

**LETTERS TO THE FIELD: REFLECTIONS ON TWO  
DECADES OF CORRESPONDENCE FROM GEORGE FOSTER**

Cynthia Nelson

As I reflect upon the influence that George M. Foster has had (and continues to have) on my personal and professional career in anthropology, I find it most appropriate and natural to concentrate on his own letters to me during the past two decades. Appropriate because it was a letter to me from George that influenced my decision to enter the department of Anthropology at Berkeley in the fall of 1958; and natural because ever since I have known George, the letter has continued to be the medium of much of our communication especially including the fieldwork years in Mexico and my "long-term" sojourn to Egypt.

Twenty-one years ago when my intellectual searches finally led me to the discovery of anthropology, I was in a dilemma regarding where to pursue my doctoral studies. Like many aspiring graduate students in the late fifties I had applied and been accepted to several universities. The gnawing apprehension that this "choice" was to be a crucial life commitment was exacerbated by the cold and formalistic answers I was receiving to my personal letters inquiring about housing, living costs and the general socio-cultural "setting" within which I was to embark on my studies. One day a letter arrived from the Graduate Advisor of the Department of Anthropology at Berkeley. Not only did he address every question that I had raised but he had taken the time to send me several clippings advertising available apartments for rent; to outline the basic kinds of living costs facing a new resident to Berkeley; and to warmly welcome me to the department, suggesting that upon arrival I stop in and see him about possible research assistant activities in the department. That Graduate Advisor was George M. Foster and that letter launched me into one of the most exciting intellectual mileux where anthropology was being taught and fought. It also inaugurated a friendship that has grown and transformed over the years.

What began as the usual student respect and admiration for a professor whose national and international reputation was well known, developed into a craftsman-apprentice relationship in the best tradition of "learning the art" at the side of the master. He was one of the few professors at that time who personally arranged to have his students experience the joys and ardors of fieldwork prior to their longer sojourns to the field. He instilled in his apprentices a respect for and appreciation of the dignity and humanity of our hosts while training us in the painstaking detail of fieldwork. All the while encouraging his students to pursue their own ideas and intuitions. I personally credit him for these values and skills becoming part of my own basic philosophy and repertoire in anthropology. To illustrate the quality of that influence I should like to quote from his monthly letters to me during the period I was undertaking my fieldwork in Erongarícuaro, Michoacan 1960/61. The fact that I have kept these letters over the years suggests the degree to which I still find them meaningful and insightful on a number of levels. For example here are some of his personal views on the experience of field work:

I'm glad fieldwork continues to please. It's the supreme experience of anthropology, something no other discipline can duplicate. If science ever eliminates the traditional field year, anthropology will be the poorer for it. It's something that can't be hurried, and everyone has to live it out for himself (*Letter*, December 6, 1960).

I'm, needless to say, delighted that you feel you haven't made a mistake, and that you find you have learned so much about the field experience I think, too, it is something you find wherever chance sends you, although I have always been glad chance sent me to Mexico rather than to a Chinese enclave in San Francisco or something of the sort. You are having the normal experience in seeing that the field is not all glamor, that it is hard work, at times boring and at times makes one want to scream (*Letter*, February 11, 1961).

I think primate behavior is interesting and valuable, but I don't think primates will replace humans for the social anthropologists who want to understand society, and I think you have to be plunked in the middle of a vigorous society, to be able to understand what it's all about. There are some experiences, such as sex, psychiatry and a close death in the family, that can't be understood vicariously. They have to be experienced. The field experience, as you are now getting it, is one of these things. Hemingway adds the bull fight to the list of things that have to be experienced, and I concur (*Letter*, February 11, 1961).

Good fieldwork is as much art as science, and if you feel less systematic than you think you should be, you more than compensate for your ability to enter into and feel the community. Indeed it is a rich and wonderful and rewarding experience and one that lots of people never achieve (*Letter*, May 5, 1961).

Not only did he consciously and continuously express enthusiasm for the field experience but also his letters were a constant source of insight as to what to be "on the lookout for." He was prodding, perceptive and unyielding when it came to being tenacious in pursuing accurate data. For example, in reacting to my fieldnotes, he wrote:

I was interested in that your one case shows this basic loneliness, which seems to me to be characteristic of so many village Mexicans. Also, A's reason for not getting married—paternal opposition is common in Tzintzuntzan. Again, her emphasis on wanting to work alone is in line with my experience—people don't like group activities. . . Are your birth data your own count or those of an assistant? Tzintzuntzan for last year had practically as many as you have, yet it is appreciably smaller. If the count is not yours maybe you'd better go over the books yourself. If the data are accurate, Erongarfcuaro has a much lower birth rate than Tzintzuntzan and that doesn't make sense (*Letter*, January 16, 1961).

Your notes on the use of banks [are] interesting. This is a very important point, since it seems to me when people begin turning to banks they are on the threshold of great changes in use of capital. It opens the way for investment in a number of things. In Tzintzuntzan money goes into land, and since land is limited, prices go to fantastic heights. The system has to break and you may be in the right place at the right time to find out how it breaks—maybe setting oneself up with a couple of looms is one way out. Of course, bees and chicken afford another (*Letter*, November 7, 1960).

Your compadrazgo data are good and seem to fit my pattern. Try to get lots more on this, since the subject is still wide open as far as it really works. With adequate data you can have a good supplemental article. And, speaking of this, in doing fieldwork it is always wise to bear in mind topics that will make good individual articles, apart from the dissertation, and to make sure data are full enough for this extra treatment (*Letter*, December 9, 1960).

