CHAPTER 13

Textual Desert – Emotional Oasis: An unconventional confessional dialogue on field experience

Stevan Harrell and Li Xingxing

A confessional dialogue (Stevan Harrell)

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The two essays below constitute a confessional dialogue between two anthropologists, one from the United States of America and the other from China, about an extended field project that has yielded profound emotional connections but very little anthropological analysis. In his witty little book *Tales of the Field* (1988), John van Maanen distinguishes three kinds of ethnographic narratives: realist, confessional and impressionist tales. The realist tale assumes that there is an objective reality of society and culture and attempts to describe them, using 'good' scientific field methods that will lead to good description, both true to the reality of the people studied and useful in advancing the science of anthropology. The impressionist tale, on the other hand, tends toward the literary rather than the scientific, spinning a narrative from the fieldwork experience without much analysis or systematization, and little or no generalization, leaving it to the reader to draw any implications. The confessional tale differs from both of these. Dealing with the process of fieldwork rather than the object of fieldwork, it is "an attempt to explicitly demystify fieldwork or participant-observation by showing how the technique is practiced in the field" (van Maanen 1988, 73). Although the confessional has a long history in anthropology, dating back to Malinowski's diaries (1967), it has become much more ubiquitous since the discipline began questioning both its own objectivity and its colonial past in the 1970s (Asad 1973; Clifford 1988; Geertz 1988).

Van Maanen (1988) points out that the confessional tale ordinarily does not stand on its own. It stands beside a contemporaneous or earlier realist analysis (or less frequently an impressionist narrative) by the same author about the same place and people. Its purpose is usually to document the process of discovery through which the anthropologist passes from confusion and blunders to insight and understanding. The confessional tale becomes a guide for "how to get along and live with grace among the fierce warriors of Gitchi-Gumi, shy hunters of the frozen north, or laid-back winos of Peachtree Plaza" (ibid., 80). Because the confessional tale is about fieldwork and the fieldworker, it cannot avoid the topic of emotions experienced in the field, and in fact "displays of empathy and involvement" serve to enhance the writer's ethnographic authority (ibid., 80; Clifford 1988).

More recently, James Davies (2010) has made a stronger case for including the fieldworker's emotions in accounts of ethnographic practice. He argues that earlier neglect of the emotional aspects of field experiences has contributed to the worship of two opposite and warring false gods: empiricism, the basis of van Maanen's realist narrative, which denies that the emotions of the fieldworker make any difference; and postmodernism, the basis of many impressionist narratives, which argues that since everything is about positionality anyway, emotion can serve as just one more nail in the coffin of the pretenses of science to be objective. Rejecting both empiricism and postmodernism, Davies (2010, 3) makes a claim for the positive role of emotions in ethnographic understanding:

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> It is clear, then, that placing emotion onto an epistemologically relevant plane implies a critique of both streams of thought: firstly, by showing how the concealed and neglected aspects of the researcher's emotional experience can actually present opportunities for understanding; and secondly, by developing a new and re-humanised methodological framework which exposes the weaknesses of the old.

What the analyses of van Maanen and Davies share is the assumption that the role of emotions in fieldwork can be a productive one and that we need to recognize this positive role. They also assume, reasonably enough, that the purpose of recognizing and analyzing the role of emotions in fieldwork is ultimately to legitimate (Clifford would say 'authorize') and strengthen the analyses, scientific or interpretive, that emerge from the fieldwork. As van Maanen (1988, 79) puts it, "Fieldwork confessions nearly always end up supporting whatever realist writing the author may have done and displayed elsewhere…".

But what happens when emotions overcome the scientific or interpretive enterprise and block the construction of any kind of fieldwork narrative? This kind of situation is rarely written about. Van Maanen comments that, "We rarely read of unsuccessful field projects where the research was presumably so personally disastrous to the fieldworker that the study was dropped or failed ever to find its way to publication" (ibid., 79). Li Xingxing and my work has not been personally disastrous – far from it. But over a decade of more or less ethnographic involvement in Yangjuan and Pianshui Villages in the Upper Baiwu Valley, in the mountains of Sichuan, we have published very little. The reasons have to do with both our doubts about our own authority and our emotional connection to the community. As we say at the end of our respective essays in this chapter, we think the blockage is probably temporary; we still intend to

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write in a ‘re-humanized framework’. Yet the blockage is nevertheless profound, and here we present our respective thoughts on how and why it has happened.

