

ALLIGATORS IN THE SWAMP:  
FIELD PROBLEMS AND TRAINING ISSUES  
IN CONTEMPORARY ANTHROPOLOGY

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As suggested in the title of the symposium in which this paper was presented, much contemporary anthropological research is taking on a new form and differs substantially from that conducted by the preceding generation of fieldworkers. Yet, with few exceptions, academic training programs are not keeping pace with this trend, resulting in the discovery by a growing number of students that they lack important tools necessary for engaging in timely field research. Even more disconcerting are reports from students who have encountered situations in the field with which they were poorly prepared to contend--but about which they could have gained awareness in their academic training. In treating fieldwork as the rite de passage culminating several years of academic preparation, our mentors all too often fail to recognize that circumstances surrounding the rite are changing as well. It seems rather ironic that the discipline noted for its understanding of cultural dynamics appears reluctant to recognize that, as the emphases of anthropological research change, so must the training and skills of those who plan to become anthropologists. As Alfred Smith observed, ". . . Anthropology also has a history. Like any other culture, anthropology changes--through innovations, diffusion and other processes. . ." (1964:251, cf. Cohen 1977:390).

By drawing an analogy from a sign I once read, perhaps my point will be readily apparent: "When you're up to your ass in alligators, it's difficult to remind yourself that your primary objective was to drain the swamp." If we view the primary objective of fieldwork as "draining the swamp," how has it changed from the Boasian era or during the past two decades? The change is essentially more one of degree than of kind, for our predecessors contended with "alligators" in the course of their field studies, but today the "alligators" seem to be both more abundant and more hostile. Successful fieldwork requires one to fend off the alligators; strategies for doing so vary from "swamp" to "swamp," and require knowledge of the alligators' personal and cultural characteristics. This is where current academic training displays critical deficiencies. Our mentors may be reluctant to divulge their field strategies for one of three reasons: (1) they are embarrassed to admit that they encountered alligators at all; (2) they lost a few skirmishes with the alligators; or (3) they

prefer that neophyte fieldworkers develop their own techniques for contending with alligators, and, in the process, have a much more "meaningful" field experience. I question, as did Carole Hill in a recent article in Human Organization, the model which "implicitly assumes that fieldwork should be an 'initiation of suffering' in order to learn 'what the anthropologist does'" (1974:411).

I wish to come out of the closet, so to speak, and discuss some of the alligators I confronted during two seasons of contract research with a federal land agency in Alaska in 1976 and 1977. They are fundamental issues also applicable to other types of research and, therefore, warrant more careful consideration than they are often accorded in field research training seminars.

I was hired through an internship program to conduct an oral history project for the Bureau of Land Management (BLM), as one component of a cultural resources inventory being conducted by that agency in Eastern Interior Alaska. The BLM had little previous experience with either interns or social scientists, and viewed me as a contract employee, hired to complete a specific task in a designated period of time. In a 12-week period each summer, my charge was to design a research project conforming to the BLM's general guidelines, conduct the necessary field and archival research, and submit a final report for publication (see Haynes 1976 and 1977). Having had previous anthropological field experience, I knew that working within a limited time framework and under bureaucratic constraints would in themselves be problematic. The unanticipated problems emerging during the course of the summer, however, came as unpleasant surprises but required my immediate attention.

The agency encouraged me to tape record personal interviews with elderly pioneer residents of the region who served as key informants. I also favored this strategy and found most informants amenable to having our conversations recorded despite the lingering memories of Watergate. As a way of protecting both myself and my informants from potential misuses of the taped interviews, I prepared information release forms which authorized use of the tapes only for "scholarly and educational purposes." My concern was that the BLM not permit private use of the tapes for monetary gain or in other historical projects without the knowledge of and approval from my informants. Later the first summer, my immediate supervisor commented on the range of purposes for which the tapes could be used, since they were now "government property." I reminded him of the restrictions that had been imposed on the tapes and threatened to destroy the recordings before subjecting them to potential misuse. I added that, if necessary, I would encourage informants to take legal action against the BLM. At that point I suppose I could have been dismissed from the project and, upon reflection, believe I would have resigned with tapes in hand, had my supervisor and I not agreed upon a careful management plan for the historical recordings. Nevertheless, I taped fewer interviews the second summer and provided informants with duplicate copies of their taped conversations when doing so. My stance did not differ significantly

from that taken by June Nash (1974) in her fieldwork in Bolivia. Nash assured a key informant that she would burn materials entrusted to her before allowing them to fall into the hands of the government.

I conducted most of my research among white pioneers, in part because of their interest in the oral history project, but also because some Athapaskan Indians in the region expressed mild opposition to the fieldwork and considerable hostility toward the BLM. This disappointed and puzzled the BLM, which seemed unaware that their patron-client relationship with the Indians was not conducive to building friendly relations essential to anthropological research. Consequently, I worked with two major handicaps: As an anthropologist and informal representative of a federal agency frequently at odds with the Indians, how could and why should I be trusted? To avoid confrontations and hopefully not endanger future research opportunities, I maintained a low profile when visiting the Athapaskan villages and temporarily set aside plans for conducting research there. I should add that the perceived images of anthropologists held by some Alaskan Natives in the study region and their increasing politicization are giving rise to stereotypes that future fieldworkers may find exceedingly difficult to overcome. Additionally, research activities in rural Alaska have increased dramatically in recent years, while their quality and practicality have been of questionable merit. This, too, will have a direct bearing on the kinds of research (and researchers) permitted in white and native rural communities.

