nuhon, nuhum are to be interpreted laan, liin, liin, luun, la’an, li’in, li’in, lo’on, lu’un; that ñaa, ñii, ñuu, ñahan, ñikhin, nuhum are to be interpreted yaan, yiin, yuun, ya’an, yi’in, yu’un; and that maa, mahan, mikhin are interpreted wa’an, wi’in. There are syllables lii, loo, luu; yaa, yee, yii, yoo, yuu, yaha, yihii, yoho, yuku; vaa, vee, vii, voo, vaha, vehe which complement the above. This proposal eliminates /m/ and /n/ from the system of phonemes, leaving the following consonant system:

| t ö k k* | ? |
| s ö h n |
| l y w |
| r |

B. Peripheral phonology

/p/ occurs word-initial in two regular <pahlo> ancho y plano, <pelo> terroñ and two sound-symbolic native morphemes <pahlo> kersplash!, <pelo> guajolotito only; otherwise in loans. /np/ [pʰ] occurs word-initial in one loan, <mbelo> compadre. /f b d g ñ/ occur in loans.

C. It is of course true that the prenasalized stops could be interpreted as unit phonemes, although it seems quite unnecessary. In this case [sʰd] would be interpreted /sʰd/.

D. Spelling (based on the first formulation of elements):

/ç/ → ch>, /k/ → <e/qué>, /k* → <e>, /s/ → <x>, /h/ → (j), /w/ → (v), /ʔ/ → (h), /np/ → (mb), /nt/ → (nd), /nk/ → (ng, g), /nk/ happens not to occur before /i/ or /e/), /sr/ → (xnd), /high/ → (i), /mid/ is unmarked, /low/ → (l). Other symbols remain unchanged.

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Terrence Kaufman

This publication, in providing original folklore and ethnographic material, also provides documentation of the Chol language in text. Both kinds of materials may be compared with similar materials for other Mayan groups with profitable results. The creation stories, especially, show important parallels, down to small details at times, with creation stories current among the Tzeltals and Tzotzils, and more distant parallels to episodes in the Popol Vuh. Judging from his own experience in collecting Tzeltal and Tzotzal traditional stories, this writer believes that there must be a much greater mass of Chol material which remains either unreported or uncollected.

The book includes twelve creation stories occupying forty-nine pages, nine descriptions of religious ceremonies occupying twenty-four pages, nineteen stories and descriptions relating to the spirit world and witchcraft occupying sixty-eight pages, and a vocabulary of sixteen pages having about twelve hundred entries. The format is in conformity with earlier issues in the SIL text series: two-thirds of each page is occupied by native language text, and one-third of each page is taken up by the English translation (in smaller type). Each word in the native text is numbered, and each word in the English translation has the same number as the native word of which it is a translation.

We are informed that there are two dialectal varieties of Chol, and that the present materials represent the Tumalá variety. Judging by lists of field workers and their locations which the SIL has published, the other variety must be called Tila Chol. According to the preface, the basic analytic work on the language was done by Evelyn Woodward Aulie.

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William J. Samarin

This monograph does more than its title suggests. It discusses several different aspects of field work. Only the first section deals with informants specifically. The other sections are entitled Linguistic Surveys, Investigating Phonology, Pair Testing, Eliciting Grammar Monolingually, Determining Meaning, and Using Tape Recorders.

Although Healey states that the monograph "describes the author's methods of handling unsophisticated informants while investigating the Telefóol language" (1), the manner of presentation is not entirely autobiographical. Witness such statements as: "To foster and maintain good relations with one's (potential) informants is an essential part of field work" (1); "pronouns are best obtained by careful observation" (13). Such recommendations are reinforced by 43 footnotes and a bibliography of 63 separate works. This then is a brief but rather serious attempt to provide potential field linguists with a practical guide. It deserved a better title, but we shouldn't criticize the author for the title if he gives good advice; he does.

It would be inappropriate to subject this work to a critical appraisal. After all, its goal is modest enough. Besides, all that I would want to say I have already said in my Field Linguistics: A Guide to Linguistic Field Work (to be published soon by Holt, Rinehart and Winston). It is enough to say that I recommend Healey's work without hesitation. One should have a copy around to lend to prospective field workers.