I wrote the first essay here after a conversation that Li and I had at a conference in June 2010. He then responded with the second. Both were written in Chinese, and I attempted to translate both my prose and his in the same manner, sticking very closely to the argumentative style and fairly closely to the prose style of the Chinese original.

Textual Desert: Understanding the Relationship Between Researcher and Subjects Through Lack of Analysis

Stevan Harrell, July 2010 (translated from Chinese by Stevan Harrell, February 2011).

This year is the 40th anniversary of my first steps into the anthropological field, the 30th anniversary of the first time I stepped on the soil of the Chinese mainland, the 19th anniversary of the first time I stepped on the soil of Yanyuan in Liangshan, and the 10th anniversary of the establishment of the Yangjuan primary school.

Speaking personally, there have been a lot of opportunities to make the Baiwu Valley (including the four villages of Yangjuan, Pianshui, Zhuchang, and Gangou) into a subject for my anthropological research or publications (Figure 13.1). I wrote quite a few early publications on the subject, including one chapter in *Fieldwork Connections,* the memoir that I co-wrote with Ma Lunzy and Bamo Ayi (Bamo et al. 2007); a chapter in the monograph *Ways of Being Ethnic in Southwest China* (Harrell 2001); an article on elementary education in the valley (Harrell and Ma 1998; Chan and Harrell 2009), and so on. But what is strange is that I have published very

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few articles recently, and they do not really count as anthropological works. Their content is interdisciplinary, mostly concentrated on earth science, particularly forest ecology and geomorphology. There are quite a few field notebooks written full of local history, clan genealogy, religious rituals and other such kinds of anthropological contents. The content of lectures, delivered in response to invitations from scholars in several countries to talk about my recent fieldwork, mostly includes anthropological information about the Upper Baiwu Valley, primarily about folk traditional ecological and environmental knowledge. There are also lectures and articles about the history of Yangjuan Primary School, on the theme of the difficulties and obstacles encountered in running a school. But, really, relatively pure, classical anthropology, such as social structure, folk religious belief, customs and habits, are invisible in my works from recent years.

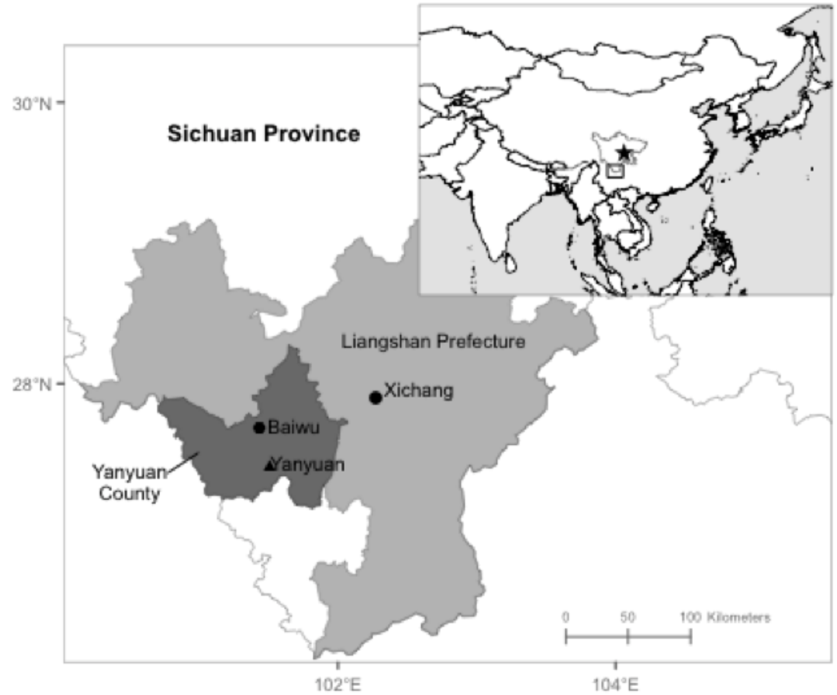

Figure 13.1. Fieldwork locations in the mountains of Sichuan province, China.

