by discovering new questions. If the language he is studying is not his own, his field method consists of the body of techniques used in getting the answers to known questions from his informants. Beyond that, he has no method

at all, aside from the unstructured one of simply immersing himself totally in the language.

Consider again the linguist working on a language, not his own, which permits questioning out of a relative clause (i.e., has well-formed sentences corresponding to "*who did you see the dog that bit"). Suppose he discovered this fact on his most recent field trip. And suppose he later learns, either on his own or by reading in the current literature (e.g., Chomsky 1964:72–73; Ross 1967), that the constraint which operates in English to prevent questioning out of relative clauses might well be a special case of a more general constraint which also prevents relativization out of relative clauses accounting for the ungrammaticality of sentences like "*I know the man that you saw the dog that bit." He then seeks, perhaps successfully, perhaps in vain, for sentences in his primary data which would show whether or not the language he is studying relaxes the more general constraint. The point of this illustration is that, unless the linguist had the *general* constraint in mind while he was in the field, he will not automatically have the answer to the more general question. And this is exactly what usually happens; no fieldworker I know of will assert that his data does not contain gaps. In fact, all will surely admit that the number of gaps grows almost in direct proportion to subsequent advances in linguistic theory. Unless the fieldworker gets to the point where he believes he can fill the gaps from his own knowledge (i.e., he is essentially a native speaker), he will be forced to return to the field again and again to fill these gaps. And who among us will ever claim a native speaker's command of an American Indian language which he learned as an adult? It goes without saying that this problem is eliminated if the linguist and native speaker are the same person.

I should perhaps make it clear at this point that I do not regard the fruits of past and present fieldwork as useless; many very important contributions to linguistic theory have depended on certain classic products of fieldwork. This is not at all inconsistent with the thrust of the argument I have been attempting to make, however. A substantial amount has been learned about American Indian languages almost entirely through the efforts of linguists working in the field. The knowledge which has been gained in this way and the personnel which has been trained in the process provide a rather secure foundation from which great advances can be made. What I question, however, is

whether it is possible to advance if we continue to operate solely within what has become the traditional framework for the study of American Indian languages. The purely logistic considerations seem to be against it.

AMERICAN INDIANS IN LINGUISTICS

While I would not like to argue that all fieldwork of the traditional type should stop, I would like to argue that those of us whose interests are primarily in American Indian linguistics should address ourselves to the problem of enabling native speakers to study their languages and further that this should be a concern of highest priority. We should work toward a conversion from the traditional method of pairing linguists with informants to a method in which the dual requisites of linguistic training and native command reside in a single individual. This, in my opinion, is the proper solution to the logistics problem outlined above. It is perhaps not irrelevant to point out that there are reasons even more compelling than the purely linguistic ones for why this conversion should take place. Those of us who study American Indian languages feel highly honored and privileged to be able to do so; if American Indian linguistics were in the hands of American Indians themselves, the honor would accrue to those to whom it properly belongs.

With this goal in mind, it would make a great deal of sense for each linguistics department in which the study of American Indian languages is a major interest to actively recruit students from Indian communities and to develop programs which would not only provide training in general linguistics, but would also be directly relevant to the study of an American Indian language by native speakers. Such students, it is envisioned, would receive degrees in linguistics in the usual way; they would, in many cases, go on to be teachers of linguistics, and we could look forward to a truly constructive and exciting tradition of American Indian linguistics. This possibility is as real as any I can think of and should be seriously pursued. However, there is at least one very strong reason why I would argue that it is not enough; it is not the only possibility and should not be the sole avenue to the desired goal. If we limit ourselves to this one course of action, we will fail, in my opinion,

to engage the talent which is in many ways the richest. There are many people who have the potential we are seeking but for whom it simply does not make sense to enter into a traditional academic program that imposes conditions and requirements which are essentially irrelevant to the contribution a person can make by working on and in his own language. Consider, for example, a member of an American Indian community, who because of age, limited background in Anglo-American education, limited knowledge of English, or what have you, cannot or does not wish to enter a university program leading to a degree. Provided this person has the relevant potential and is interested in a serious study of his own language, I fail to see any reason whatsoever why he should not be enabled to engage in that study—as his life's work, if he so desires. That is, I do not see any necessary connection between the attainments which we have come to regard as requisite in academic life and the potential which a person might have for doing truly significant work on his language. In fact, I would strongly urge reconsideration of the prevailing tradition according to which such intellectual endeavors as linguistic research, among others, are limited almost entirely to the academy. In American Indian linguistics there are some rather splendid examples which support this point of view—to cite one example, the work done by Robert Young and William Morgan on Navajo (Young and Morgan 1943); this is a classic, in my estimation, and there are others which have been produced, especially in earlier periods of our history, by persons who were operating in a framework other than an academic one. It appears to me that a lot can be said for the opinion that the greatest potential for advancement in the field of American Indian linguistics lies outside the traditional academic framework and that it is to be sought rather in the group of individuals who are fluent and sensitive native speakers of the languages. To put it another way, there exists a group of potential linguistic scholars who are the natural heirs to the tradition of American Indian linguistics; moreover, the extent to which this group intersects with the group of potential academics in the Anglo-American sense is, in all probability, accidental. Most, if not all, of us who have done fieldwork on American Indian languages have known individuals of the type I am alluding to; they have been our informants in many cases. They have ranged from monolinguals to Ph.D. candidates, and their position in this range has had little if anything to do with their excellence.