Perhaps what was most valuable to the neophyte fieldworker was George's generosity in sharing his theoretical ideas with his students as he was working them through—even those of us who were thousands of miles away in the field. I remember particularly his letters to me when he was developing the idea of the dyadic contract as an explanatory model of peasant village social structure.

I have been having fun working out a paper for the AAA meetings in Minneapolis the end of next week. "Dyadic reciprocity as a cohesive factor in Tzintzuntzan society." As I have worked over the data I have more and more been impressed by what seems to me to be the elemental basis for social organization. Whatever the institution—family, compadrazgo, friendship, church, etc.—in fact life is organized on a series of *paired contracts* that ego makes, all through life, with the people whom he (or she) works most closely. . . These ties are expressed and given substance by a continuing exchange of tangible goods and services. As long as the exchanges continue, the contract is in force. When they end, even though the institutional tie—compadre, brother, next door neighbor—continues, the relationship is dead. . . I find I see things in Tzintzuntzan when I think of society in this way that I didn't see earlier. For one thing, it helps understand why cooperative work is such a problem—the structure of society, dyadic, just doesn't produce the small groups equally bound together that could be the basis for cooperative work. I started on this line of thought when I began to realize how important it is to offer visitors a bit of cooked food—not all visitors, but just some of them—who turned out to be the dyadic partners in these informal relationships. I am running off a copy of the paper which I shall send under separate cover (*Letter*, November 7, 1960).

I have rewritten the paper I sent you beyond recognition. Little by little it dawned on me that the exchange pattern is not the really significant thing, that this merely validates the important thing, which is that all life is organized (outside the nuclear family) in terms of informal, implicit dyadic contracts, which include people within the family, people in the neighborhood, compadres, relatives, *but not all of the people in any of these categories*, who, in traditional role analysis, all have the same position vis-a-vis ego. It develops into a wonderfully simple model that makes me ask all sorts of questions, the data in a whole series of fields fall into line. For example, what is that vague thing, *personalismo*, except a situation in which two people figure they can get what they want more easily by ignoring the theoretical roles of the system in which they meet? Why won't Mexican villagers cooperate? No two people have the same group of partners so there is no common ground to start people out. On the other hand, the lack of blocks works in another way: we get lots of feuds—which almost by definition are between individuals—but few if any real factions, in the Indian sense. To have factions you have to have functional extended families, unilineal descent groups, or other units that define common positive and negative interests. Sorry I can't go into details, but I hopefully am on to a model for bilateral kinship societies that will be helpful in a whole lot of ways (*Letter*, January 16, 1961).

These letters highlight the development of some of Foster's most enduring theoretical contributions to peasant social organization as well as illustrate those qualities of honesty, integrity and forthrightness—qualities which he has not relinquished over the years. His openness to intellectual dialogue with his students created a sense among us at Berkeley that Foster took us seriously; for he made us feel that we were as much a part of his own intellectual growth and development as he was to ours. It has been this continuous sharing and communication of ideas over the years that has transformed my relationship to George Foster from that of student-apprentice into that of friend and colleague. Despite the long distances between Cairo and Berkeley and my own ethnographic shift from Mexico to the Middle East, George has never ceased to maintain a concerned and keen interest in the trajectory of my own career. On the one hand has been his demonstrated trust in my own professional judgments by his personalized attention in helping my students from the American University in Cairo be accepted and trained at Berkeley.

On the other hand is his ever perceptive and critical evaluation of the canons of honest research. In recent years I have been involved in trying to interpret the role of anthropology in primary health care for the World Health Organization, Eastern Mediterranean region. In 1976 I undertook my first consultancy assignment for this same organization as a technical adviser to study the Primary Health Care Programme in the Sudan. The purpose was to figure out ways and means of developing methodologies, materials and techniques on the training of village level primary health care workers in health education and health promotion. Knowing that Foster was also involved with similar activities, I wrote to him expressing the challenge and frustration at this new type of "research" experience. He honestly answered as follows:

I am delighted that you are being drawn into medical anthropology. The Sudan must have been fun. Yours seems a completely different branch from the one I have been involved in. Most of the people I worked with were in the Division of Strengthening Health Services. The group's report which is recently out, and worth very little, is based on *very* poor research, if you can call it that at all, then dressed up, and emerges as a very authoritative-looking document. Makes me suspicious of *all* such publications. Its purpose is not really research; it is to justify policy previously made—in this case Primary Health Care as the approach to health needs of developing countries (*Letter*, February 1, 1977).

Despite this forthright assessment of the pitfalls of international health services research Foster continues to be committed to the necessity of relating the insights of our discipline to the broader problems of the world. In the fall of 1978 he went to Manila and Kuala Lumpur to teach health educators about the social aspects of changing habits. And although he felt that formal public health education is pretty discouraging and that practitioners can't get it out of their head that "communication" is not all there is to getting people to practice more healthful living.

I struggle—I don't know how successfully—to try to put the idea across that to change people's habits, and to decide whether you ought to try to change them, you've first got to know what they believe and what they do. Seems like an obvious point, but it isn't (*Letter*, January 24, 1979).

Foster's entire academic career has been devoted to the mutual interplay and influence of theoretical and applied anthropology and I as well as the discipline are much more enriched by his efforts. As he recently wrote: "If we can't get anthropology out into the world, we will become a dying science. It is incredible how difficult it is to get some people off their thrones" (*Letter*, September 26, 1978).

As Foster is about to embark on a new stage of his long and illustrious career we may look back upon those "First Thirty Years" as ones of continuous enrichment and productivity. It is incumbent upon us, his students, to sustain the harvest that he has so assiduously cultivated. I, for one, look forward to the "Second Thirty Years" where his collegueship and friendship may be more richly savored in the leisure of his retirement.