Not long ago, I took up this question with my anthropological colleague, long-time friend and collaborator in the establishment of the Yangjuan Primary School, Professor Li Xingxing (see Figure 13.2). Li also indicated that, even though his works from recent years were numerous, with ethnohistorical and anthropological content about Tibetan, Qiang, Ersu and Han communities in various locations (for instance, Li 1994, 2008a, b; Li, Feng and Li 2008), he likewise had very few works about the Upper Baiwu Valley, to the point that, like me, he only had a few miscellaneous documents that did not count as anthropological analyses. After ten years of interaction, with at least one visit every year to the Upper Baiwu area, having put in considerable effort on behalf of the Yangjuan Primary School - including raising funds, establishing the Cool Mountain Education Fund, a non-governmental organization that offers scholarships to graduates of the school, supplementing the salaries of the temporary teachers, and acting as an advisor to the school - we can indicate that we have a certain amount of knowledge about this area, a certain degree of experience, and also that this knowledge and experience is an ideal fit for anthropological analysis. Other than my early research base at Sanxia in Taipei County in the 1970s and 1980s (Harrell 1982), my experience in the Upper Baiwu Valley can count among my field sites as the richest, deepest, the most suitable for anthropological analysis. Works dealing with Xiaoshan in Zhejiang (Harrell et al. 2011) and Yishala in Panzhihua (Zhou, Han, and Harrell 2008) were published early on in my career, and my knowledge of these two sites is really much less than what I know about the Upper Baiwu Valley. So why have I no anthropological works at all about the Upper Baiwu? Why has a ‘textual desert’ appeared?

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Figure 13.2. Li Xingxing (left) and Stevan Harrell during a relaxed moment in Yangjuan Village, 2011. Photo by Ma Vuga.

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We can seek the reason for this ‘textual desert’ from several directions. One possibility is that my knowledge is inadequate. The winding path of fieldwork means that sometimes it is hard to find our direction, so we have to proceed step by step. The path perhaps begins with language. Comparing the linguistic situation in the Upper Baiwu with Sanxia, the difference is very apparent. For my Sanxia research, I spent almost a year in a single village, half of which I was living alone, had no assistant, no translator and my family had gone home; a solitary *adoga* (foreigner) speaking 90 per cent Minnan from morning to night, and only speaking Mandarin or English a small portion of the time. In this kind of linguistic environment, I progressed fast and after half a year I had basically ‘graduated’. If three or four elderly men were sitting together chatting, I could understand the conversation 100 percent. I am afraid that now, my language level at that time has become a personal standard, and anything that does not come up to that standard I mentally count as failure. But it is still hard for me to admit that I am pretending when I imply to academic colleagues that I have the linguistic qualifications to work in Baiwu. Actually, ever since starting at the age of 39, I have earnestly studied Nuosu language in fits and starts, hiring tutors to help more than once. Every time I go to Liangshan, I make the greatest effort possible to speak the local language, and without a doubt I make a little progress year by year, increasing my vocabulary, improving my listening ability, correcting my grammar. When I went to Yangjuan Primary School in 2006, because I had spent the previous year with Ergu Azhi translating the bimo book *Kepu Jjylur*, villagers were surprised that my language had improved so much. Still, up to the present, my project to study Nuosu language has failed. I can only hold the simplest conversations. If two Nuosu are chatting, I basically cannot understand. If I ask a relatively complicated question, I either cannot get the question out or I very awkwardly spit out a pile of nouns and verbs. If by chance my interlocutor understands, she will certainly give a reply that is too complex, so that I will require translation or will only understand a small part of it. At the same time, my fame in Yi Studies circles has spread far and wide: 'American Muga can speak Yi language, other *hxiedie co* (foreigners) can't', making me even more embarrassed. But very stubbornly, I do not dare give up; I keep hoping that someday I will be able to speak fluent Nuosu. Because of this, every time I interview I try to use Nuosu (Figure 13.3). Besides, to speak honestly, using Nuosu language, in addition to practicing to be able to speak, has three other reasons.