What all of this suggests to me is that we would be most fruitfully engaged if we devoted a considerable amount of our effort in the future toward developing the natural class of potential American Indian scholars. This involves two very large considerations: (1) appropriate training in linguistics, and (2) careers which make use of the training. I will assume that neither of these considerations poses any serious problem for those individuals who might enter ordinary degree programs. I will be concerned in what follows with the individual who has the ability and desire to work on his language but who might not, for one reason or another, obtain an academic degree. I will address myself primarily to the first consideration, appropriate training. However, the problem of careers is by far the more urgent one and must, I feel, be squarely faced in each instance. I have discussed this very briefly elsewhere (Hale 1969); my feeling now is that the solution lies in American Indian communities themselves. Many communities feel a need for language experts; this is true in some of the pueblos, I understand. And the need is growing. It seems to me that the suggestion which this paper attempts to develop is a reasonable response to this need.

The possibility I would like to suggest here is one in which the expertise and personnel which now exist in American Indian linguistics have a critical role to play. That is, it is one which builds directly on our past experience and findings. The suggestion is this: that each linguist who has worked intensively on an American Indian language, where feasible, undertake to train one or more speakers of that language to engage in linguistic work on it. This suggestion is certainly not new; it occurs to us all of the time, I am sure. But we may have had an overly restrictive interpretation of the feasibility clause—e.g., to do linguistics one must have traditional academic training and a degree. There are, of course, real feasibility considerations: career possibilities for the trainee, age, number of speakers of the language involved, the problem of funding, and so on. But, as I have said, provided a prospective trainee is gifted in the relevant way, there is a lot to be gained by dropping the basically irrelevant academic requirements; I think we can go very far indeed if we do that.

I turn now to the question of how appropriate training might be provided in such cases. This will, without question, vary with the individuals involved. So what I have to say can only be regarded as suggestive. In fact, I will have to limit my discussion to a single experi-

ence, since I have had only one which is relevant. More accurately, I will describe an experience which Mr. Albert Alvarez and I had together, an eight-month partnership (February-September 1969) in which he taught me about Papago and I taught him about linguistics.

A LINGUISTIC PARTNERSHIP

I met Mr. Alvarez in 1963 when he came to the University of Illinois with Dr. Dean Saxton, a linguist also working on Papago. Mr. Alvarez served as linguistic informant in a field methods course which I taught in the anthropology department there, and his talent emerged rather quickly in that context (Hale 1965, 1969). I worked with him intermittently after that until February 1969 when he came to M.I.T. to serve as my assistant in a field methods course and to begin learning how to do linguistic work himself. During the eight months he was at M.I.T., we worked toward the general goal of enabling him to begin writing about his language.

During the first four months we worked primarily on phonology, and during the ensuing four primarily on syntax. Mr. Alvarez felt that he expressed himself best in Papago, so he did most of his writing in that language; he wrote three longish essays (ranging from thirty to forty typed pages each), one on Papago phonemics, one on the meanings of the Papago tenses and aspects, and another on Papago sentence types. In addition, he wrote a number of short essays (ranging in length from a half-page to several pages) on ways in which certain superficially similar sentences contrast in meaning. These essays were translated in order to make them available to a wider audience. Initially, I did the bulk of the translating, primarily so that Mr. Alvarez could continue writing new material in Papago. Later, we realized that it would be helpful to him if he translated some of the short essays and I edited his translations—I now know that we should have done this all along, not only because of the help it was to him, but also because of the fact that a lot was lost in my translations.

Our method of operation had two rather separate aspects, one relating to the essays and another relating to the formal aspects of linguistics. Initially I did not perceive these as different; but they grew to be different and in ways which are highly suggestive. The formal aspects of our procedure consisted of a series of lectures which I prepared relating