At the beginning of the second field season, an official from the State BLM Office suggested that I alter the focus of my research toward examination of more substantive topics. He sought information about traditional land use patterns which could be used in settling disputes arising between the BLM and Alaskan Natives--one consequence of the complex land distribution procedures stemming from the Alaska Native Claims Settlement Act of 1971. In essence, he was asking me to enter into an undesirable patron-client relationship with local pioneers. I argued that to seek such information would endanger my goals of compiling more general historical information about the region. I unwittingly became the pawn in a power game between the State and local BLM Offices. My first report had been well-received by the local Office and community residents, and may have at least temporarily contributed to improved relations between the two parties. The State Office could take no credit for the success of this project, reacted by criticizing the nature of the research, and called for investigations more applicable to planning at the State level. I sympathized with their needs but found the local Office supportive of my plans to continue less controversial, yet important historical research.

Local reception to fieldworkers often reflects the experiences of community residents with previous researchers, whether they were social scientists, journalists or historians. I had the misfortune to begin fieldwork the second summer in a community portrayed in a

newly-released, best-selling book. The author had resided in the community for several months, gained the confidence of local residents, and then wrote about his impressions of life there. He revealed conflicts between certain segments of the community and quoted opinions expressed by his favorite informants regarding other local residents. I cannot doubt his accuracy in some instances, but the author's failure to disguise his informants or the targets of their criticism greatly disturbed me. He employed actual names when pseudonyms would have sufficed, and not surprisingly, profoundly angered the targets of his criticisms. Fortunately, I had worked briefly in that community the previous summer and had circulated copies of my final report among key informants and other interested residents. When comparing my work to that of the novelist, the locals found me far less threatening and continued to support my work (although in some cases with noticeably less enthusiasm). A first-time fieldworker would probably have received a much cooler reception and encountered major opposition to his or her proposed research.

Other problems also confronted me in the course of this contract research. What was to be done with the raw data not incorporated into final reports? How could I conduct "anthropological" fieldwork within the constraints of a bureaucratic agency? Would my association with the BLM affect my future research plans in the area? These and other questions are not easily resolved, and possible solutions can vary considerably, but this does not mean that we should ignore such important issues and disregard their potential implications.

These are examples of the kinds of problems that can arise in the course of short-term contract research. They also represent topics frequently given little formal attention in academic training programs aimed at preparing students for field research. They may seem trivial to seasoned and veteran fieldworkers, but such issues are the source of considerable anxiety among younger researchers attempting to establish themselves in a highly competitive profession. I suggest that these anxieties can be reduced through the discussion of ethical and other fundamental issues that we all may face in the course of anthropological field research. The case studies presented in the volume by Rynkiewich and Spradley (1976), for example, could serve as the basis for many constructive seminars. The strategy could not be a futile attempt at resolving issues in the classroom, but instead, a consciousness-raising measure directed at better preparing students for fieldwork.

Summer internships or field apprenticeships during the academic year are important training devices for preparing students for more intensive fieldwork. However, they should not be construed as measures by which professors can escape their responsibilities of assisting students in pre-field preparation and in monitoring short-term field placements. Britan (1978), for example, recommends supervised research practica in bureaucratic settings as part of graduate training, since they may become an important

concern of anthropology in the future. Anthropologists are being called upon to engage in research having relevance outside the ivory tower, but can these expectations be met, can we expect to contribute to "real" problems if the traditional "sink or swim" attitude pervades research training? McGoodwin (1978:175) is correct in asserting that much contemporary graduate training for anthropological fieldwork is like "expecting a student pilot to solo without ever having been in an airplane." A colleague of mine added, "and they wonder why a few of us suffer crash landings."

I was fortunate to receive careful guidance from my advisor during my first anthropological field experience in 1972. He contended with a naive farmboy somewhat awed by the Indian reservation setting, and instilled in me a sensitivity to the complexities of cross-cultural research. This did not serve as a substitute for the experience itself--for in some respects it was a painful one--but it did prepare me to more fully appreciate and learn from my initial fieldwork, and to apply this newly found knowledge to more recent field experiences.

To draw the analogy once again, I faced some, but not all the "alligators" that can confront anthropological fieldworkers, and was fortunate enough to defend myself from their hungry jaws. Academic training alone cannot resolve field issues, but it is the appropriate place to learn about "alligators" and "swamps." The successful completion of a major field project, or "draining the swamp," is a formidable task made somewhat less threatening if students gain general knowledge about the "alligators." Who are students to rely on for this knowledge, if not their teachers?

Walter Goldschmidt clearly stated the dilemma facing contemporary anthropology in his Presidential Address before the 75th Anniversary Meeting of the American Anthropological Association:

[Our] sins of omission are the failure to build upon the crude beginnings of the uses of anthropology to illuminate problems that exist in the real and everyday world, both here and abroad.

More importantly, we failed to build the essential infrastructure for the development of a pragmatic anthropology . . . [1977:299-300].

Will Goldschmidt's words become our profession's epitaph? Or, more optimistically, are anthropologists willing to meet his implicit challenge and begin preparing students for their vital roles in the development of a pragmatic anthropology?

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