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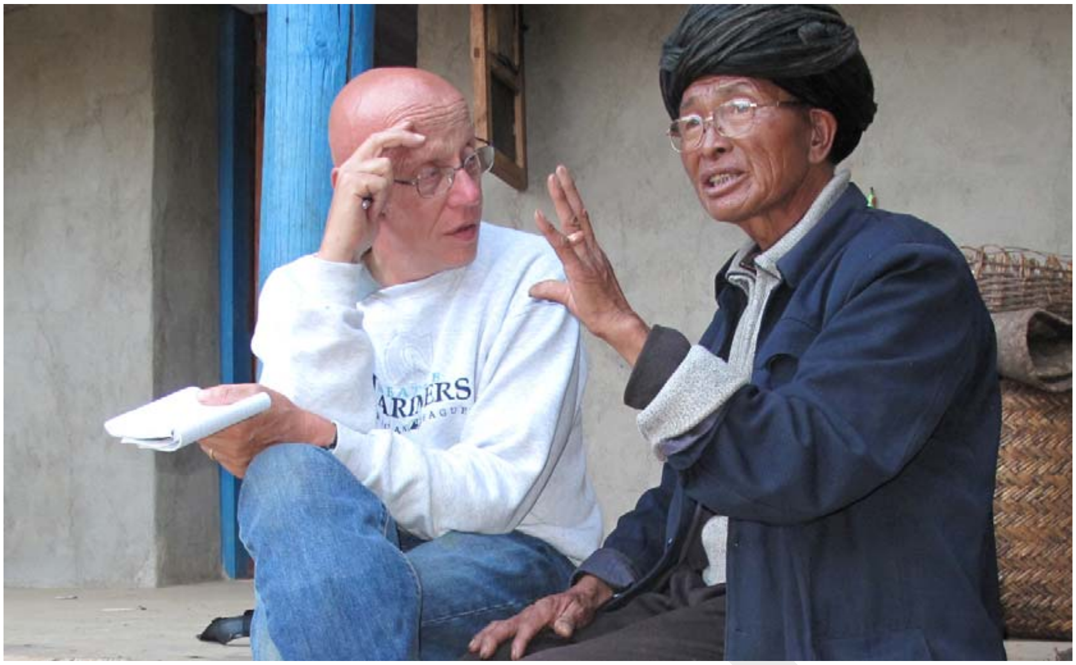

Figure 13.3. Stevan Harrell chats with Yangjuan villager Lygur Gogo, 2011. Photo by Li Xingxing.

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The first is respect. The principles of China as a multiethnic nation include *minzu* equality; they should learn from each other, help each other, including learning each other's languages. But in reality Han people who can speak Yi language are as rare as flowering aloes, frequently talked about and seldom seen. There are probably several reasons, but among them certainly "minority languages are difficult," "Han language is used everywhere," and "what need is there to learn the language—if by chance there is someone who doesn't understand Chinese, you can always find someone to translate," are explanations disrespectful to minorities. For this reason, even though my language is not good, and I have linguistic obstacles to data collection, in my heart I still have the feeling that 'I ought to try'.

The second is my personal prideful nature. Even though 'American Muga can speak Yi language' is exaggerated fame, I still definitely can learn languages faster and more easily than the average person, including the average anthropologist. I am afraid that it is in order to preserve my self-pride over my ‘linguistic gift’ that I continuously try to prove that ‘American Muga can speak Yi language’.

The third is that my listening ability in Liangshan Chinese is really not ideal. Add the fact that many people in the Upper Baiwu Valley speak Chinese not only with a local accent but also with a Nuosu accent, with the result that often whether people are speaking Nousu or speaking Liangshan Chinese, my listening ability is equally lacking. If I cannot understand people’s speech, I feel I have no right to speak, and ought not to be playing the role of an ‘expert’ in local society and culture.

Still, my lack of knowledge is not confined to linguistic or communications obstacles. There are also two methodological weaknesses, one of which can be blamed on the system of scholarship and the political system that lies behind it, and the other, I am afraid, I can only blame myself. The systematic obstacle is the difference I mentioned in *Fieldwork Connections* between ‘fieldwork’ and ‘social investigation’. The former is a basic principle of Western bourgeois anthropology, which began with the legend of Malinowski. According to legend, because Malinowski was an ‘enemy’ alien in the British Empire during the First World War, he was put under ‘house arrest’ on the Trobriand Islands and unable to return to Australia. All he could do was stay on the Islands and diligently research, so he unintentionally invented the method of ‘participant observation’. After this, every Ph.D. candidate in anthropology has had to squat on a point following Malinowski’s example, participating in local life, learning the language, learning to look at local society and culture from the viewpoint of the locals. Despite the fact that later researchers had already discovered that even Malinowski did not live up to his own

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standards for fieldwork (Malinowski 1967; Young 2004), let alone the majority of his successors, his ideal standards still linger in the minds of my generation of anthropologists, and we all think that if we do not come up to Malinowski's ideal standards then we have no right to speak.