specifically to questions of Papago phonology and syntax; we arranged to meet for at least two three-hour sessions per week during the first four months and for three somewhat longer sessions per week during the ensuing four. During these sessions, I delivered a lecture (in English, but using Papago examples), we discussed the material, and Mr. Alvarez took notes. My preconceived plan was that Mr. Alvarez would not begin writing on his own until we had gone through a considerable amount of lecture material. It became obvious to us almost immediately, however, that it would make a great deal of sense if, as homework, Mr. Alvarez took a particular problem we had discussed in a lecture and wrote it up. This would mean that he could begin immediately to write about his language and to face the problem of developing an appropriate linguistic terminology for it. We decided to proceed in that way, and the first topic Mr. Alvarez wrote about was the distinction between tense and lax stops in Papago. It required a great deal of rewriting, amplification, and discussion before either of us was satisfied with it, and the same is true of most subsequent ones. The writing of essays presented a set of problems which was almost entirely separate from the formal aspects of linguistics dealt with in the lectures; perhaps a primary problem was that of choosing an appropriate set of terms to refer to linguistic entities (e.g., "consonant," "vowel," and later, "transitive verb," "stative sentence," and so on). Actually this problem turned into a blessing. Mr. Alvarez's decision to compose his essays in Papago forced us to be very clear in our thinking about certain facts and to pick a terminology accordingly. We left the actual job of creating phonological and grammatical terms entirely up to Mr. Alvarez; he felt very strongly that a linguistic term should not be a conventional label, to be learned along with a concept, but rather a coinage which, in part at least, defines the concept to which it refers. This forced me to be extremely explicit in explaining concepts to Mr. Alvarez and, at the same time, gave him an opportunity to disagree about the relevance of a particular concept to a description of Papago. This effort on both our parts has taught me more about Papago than I could ever have learned otherwise.

In any event, the terminology problem had an interesting effect on the way in which the essays were written—specifically, on the *order* in which they were written. It was possible for Mr. Alvarez to write with considerable fluency about the tense-lax distinction in stops long before

he was able to discuss the general class of sounds to which the stops belong, simply because a term equivalent to "stop consonant," ʔí:bhei kúkpadam (breath stopper), suggested itself immediately, while an appropriate term and an appropriate explication on my part for the class of consonants, ʔí:bhei ṣó:bida (breath interception), emerged very slowly. This was the typical situation throughout our work together, and the essay-writing aspect of the program diverged from the lecture program to such an extent that when we got into syntax and semantics, Mr. Alvarez was often writing about topics far beyond what I could have hoped to prepare a coherent lecture about. In fact, his short essays and his longish essay on sentence types made suggestions about Papago which I had no way in the world of anticipating.

The series of lectures which I gave to Mr. Alvarez covered taxonomic phonemics (pretty well represented in the essay he wrote on Papago phonology) and an introduction to Papago syntax (the simple sentence, abstract and surface representations of simple intransitive and transitive sentences, and some fifteen transformational rules together with the arguments motivating them). This second series of lectures is only indirectly represented in the essays which Mr. Alvarez wrote relating to syntax and semantics, partly because of the natural lag occasioned by the terminology problem but also, and more importantly, because of the fact that there were topics which could be written about in comparative independence from the aspects of linguistics covered in the lectures. I feel that it would be more profitable in this paper to discuss the essays rather than the material presented in my lectures, which were essentially what one would find in an introductory linguistics course, save for the fact that they were related specifically to Papago.

At the beginning, I suggested the essay topics. Toward the end of our work together, Mr. Alvarez began to suggest new topics and extensions of earlier ones. In either case, our first step in a given instance was to discuss the question of whether the topic was worthy of an essay (whether it was in some sense a real topic) and, if so, what its ramifications were and how one might go about writing it up. Mr. Alvarez then wrote a first draft in Papago which we discussed again for possible revision and editing. After the final draft was typed, the essay was translated.

Except in the case of those on phonology, Mr. Alvarez's essays are rather different in nature from the kinds of material one sees in

current articles in linguistics; they resemble more closely the kind of commentary found in traditional grammars written by native speakers who pay close attention to the meanings of sentences. As we proceeded I became firmly convinced that exactly this type of commentary is what is sorely needed in American Indian linguistics; we have virtually none of it at present. And it is not at all unreasonable to argue that much of our ability to advance in the study of the better-known languages of the world (particularly Indo-European languages) is due in very large part to the existence of sensitive and extensive commentary on the meanings associated with grammatical categories. I am greatly impressed, for example, by such sensitive works as Bello's grammar of Spanish, written over a century ago, but often reprinted (Bello 1964; cf. also Alvarez and Hale 1969). We would do well to aspire to such works; if we attempt to do so in American Indian linguistics, the role of the native-speaking grammarian will be critical. It is in this light that essays like those which Mr. Alvarez began to write are of great importance and might well provide a central focus in training programs or partnerships of the type suggested here. They also provide a firm basis for teaching the more formal aspects of linguistics; thus, for example, a formal discussion of the way in which tense and aspect are represented in the grammar of a particular language benefits greatly from a prior essay which discusses the tense-aspect distinctions made in the language, what these categories mean, how they are used, and the like. This is particularly appropriate where the student who will later be exposed to the formal discussion is himself the one who writes the prior essay.

