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Cultural Anthropology and Linguistics in the Great Basin: Some Proposals for the 1990s

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PREDICTING where a discipline or sub-discipline should go in the immediate future and be 10 years hence is risky business. On the one hand, it is based on an evaluation of the goals of that concern, whether publicly stated or more subtly implied, and a consideration of how well those goals have been achieved to date. On the other, it is based on some realization that what has been done and what is likely to be done is grounded in personal agendas—those of the people continuing to work in the region as well as those of persons who might be convinced to begin work in the near future. Thus, at any point in time, programmatic statements are based on a reading of history mixed with personal desires and plans. This one is no exception. If it is of use to anyone, it will be students and young scholars who perhaps have not yet set their research agendas.

READING THE PAST

As noted, it is difficult to say much about the future of cultural anthropology and linguistics in the Great Basin in the 1990s without saying at least something about the past. In other words, what was it (and is it) that ethnologists and linguists have been trying to do, who has contributed to the tasks to date, and what remains to be done? Although there are some recent summaries of data tied to these questions (e.g., D. Fowler 1986; Jacobsen 1986; Miller 1986), they are worth reviewing again.

Certainly one of the goals of Great Basin ethnography has been to try to describe and un-

derstand pre- and immediately post-contact lifeways—to do reconstructive ethnography. With reference to this, the Great Basin and its peoples through the years have been hosts to some of the greats in this type of work. The list, in fact, reads like a “who’s who” of the Americanist tradition: John Wesley Powell, Stephen Powers, James Mooney, Samuel Barrett, A. L. Kroeber, Robert Lowie, Edward Sapir, Frances Densmore, Julian Steward, Isabel Kelly, Marvin Opler, Omer Stewart, E. Adamson Hoebel, Demetri Shimkin, Sven Liljeblad, Warren d’Azevedo, and Ake Hultkrantz, as well as a number of others still moving up in the ranks. Most of those listed (as well as a few others) have invested more than a single field season in the region, either by working with one group repeatedly, or with more than one group. Given that amount of attention, and especially the credentials of the researchers, one would expect that Great Basin cultures for the past have been well described and perhaps well synthesized. Yet, as many of those who wrote ethnographic sketches of Great Basin peoples for the recent Smithsonian *Handbook* volume (d’Azevedo 1986) can well attest, that is hardly the case. What exists is a scattering of basic descriptive monographs for the region, most done in the early part of this century, and a few as late as the 1940s. Included are monographs on the Washoe, Owens Valley Paiute and Surprise Valley Northern Paiute; Chemehuevi and Kai-bab Southern Paiute; Gosiute, Wind River, and Northern Shoshone; and some Northern and

Southern Ute groups. In addition, there is the thin net of the Culture Element Distribution (CED) Survey (Kroeber 1935), which in some cases gives the only data on other Great Basin groups and subgroups. If one tallies the actual time spent in the field on some of these classic studies it is surprisingly little—one month to a summer or two for the monographs, two to three days for the CED reports. Similarly, if one looks at the numbers of Great Basin people who have acted as consultants for these works, it is also surprisingly few: four or five individuals for the monographs, one or two individuals per subgroup for the CEDs. In many cases, those who have spent the most time in the region have been those who have published least—perhaps a result of the realization that “the more you know, the less you understand.” Those who spent the least amount of time felt they understood more. Those who interviewed more people rather than fewer also had to deal with the problem of individual variation that, given the nature of Great Basin societies, is considerable. Again, perhaps, the more they interviewed the less they understood.

The focus of all of this, which really is not a large body of data when it comes right down to it, was reconstructive ethnography of the type Isabel Kelly (1964:iii) once characterized as the “how-was-it-in-your-grandfather’s-day” genre. That was the ethnography that Americanists were trained to do, and that was the type that was attempted and completed. It is also the type that still interests some, including me. But unfortunately, none of the people who were abroad in the Great Basin between 1900 and 1930, with one or two exceptions, produced an ethnographic description of the “how-was-it-in-your-parents’-day” or “how-is-it-in-your-day” genre—post-contact or post-reservation ethnography. This is particularly sad, given that it has become clear that the “how-was-it-in-your-grandfather’s-day” genre is increasingly suspect as to exactly what time period it really rep-