It is unfortunate that there are not more studies of this kind. The literature on linguistic field work, in spite of the size of Healey's bibliography, is very small indeed. In fact, one of the most avoided subjects in linguistics is field methods. Although investigators have spent untold hours in the field gathering data from every imaginable kind of linguistic community, very few have reported on their experiences. Very few, indeed, even document the nature and extent of their contact with the source of their information. Reflection on instructive successes and failures is virtually absent in linguistic literature. There is certainly dammed up throughout the world today a tremendous body of knowledge about linguistic field work, but we get only trickles of it from time to time.

There should be some profit in seeking an explanation for the relative silence about field linguistics. It may be that investigators have not addressed themselves to field methods, because there were more urgent or more exciting problems before them. It is not a secret that some linguists have reacted to field methods as they do toward teaching methods in primary school. They could see nothing more serious in a discussion of field linguistics than that one should treat his informant well, that one should avoid becoming ill, etc.

Some linguistic practitioners may have felt that linguists were born, not made. Such a view seems to have been held about twenty years ago. George L. Trager, in writing about Nida's attempt to teach linguistics to missionaries, says: "I have heard the opinion that we should pay little attention to those students who don't understand linguistics right away and who are not top-notch and 'born' linguists." (When asked if Leonard Bloomfield had ever held such a view, Bernard Bloch vigorously denied it. He told me that Bloomfield may have discouraged people from pursuing linguistics as a career because of the scarcity of jobs, but not because linguistics was unteachable.)

For the past we can only guess at the reasons why linguists have avoided the field problems of their science. For the future we may have transformational grammarians to blame. This is put too strongly, I know. But it is nonetheless true that some transformationalists are frankly disinterested in how they get from raw data to their own grammar which would account for it. Their concern is not with discovery procedures, but with the more basic question about the

nature of language. None would assert that formal linguistics is incompatible with good field linguistics, but can anyone deny the danger of de-emphasizing field methods in the present preoccupation with linguistic theory?

A kindly colleague who believes that transformational grammar is extremely useful in field work (see below) confessed that the preceding paragraph made him bristle. He den'''t deny the danger I feared. But I do not seem to be alone in my concern about the implications of what some have said for field work. Householder, for example, makes the following observations when he discusses Chomsky's principle of three adequacies:

It is clear from this paragraph and many other passages in this speech that Chomsky feels that 'observational adequacy' is completely 'uninteresting' (and whatever Chomsky himself may intend by this term, his followers without exception interpret it as meaning 'bad', 'scientifically unsound', 'to be avoided at all costs', etc.). This has the unfortunate effect that mere mistakes of fact, no matter how gross and glaring, tend to be looked upon as trivial, and no votary would admit publicly that he spends any time in avoiding them. I am sure Chomsky did not mean to exert such an influence, but it is unquestionable that he does.

. . . No doubt Chomsky means 'descriptive adequacy' to include 'observational adequacy' somehow, but he doesn't say so, and the result is unquestionably the notion that data are unimportant to the linguist.²

One reason for soliciting more published information about field linguistics is the training of field investigators. It is an awfully wasteful procedure to require each new field worker to learn by trial-and-error. With the mountains of work that still lie ahead of us, we need many more investigators than are now available. At the same time, we need to know how to do our work with the greatest possible efficiency and accuracy. Ideally, a prospective field worker should put in a period of apprenticeship with an experienced linguist, when the mentor and the student could work closely together collecting and analyzing the data. In such circumstances a student could learn to appreciate and emulate his teacher's abilities. The only regular substitute for this apprenticeship is a course offered in 'field methods' where instructor and students elicit data from a native speaker. How adequate such courses generally are is something I have not had the opportunity to assess. My own experience with trying to set up realistic goals for these simulated 'field sessions' makes me convinced of their importance in linguistic training but also unhappy with their limitations.

An equally important reason for greater attention being given to field work is that we might eventually arrive at a better understanding of the relation between the product of linguistics (e.g. in grammars and in various kinds of studies of human behavior) and the 'dirty work' that precedes it. After all these years we still know far too little about informant reaction, for example. We know that the best informant is an intelligent one, but what is the best way to use an informant? About what kinds of linguistic information are the informant's responses most reliable? The time has surely come for some rigorous and sophisticated experimentation. And while we are waiting for these experiments to be set up and reported, linguists should be encouraged to report on their less formal experiments.

A few field workers have reported greater success with the generative-transformational approach than they would have had with the traditional approach. Their procedures need to be tested by investigators of different degrees of competence in all kinds of situations. We should even hope that someday various methods of eliciting language data will be tested on a single language. Everyone would stand to profit from such experiments.