Mr. Alvarez's informal linguistic essays, then, became the prominent feature of our work together—a development which, although previously unanticipated, suggests a highly fruitful procedure for future programs of this sort. The writing of such essays is of unquestionable value, perhaps a prerequisite, in the teaching of formal linguistics; furthermore, it may well be the key to further advancement in American Indian linguistics.

It might be well to discuss briefly some of the benefits which accrue to linguistics itself—or, more specifically, the linguistic study of a particular language—from such a procedure.

The mere act of writing in a language not previously written to any great extent occasions discoveries of considerable importance, especially where the writer is attuned to linguistic considerations. When Mr.

Alvarez began writing a great deal, he began to notice phonetic inadequacies in the notation generally accepted for Papago. In some cases these were simply accidental misspellings that had survived despite the work of a variety of linguists studying Papago over the years. But on one occasion he noticed an inadequacy of a more serious nature, one which indicates that there is a phonemic distinction in the language which we have been missing altogether. For example, in the word /kókda/ (to kill, plural object), the segment /d/, which we had assumed all along was simply that, is what we might call, for lack of a better term, "emphatic" (not to be confused with tense—see below). We do not yet know what this feature is, but whatever it is, it is phonemic (in the taxonomic sense at least)—the emphatic /d/ of /kókda/ is different from the nonemphatic /d/ in /wópda/ (boots), for example—and it is pervasive in the stop system, as far as we can tell. In this instance, as was quite generally the case, the essay-writing procedure generated a topic for further inquiry. Discoveries made by Mr. Alvarez as a result of writing extensively in his language were not limited to phonology; the final item in the Appendix was written by Mr. Alvarez when he noticed and became intrigued by the fact that he sometimes wrote /háscu ʔá:gc/ and sometimes /háscu ʔá:gk/ in imperfective "why" questions. On the grounds of our general understanding of Papago, we would expect only /háscu ʔá:gc/ in imperfectives, and only /háscu ʔá:gk/ in perfectives. His commentary on this question makes it rather clear that the semantic burden which is carried by the aspectual system elsewhere in the language is in the case of the "why" expressions assumed by the suffixes /-c/ and /-k/ which, *except* in this case, are merely alternants conditioned by the aspect of the verb to which they are attached and, in and of themselves, have no aspectual meaning. This is something about which I have been misled for over a decade, having always assumed that the use of /háscu ʔá:gk/ in imperfectives was simply a mistake (a failure in performance, if you will) on the part of Papago speakers; this ignorance on my part would have been permanent had Mr. Alvarez not written his short but informative essay. Experiences like this rather typical one have had a lot to do with convincing me of the critical role which native-speaking grammarians will play in the future of American Indian linguistics (consider also, for example, item C in the Appendix, a comment on the semantic difference between the particle /héms-hi/ and the verb /hab R′ʔá:g/ [to think

counterfactually] which I had previously assumed were synonymous; and item H, which suggests that the locative particles can fulfill a function in Papago which is closely analogous to the category actual-nonactual [or visible-invisible] widely documented in American Indian languages but heretofore not recognized in Papago).

One of the greatest dividends of the essay-writing procedure was the large number of suggestions for future research which grew out of it. This was so not only in the case of features of Papago never before noticed or written about but also in the case of aspects of Papago which we have come to take for granted. Mr. Alvarez's essays on phonology cover ground which has been covered before; nonetheless, they make a suggestion about Papago phonetics which could profitably be researched further. In attempting to describe verbally the extremely subtle phonetic distinction between tense and lax stops in Papago, particularly in initial position as in pairs like /kái, gái/ (seed, to roast) and /tái, dái/ (fire, to set), Mr. Alvarez was forced to rely entirely on his own observations. The fact is, we know nothing at all concerning the phonetic nature of the tensity feature in initial stops in Papago. He noticed that, when the stops were pronounced in a whisper, the tense series was characterized by a noise burst which reminded him of high pitch; this characteristic was absent from the lax series. He therefore referred to the tense stops as s-múʔukam (sharp) and to the lax stops as s-hé:bagim (mellow)—see item A in the Appendix; this terminology quite accurately reflects his observations. Recent work on tone in Tibeto-Burman (Maran n.d.) suggests very strongly that there is a real relationship, phonetically (and phonologically in some languages), between high tone in vowels and voicelessness (and tensity) in stop consonants. This may well account for Mr. Alvarez's observations; at the very least, the possibility warrants reopening the investigation.

The essay on Papago sentence types (from which item B in the Appendix was excerpted) grew out of Mr. Alvarez's questioning of the relevance of the notion of grammatical subject in the description of Papago sentences. In my lectures, I had assumed that it was quite correct to say that the noun phrase /(g) ʔóʔodham/ (man) is functioning as the grammatical subject in all of the following sentences:

(1) ʔÓʔodham ʔat g sí:kĩ múa.
    (The man killed the deer.)