resents. Most were based on interviews of persons who were not even then recalling pre-contact traditions. I know that I have struggled with this question of assigning a time frame to unpublished field notes in working with Willard Park’s extensive materials from the 1930s for the Northern Paiute of western Nevada (C. Fowler 1989a), with Isabel Kelly’s notes from the 1930s on the Southern Paiute and Chemehuevi (C. Fowler 1989), and at the moment with my own and Margaret Wheat’s unpublished notes from the Northern Paiute of Stillwater from the 1950s through 1970s. In many senses, as we all know, the pre-contact period is really the mythical past, and it too, like *The Time When Animals Were People*, is more folktale than reality. Yet at least some of us quest after that past, or some semblance of it, and will continue to do so.

This leads to the first task for the 1990s: continue to clear up the descriptive record for the pre- and immediately post-contact periods, by getting into print, hopefully with some thoughtful evaluation, the various known collections of unpublished field notes still extant, and also continuing to conduct basic field research to enhance this record. I, for one, will be working on a second volume of Park’s materials, those of Kelly, and Wheat, and if time permits, Carobeth Laird’s additional data on the Chemehuevi. I will also be working on a general monograph on Great Basin material culture, based on collections presently held in museums. Some other examples include the work of Sharon Rachele, a doctoral student at the University of California, Riverside, on the WPA materials for Owens Valley and Mono Lake (e.g., Essene 1935; Hulse 1935); and that of Pam Bunte and Robert Franklin on Sapir’s Southern Paiute song text manuscript (Sapir MS).

Several works have also appeared in recent years that help to fill the gap in studies from and for the early period: Maurice Zigmund’s

(1980, 1981) mythological and ethnobotanical materials on the Kawaiisu; Ann Cooke Smith's (1974) master's thesis on Northern Ute material culture; her collection of Ute tales from her doctoral dissertation research (Smith 1992); and soon to appear, her Western Shoshone Tales (Smith n.d.). Edgar Siskin's (1983) doctoral dissertation on Washoe shamanism and peyotism has also been published, as have parts of Carling Malouf's (1974) master's thesis on the Gosiute, and much of Carobeth Laird's (1976, 1984) work with George Laird on the Chemehuevi. I really don't think that we can do much more for this basic descriptive record until we clear the decks, so to speak, of some of these extant collections. Hopefully, others who have large bodies of data representing attempts to reconstruct the pre- or immediately post-contact time period will either archive them or work on them. For example, the University of Nevada, Reno Library recently received all of Edgar Siskin's Washoe field notes and photographs. It also holds the tapes (some transcribed) and photographs of Margaret Wheat of the Northern Paiute. Other archives also hold important materials.

IMPROVING THE RECORD FOR THE PAST

The second part of Task One for the 1990s is to improve this ethnographic record with contemporary studies still relevant to reconstructive ethnography. As we all know, there is a considerable amount of cultural continuity still extant in Great Basin Native American communities. However, it takes a real investment of time to draw it out and evaluate it. People all over the region are still engaged in aspects of subsistence that show continuity with the past: the harvesting of roots and tubers in the Burns area, the topic of fairly recent studies by Couture et al. (1986); the harvesting of Pandora moth larvae by the Owens Valley Paiute, as recorded by Nancy Walter and me a few years

ago (Fowler and Walter 1985); and although not ethnographic, the attempts by Simms (1987) and others to reconstruct time and yields of certain Great Basin foods from simulation experiments.

There are still quite a number of people in the region who know something about other traditional pursuits, such as the taking and use of medicines, fibers for manufacturing, and a host of other things that have to do with people-to-land relationships. Each person targets a few resources, rather than the hundreds of their grandparents or great grandparents, but what they know is very important to record. Similarly, there is a considerable amount yet to be learned about traditional material culture, social organization and intergroup relationships, religion, and much more. Great Basin religion is particularly poorly described, apart from some good data on shamanism (e.g., Handelman 1967; Park 1938; Siskin 1983). Again, these data are not likely to be learned easily or quickly, as many of them are subtle. Nor can one count on a single person or two or three to know everything. However, if an ethnographer is already well versed in what has been recorded, that person can certainly enhance the record with the proper investment of field time. Institutions need to encourage this among their students, especially on the doctoral level.