(2) ˀÓˀodham ˀo cíkpan.
(The man is/was working.)

(3) ˀÓˀodham ˀo múmku.
(The man is/was sick.)

(4) ˀÓˀodham ˀo géˀej.
(The man is/was big.)

(5) ˀÓˀodham ˀo ge gógsga.
(The man has/had a dog.)

(6) ˀÓˀodham ˀo ké:k.
(The man is/was standing.)

(7) ˀÓˀodham ˀo bágatahim.
(The man is/was getting angry.)

And I assumed further that it would be relatively easy for Mr. Alvarez to coin a term to refer to that function. This was not the case, however; he doubted, perhaps rightly so, that the notion of subject as I was using it (i.e., as it is commonly used in the context of syntactic theory; cf. Chomsky 1965:68–74) was a valid one. He felt that it was appropriate to divide any sentence into two parts: a comment, ˀá:gacugdam (that which is said about something) and a topic, ˀe-ˀá:gacugdam (that about which something is said), but we both realized that this was a different matter. The notion of topic did not refer to the grammatical subject but rather to the noun phrase which was in some sense the focus—the initial noun phrase, in fact; since the grammatical object can be permuted to initial position, it can also be the topic.

Mr. Alvarez went on to suggest that the function of /(g) ˀóˀodham/ in sentence (1) should be referred to by the term wúadam (performer of a transitive action, that which does something to something); in other words, it is the agent. The same noun phrase in sentence (2) he labelled ˀe-wúadam (that of which an action is predicated, the actor). In sentence (3), he suggested, it is the cúˀigkam (that of which a state is predicated); in (4) it is the má:skam (that of which an attribute is predicated); in sentence (5) it is the ˀéñgakam (possessor); in (6) it is the ˀe-jú:kckam (that of which a stance or attitude is predicated); and in (7) it is the ˀe-júñhimkam (that of which a change or development is predicated). For Mr. Alvarez, then, the function of a noun phrase depends on the type of sentence in which it appears. He arrived at his classification of sentences by noting that for each category of verbs

there is an appropriate pro-verb. Thus, the pro-verb /júñ ~ wúa/ (to do to) stands for any active transitive verb; the verb /ˀe-júñ ~ ˀe-wúa/ (to do, act) stands for any active predicate (active intransitive verb, or active transitive verb together with its object); /cúˀig/ (to be) stands for stative verbs; /má:s/ (to appear) stands for attributive verbs; and so on. And he used these verbs in developing a set of terms with which to discuss sentence types and the functions of noun phrases.

It becomes evident in the total essay that the functions Mr. Alvarez identifies are deep semantic ones. They do not change under transformation, apparently, since the object of a transitive sentence and the subject of a passive one are equivalently ˀe-júñkam (that to which something is done), i.e., objects. It is therefore clear why no term suggested itself for the notion of grammatical subject, a real enough notion in Papago syntax since it is the grammatical subject which determines the person and number agreement in the auxiliary. The grammatical subject does not correspond to any unified semantic concept; it is a purely syntactic notion and corresponds neither to the deep semantic functions nor to the surface semantic notion of topic.

This essay is extremely rich in suggestions, far richer than I can hope to show here. Basically, its interest for me resides in the fact that it has forced me to reconsider certain beliefs I have held for a rather long time. For example, it challenges the view that

(8) Jéweḍ ˀat ˀe-móihu.
(The ground was plowed.)

and

(9) Jéweḍ ˀo móihunas.
(The ground is/has been plowed.)

(in which /(g) jéweḍ/ is the grammatical subject) are sentences of the same type—passives derived from sentences in which the noun phrase /(g) jéweḍ/ is the object rather than the subject. The essay is consistent with that interpretation of (8) but implies that it is incorrect in the case of (9); specifically, it suggests that (8) is essentially the same kind of sentence as

(10) ˀÓˀodham ˀat g jéweḍ móihu.
(The man plowed the ground.)

i.e., active transitive, in which /(g) jéwed/ is the object, while (9) is of a fundamentally different kind, stative, in which /(g) jéweḍ/ is the subject. Since the types represented by (8) and (9) are each entirely productive, it is of some importance to determine whether they are in fact fundamentally different. If they are, they provide data which are immediately relevant to the question of how selection restrictions are to be represented in the grammar of a language. In this case, the restriction which determines, in part, the appropriateness of the occurrence of/(g) jéweḍ/ (ground) with the verb /móihun/ (to plow).

The essays which Mr. Alvarez wrote are, in my opinion, the beginning of a quite new and fruitful direction in the study of the Papago language, only a beginning, to be sure, but I believe that it is a strong beginning.

The training procedure which is suggested here is best viewed as a partnership, rather than as a teacher-student relationship of the traditional sort. It is a partnership of individuals with distinct kinds of expertise in which each party seeks to impart his special knowledge to the other—one party seeks to explicate features of his native language, the other to explicate linguistic concepts and questions. The partnership is successful to the extent that this exchange of competences takes place. As a pedagogical device, its greatest value lies in the fact that neither party dominates the other; the essay writer is free to disagree with the linguist and vice versa. Most important, however, is the fact that the essay-writer can explore frontiers which are far beyond the range of topics which the linguist can treat in his lectures on the formal aspects of linguistics. Because of this, the essays, if they are good ones, constitute an immediate contribution to the study of the language.

SOME COMMENTS IN RELATION TO PUEBLO LINGUISTICS

Published and unpublished materials which exist for the Pueblo languages amount to a respectable body of data. While it is true that not all Pueblo languages are represented in this material, each of the four linguistic groups is represented. The published literature alone includes grammars or grammatical sketches for Hopi (Whorf 1946), Keresan (Miller 1965; Davis 1964), Zuni (Bunzel 1934; Newman 1965), and Tanoan (Trager 1964); and the four groups figure extensively in a rather impressive bibliography of short articles, reviews, appendices, and the like which have dealt with linguistic matters. If one adds to this the linguistic data to be found in work which is not strictly linguistic in purpose (e.g., J. P. Harrington's ethnographic studies) and the now vast amount of unpublished linguistic material by scholars who have been involved in Pueblo linguistics over a long period (e.g., Stanley S. Newman, George L. Trager, Carl F. Voegelin, and students of these men), it is possible to assert that the situation of Pueblo linguistics compares rather favorably with the linguistics of other areas of North America. To be sure, the scholar is greatly hampered by the fact that the materials which exist are not always accessible—only two relatively extensive vocabularies are available in a form which permits ready access to accurate lexical information. (Voegelin and Voegelin 1957; Newman 1958). Also there are obvious gaps in documentation; for example, Jemez, so far as I know, is not extensively documented even in unpublished sources.[3] But despite these drawbacks, there exists a good foundation for further progress in Pueblo linguistics.

The thrust of this paper has been to argue that the contribution of the native speaker in linguistic research is central. There are limits beyond which it is extremely difficult for a nonnative speaker to go, and these limits are reached quickly in the kind of research characteristic of American Indian linguistics in recent decades. I think most Americanists would agree that a plateau is reached when a relatively thorough understanding of the phonology and morphology is attained in the course of fieldwork on an American Indian language. In fact, that is the point at which the linguist has traditionally paused to write a grammar of the language. Prior to that time it is quite possible for the investigator to work with only minimal reliance on the linguistic intuitions of a native speaker. Beyond the plateau, however, the real data of linguistics become identified more and more closely with the native speaker's intuitions—so much so that it is really questionable whether the traditional linguist-informant pairing makes any sense at all; to the extent that the pairing is effective, the informant is no longer an informant but a linguist. If this is so, then I think it is fair to say that future progress in Pueblo linguistics will be in the hands of linguists who are native speakers of Pueblo languages. I would like to relate this point to a specific Pueblo case by citing a couple of examples from Jemez gram-

mar which will serve to contrast the kinds of knowledge which exist below and above the research plateau alluded to in the preceding paragraph.

Jemez transitive sentences distinguish active and passive voices. The latter is characterized by a special passive suffix on the verb and by an agentive suffix on the agent noun phrase. In addition, the verb is inflected by means of the intransitive prefix paradigm in agreement with the person and number of the derived subject (i.e., logical object or patient). Thus in

(nį·) ve·la-tæ̨ į-tos-æ.
(I man-by lsg-hit-passive)
(I was hit by the man.)

the verb is in the passive form /tos-æ/, cf. the active /tóse/ (to hit), and takes the intransitive first person singular prefix /ɨ-/ in agreement with the derived subject /nį·/ (I) which is optional in the surface form of the sentence. And the agent /ve·la-tæ̨/—cf. /ve·la/ (man)—appears with the agentive suffix /-tæ̨/. By contrast, in an active sentence, the verb inflects by means of the transitive prefix paradigm (indicating, partially at least, number and person for both the subject and the object). The subject and object are both unmarked for case, and the verb appears in its active form:

(nį·) ve·la ta-tóse.
(I man lsg3sg-hit)
(I hit the man.)

The prefix /ta-/ indicates first person singular subject acting on third singular object.

There is nothing particularly striking in the fact that Jemez distinguishes active and passive voices, and there is nothing at all surprising about the way in which the distinction is reflected in Jemez nominal and verbal morphology; the formal aspects of the passive are exactly what one would expect given other facts about Jemez grammar. This is not the whole story, however. The role which the passive plays in Jemez syntax is quite different from what one would expect from an Indo-European perspective. On the contrary, the use of active and passive is not free in Jemez. Instead, certain properties of the logical subject and object (i.e., actor and patient) determine whether a given

transitive sentence will be in an active form or in a passive form. The most obvious constraint (so obvious as to be noticed almost immediately in fieldwork) is that a transitive sentence must be in the passive form if the actor is third person and the patient is non-third person. Thus, there is no corresponding active form for the first of the two sentences cited above.