A potential source of new data relevant to reconstructive ethnography in the 1990s may be the various "compliance" studies mandated by federal environmental legislation (e.g., American Indian Religious Freedom Act, National Environmental Policy Act, etc.). Properly done, these can and already have contributed materials to the record that are exceedingly interesting. A case in point is Clemmer's (1990) work on the Tosawih Quarries and Rock Creek Dam areas of Nevada. Another is the work by Stoffle and colleagues (1983; 1990) on the Intermountain Power Project and proposed high-level nuclear waste repository at Yucca Mountain, Nevada. But in order for these data

to be truly useful, the studies must be as rigorous as other ethnographic field investigations, and the results properly disseminated—not buried in the gray literature as has been much of cultural resource management (CRM) Great Basin archaeology in recent years. Indian people must also see some positive results of their willingness to come forward and talk about sensitive places and issues. Let's hope the 1990s will see some good partnerships of this type as well as some high-caliber results. The potential is certainly there.

Certainly another major ethnographic task for the 1990s is to enhance the record of post-contact, post-reservation, and contemporary Great Basin life, the neglected side of ethnography done in the early years. At present, due to the lack of studies of contemporary life in the 1910s, 1920s, 1930s and 1940s, we are forced back into the reconstructive mode for describing lifeways during these periods. Some of the tribal histories written in the 1970s (see Alley 1986) help to fill this gap, but most of them attempt to cover too much and thus need considerable amplification. Knack and Stewart's (1984) monograph on Pyramid Lake, *As Long As the River Shall Run*, and Bunte and Franklin's (1987) work on the San Juan Southern Paiute, *From the Sands to the Mountain*, set good precedents for what needs to be done.

There are also several unpublished doctoral dissertations the publication of which could enhance this record (e.g., Clemmer 1972; S. Crum 1983; Endter 1987; Hittman 1972; Houghton 1973; Lynch 1971; Roth 1976; Walter 1986). Much of the approach of these works is perhaps better termed ethnohistoric than ethnographic, yet given the rich documentary base languishing in various federal archives, the approach is certainly fruitful. Carefully utilized, along with ethnographic data from thoughtful interviews of Great Basin people who were involved during these periods, these data could contribute a great deal toward

understanding these periods. It would be significant if the 1990s saw the publication of 10 or more such studies, including some of those already in manuscript. Steven Crum (personal communication, 1990) is presently working on a documentary history of the Western Shoshone expanded from his dissertation (S. Crum 1983), which will be particularly useful given the lack of data on many of these groups. Martha Knack (personal communication, 1989) is also working on one or more for Southern Paiute communities. But there is much to do with reference to all the other Great Basin groups. Again, students and others should be encouraged to undertake these tasks, as well as to do contemporary reservation and community studies. There is an appalling lack of data on nearly all aspects of contemporary life in the region—sociopolitical, economic, religious, inter-ethnic, etc. (but see, for example, Knack 1980). Again, some CRM projects might result in the funding of useful studies in this direction, if agencies require them. Certainly the assessment of potential socio-economic impacts is one important aspect of major land-altering activities.

NON-INDIAN COMMUNITY STUDIES

Up until now, I have been discussing ethnography in the Great Basin as if it concerned only the description and interpretation of Native American cultures. But certainly there is a much wider task out there for those interested in other groups—Basques and other ethnic groups, ranchers, farmers, miners, small rural communities, rapidly expanding urban populations. There are a vast number of field opportunities for many to work in these areas, as very little has been done (e.g., Douglass 1979; Nelson 1952). Rural sociologists and anthropologists working through Utah State University have recently studied Caliente, Pahrump, Beatty, Alamo, and several other communities in southern Nevada in connection with assessing

impacts of the proposed Yucca Mountain nuclear waste facility (Little and Krannich 1990). Some studies of contemporary Native American communities partly focused on inter-ethnic relations were funded and conducted through that project, although all of this work also remains unpublished at present (e.g., C. Fowler 1990; Fowler, Rusco and Hamby 1988). Many other such opportunities present themselves for the 1990s.