Up to this point, the facts I have given relative to the Jemez passive are of a readily accessible kind. They would emerge quickly in the context of traditional fieldwork. The morphology would be obtained by systematically eliciting the passive for all transitive verbs collected, and the special constraint would be quickly noticed in the course of eliciting paradigms, since the constraint results in the paradigmatic gap that there are no prefixes for third person acting on non-third person. So far, no particular appeal is made to the linguistic intuitions of the native speaker—this amount of information could be obtained if the investigator limited himself to questions like "How do you say —— in Jemez." Beyond these basic morphological and paradigmatic facts, however, there is a rather large body of questions whose answers require an active contribution from the linguistic intuitions of a native speaker. One of the questions which remains can be summarized roughly as follows: the passive form is required when the actor is third person and the patient non-third person. What conditions the use of the active and passive in all the other cases? Is it free? Are there additional constraints or preferences? And so on. Consider, for example, the case in which both the actor and the patient are third person. My data indicate that both passive and active forms occur, but they also suggest that the choice is not free. There is some indication that nominal concepts are ranked (from highest to lowest: human, animal, inanimate, abstract) and that the ranking plays an important role in determining whether a sentence will appear in the passive or the active. I suspect, on the basis of very limited data, that the passive is preferred if the patient outranks the actor, the active if the opposite is true.[4] I cannot test this hypothesis, however, since my knowledge of Jemez is limited by the confines of a corpus. If I were a native speaker, I could pursue this line of investigation without hindrance, since I would be free to consult my own linguistic competence.

Needless to say, the questions which I cannot answer about the Jemez passive are of far greater interest than the purely morphological

and gross syntactic questions for which I have some answers. But it is in the nature of the problem that the interesting questions will remain unanswered if the linguist is not permitted access to a native speaker's competence. A return to the field is not the best solution to this problem. It may provide answers to some of the questions which the linguist has while he is actually in the field, but it will be of no use in relation to questions which occur to him later. The reasonable response to this problem, in my opinion, is to work to bring about a situation in which American Indian linguistics is in the hands of native speakers of American Indian languages.

Jemez, like the other Tanoan languages (cf. Dozier 1953; Trager 1946:214), permits incorporation of nominals into the verb word. Thus, in

ta-nǫ̂·ši-pæpæʔ.
(lsg3sg-pottery-make:imperf)
(I am making pottery.)

the object /nǫ̂·ši-/ (pottery) is incorporated between the person marking prefix and the verb stem. When I first learned this fact about Jemez, my interest was in such morphological considerations as the relative order position of the incorporated noun among the elements of the verb word and the phonological consequences of incorporation on the verb stem. To study these questions, it was sufficient merely to collect a large number of examples of incorporation. At a later time, my interest shifted to the syntax of incorporation—that is, to the grammatical conditions for incorporation. There are many questions involved in this topic, among them are the following: (a) Which grammatical relations can incorporate? Object only? Subject? Agent? (b) Is incorporation a syntactic rule involving the actual movement of a nominal into the verb word? If so, must it precede or follow the rules which effect the prefixal person and number agreement in the verb? (c) What structural and semantic properties of noun phrases determine their ability to incorporate? Must an incorporating nominal be devoid of determiners, for example? Can the head of a noun phrase incorporate into the verb and leave its determiner(s) behind in the position originally occupied by the full noun phrase? Must an incorporating nominal be indefinite? Nonspecific? (d) What is the difference semantically between a sentence in which a particular noun is incorporated and an otherwise identical sentence in which the same noun is left unincorporated? For example, how do the following sentences differ in meaning?

ɨ-gɨwǽyi-zɨ́·t'-æ.
(lsg-horse-throw-passive)
gɨwǽyi-tę ɨ-zɨ́·t'-ǽ.
(horse-by lsg-throw-passive)
(I was thrown by a horse.)

(e) Can an incorporated nominal function as antecedent in anaphora? That is, for example, could /gɨwǽyi/ (horse) in the first of the two sentences above be referred to by means of an anaphoric element appearing in a subsequent clause? (f) What is the relation between the passive and incorporation? If both are movement rules, how are they ordered with respect to one another? (g) What is the relationship between incorporation and relative clause formation? For example, how does one explain the fact that /gɨwǽyi/, which is semantically the antecedent in the following relative clause, is incorporated in the verb of the embedded sentence?

ɨ-gɨwǽyi-k'a-ʔe
(lsg:dat-horse-be-rel)
My horse (literally, horse that is to me)

It is rather obvious that at least these questions are of relevance to any attempt to understand the syntax of Jemez noun-incorporation. However, the relationship between these questions and the data which I actually have on the subject is entirely accidental, since precisely these questions did not occur to me while I was collecting Jemez material. It is therefore impossible for me to study Jemez incorporation without collecting more data, which would present no problem if I were a native speaker of Jemez.

As the field of linguistics advances, the questions which concern linguists change. The main areas of concern and success in modern American Indian linguistics have been phonology and word morphology. The interests of many linguists have now shifted to areas of syntax which are only poorly represented, if at all, in the majority of available grammars. This shifting of interest will continue so long as linguistics is a healthy field. It is therefore certain that no finite corpus of data,

regardless of its size, will succeed in being relevant to the questions linguists will pose. The two examples from Jemez illustrate this point, but virtually any topic of Jemez grammar would serve to illustrate the minimal extent to which my Jemez corpus addresses the questions I now have about Jemez grammar. By contrast, the linguist who is a native speaker of the language he studies has the advantage of immediate access to data as his concerns change in the course of his development.

CONCLUSION

I have argued in this paper that the future of American Indian linguistics will depend on the extent to which American Indians are enabled to engage in scholarly work on their languages.

The procedure sketched in the section before the last one is offered as a suggestion of how the training aspect of this general question might be approached. I believe, however, that training is the least problematic aspect. A more serious problem lies in attitudes which prevail in Anglo-American education and the society at large concerning the credentials recognized as prerequisite to a career in certain kinds of scholarship—including the study of American Indian languages. These attitudes must be changed. In this case, proper credentials seem to me to be fluency in an American Indian language, talent and interest in the study of the language, and appropriate training relating specifically to the study of the language. This country would be enormously enriched if a large number of people with precisely these credentials were enabled to devote full time to writing about their own rich and beautiful linguistic traditions.

Appendix

ALBERT ALVAREZ
Sells, Arizona

A. *The Sharp-Mellow Distinction in Papago Stop Consonants*

Kunt ˀíd ˀam héˀes ˀép o ṣaˀi ˀem-táṣogī mañ háscu ˀá:gc hab ha-ˀáˀaga ˀídam /p t c k/ mo s-múˀukam hab káidag c háscu ˀá:gc hab ha-ˀáˀaga ˀídam /b d j g/ mo s-hé:bagim hab káidag.

M ˀamt héjel o si ˀe-káihamad ˀam s-júpij hab ha-cécˀejc ˀídam mant ˀin o ha-ˀóˀoha ˀe-húhugid ˀan t ˀam o ˀe-káic mo héma s-múˀukam c héma s-hé:bagim hab káidag ˀídam ha-ˀéˀeḍa. M ˀia hab cúˀig

/kái/ /gái/
/pí:ṣ/ /bí:ṣ/
/tái/ /dái/
/cíwa/ /jíwa/.

Kutt hémuc ˀídam ha-ˀáb /k/ kc /g/ ˀab o ṣónwŭic ˀídam mañ ˀan hú hab ha-ˀá:g "s-múˀukam" c "s-hé:bagim."

ˀÍ:da maṣ wuḍ /k/ mamt hékid hab o céi t gm hú si ˀem-báˀitk ˀeḍ ˀam o si ˀal ˀe-kú: g ˀem-ˀí:bhei k ṣaˀi si s-hó:tam gam a hékaj ˀép o si ˀal s-kópñim ˀe-kú:pio. K ˀí:da mat ˀam o si ˀal kóp si ˀal s-múˀukam hab o céi. Kuñ heg hékaj hab káij mo s-múˀukam hab káidag ˀí:da /k/. Kc ˀí:da maṣ wuḍ /g/, mamt hékid ˀam hab o céi t hab-a másma ˀép o ˀe-kú: gm hú si ˀem-báˀitk ˀeḍ g ˀem-ˀí:bhei k aṣba pi hab másma ˀam hú o si ˀal kóp ˀam ˀe-kú:piˀokk aṣ s-hé:bagim ˀam o ˀe-kú:pio. Kuñ heg hékaj hab káij mo s-hé:bagim hab káidag ˀí:da /g/.

Kc hab-a másma ˀam ha-tá:gio ˀídam háˀi /p t c/, mo wé:sijc s-múˀukam hab káidag. Kc ˀídam háˀi /b d j/ mo hab-áˀap wé:sijc s-hé:bagim hab káidag. Ṣa g wépo mant ˀam a ṣa ˀem-táṣogī mañ háscu hab ˀá:g ˀab ha-ˀámjeḍ ˀídam "s-múˀukam" c "s-hé:bagim."

Kutt hémuc g /p/ kc /b/ ˀam háhawa ˀép o ˀáˀaga mo ˀam a gáwulko ˀámjeḍ kópkˀe kc aṣba hab-áˀap s-múˀukam c s-hé:gagim hab