INTERPRETIVE STUDIES

Beyond basic ethnography, there is also ethnology, the synthesis and interpretation of cultures according to a specific theoretical framework. A very large task remains for the 1990s in this area, and it would be appropriate to see several works added to this rather small list during the period. Julian Steward's (1938) *Basin-Plateau Aboriginal Sociopolitical Groups* still stands out as one of the main interpretive treatises for the region, even if it only includes social and political organization *vis-à-vis* the nature of the environment for a few select groups (Owens Valley Paiute and Western and Northern Shoshone). Others that are partial or topical ethnological treatments are Willard Park's (1938) *Shamanism in Western North America*, Omer Stewart's (1944) *Washo-Northern Paiute Peyotism*, Beatrice Whiting's (1950) *Paiute Sorcery*, and more recently, Joseph Jorgensen's (1972) *The Sun Dance Religion: Power for the Powerless*. Some of the Basque work by William Douglass (1979) and his colleagues fits here as well. And there are a few others, again some in the form of doctoral dissertations that remain unpublished. It is interesting to note regarding syntheses and theoretical works, however, that Great Basin studies have not kept pace with changing paradigms in the sub-discipline as a whole. Certainly much remains to be done in synthesizing older data, publishing existing works, and undertaking new studies along these lines.

Perhaps if the descriptive record is improved sufficiently during the 1990s, more opportunities for synthesis and interpretation will present themselves.

LINGUISTICS

Although not originally part of my assigned task, I do feel compelled to say something about the state and future of linguistics and linguistic anthropology in the region for the 1990s. Again, if we look at the record here, we are not in very good shape. Southern Paiute/Ute remains the best reported Great Basin language, with the early work of Sapir (1930-31), and later studies by Bunte (1979), Givon (1979, 1980, 1985) and Press (1979). The recent publication of a Panamint grammar and dictionary by Daley (1989a, 1989b) has certainly brought the record on that language from near zero to a significantly higher level. Miller's (1972) Gosiute Shoshone grammatical sketch, along with texts and a brief lexicon also further our understanding of that language a great deal. As for the remainder, we again have some unpublished dissertations, such as Jacobsen's (1964) on Washoe grammar, Norris' (1986) on Owens Valley Paiute, Lamb's (1958) on Northfork Mono (Lamb [MS] also has an unpublished dictionary of the latter), James Goss' (1972) on Southern Ute, and now Jean Charney's (1989) on Comanche. For the others there are smaller efforts in published papers, but not thorough treatments in terms of grammar, lexicon or dictionary, and texts.

This situation also needs to be changed in the 1990s, with additional publication of extant materials and the collection of new data. Indian people all over the Great Basin are in need of such studies, but also written in a format that they can use. I hope to continue my own studies, and the much more extensive work of Sven Liljebald on Northern Paiute during the 1990s, another personal task.

A great deal remains to be done in other

aspects of linguistic anthropology—socio-linguistics, ethnolinguistics and all of those related topics. Beyond grammars, dictionaries, and texts, are the many ways in which language is and was used in everyday life, in song, speech making, prayers, and much more. Although there are some studies of this type for the region, such as B. Crum's (1980) work on Shoshone poetry songs, Vander's (1988) monograph on the song repertoires of five Eastern Shoshone women, Loether's (1985) work on Mono ethnopoetics, Miller's (n.d., 1970) on Western Shoshone bilingualism and language death, and others, we should try to double or triple this record during the 1990s. Here is a place where Indian people can obviously contribute a great deal through work on their own, as they certainly have already (e.g., B. Crum 1980).

In fact, in the last analysis, perhaps our biggest and best task for the 1990s is indeed the training of Indian people themselves to do much of the work that remains in linguistics, ethnography, ethnohistory, and ethnology. We cannot expect that many or all Indian people will automatically be interested in these tasks, just as very few non-Indian students are interested in ethnography and linguistics today. But with proper encouragement and an open spirit of cooperation, at least some young people, as well as a number of older people, can be convinced that these are their tasks, too. I, for one, hope that the 1990s will be the decade where we see a true increase of participation in all of these endeavors by Indian people. I do not think that we should be satisfied with less.

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