The latter, 'social investigation', was born in completely different scholarly and social environments, ever since the massive Chinese national-level project of 'Social and Historical Investigations' in the 1950s. For this project, in order to realize the incorporation of minority peoples into the great family of nationalities, and to promote revolution and development in minority districts, it was necessary to incorporate over 400 ethnic groups into 55 recognized minority *minzu* within two years (Mullaney 2010). Social structure, the stages of historical development, cultural traits; all were recorded, categorized, analyzed, and offered up for units engaged in Nationalities Work to use. In order to record this many social groups and communities in such a short time, there was of course no way to undertake long-term fieldwork in one site or a small number of sites; it was only necessary to invent a method of short-term social investigation. And what I have done in the Upper Baiwu has been short term, rapid investigation. It is undeniable that my notebooks are full of notes, but I always feel they are lacking in depth.

Another kind of methodological deficiency is not a systemic question but is a personal fault. This is that I am afraid to ask, afraid to pursue information. I do not dare disturb people's work time or leisure time, and do not dare intrude into their lives. At the same time I am afraid that people will refuse to answer my questions or will answer with random nonsense. Because of this, I do not actively pursue important information. A lot of anthropologists have this problem -

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even Malinowski was no exception - to the point that stories about squatting in the tent, reading novels and eating sandwiches are very common. Naturally, in short-term investigations anthropologist are not quite so likely to take such radical measures, but the same kind of emotional feelings still happen, and they lead to timidity. Timid, comfortable in the familiar, the result is over-reliance on those few people one knows well. The people I know well in the Upper Baiwu are the agnatic and affinal relatives of my old friend Ma Lunzy, along with some long-term collaborators. In addition, even though there are four villages in the Valley, I am accustomed to living at the Yangjuan Primary School, and the School is not only situated in one of the four natural villages, the houses near the school are these people's houses, adding to my timid tendencies. As a result, not only has my own perspective been overly influenced by those few people, at the same time there are a lot of people I do not even know, or even if I recognize them I hardly ever talk them, interview them or ask for their opinions. My view of the Upper Baiwu is basically the view of the first branch of the Mgebbu Clan and its affines, along with a few members of the second and third branches. Because of this I feel that my knowledge and understanding of this area are extremely limited.

It can be seen from all this that for any anthropological researcher, being subject to these two kinds of limits on knowledge might very possibly induce caution or a lack of self-confidence, or create an obstacle to publishing things as an 'expert'. But the inadequacy of knowledge, even though it is a fact, is far from being able to completely explain the 'textual desert'. In the first place, as I noted above, other than the Sanxia of the 1970s, my knowledge of other places (including today's Sanxia) is even shallower, my understanding is even less deep and less complete. In comparison, my knowledge of the Upper Baiwu is relatively deep and a little more

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complete. We may need to explore another path to find the origins of the ‘textual desert’. The next step in exploration just might begin with emotions.

In 2000, when I put on the ‘Mountain Patterns’ exhibit of Nuosu culture at the Burke Museum of Natural History and Culture at the University of Washington with my two friends Ma Lunzy and Bamo Qubumo (Harrell, Bamo, and Ma 2000), a lot of Yi scholars and officials in China heard about it, including CCTV host Shama Ago. As soon as Ago heard there was a Yi exhibition in America, she made plans to come to Seattle to make a film on the topic of my work in Liangshan, called *“Xin Xi Liang Shan.”*[1] While ‘emotional attachment’ and ‘objectivity’ are considered contradictory, in reality my ‘attachment’ to ‘Liangshan’ should not be an obstacle to analysis or publication. I cannot deny that I identify with ‘Liangshan’, but this identification was not to the point of being an obstacle to publication. In the first place, even though Yi colleagues all thought I was ‘researching Yi culture’, or even ‘helping introduce Yi culture to the world’, this was originally not my intention in conducting research. The only actual experts in understanding Yi culture are Yi. The understandings by us outsiders are too shallow. Yi scholars’ overstated praise of my primitive ability to speak Nuosu can only emphasize the difference between insiders and outsiders. As an anthropologist, my contribution to understanding ‘Yi culture’ can only come from an analytical or comparative perspective. The only obstacle to my writing this kind of article is my feeling that my knowledge is inadequate, that I still need to make further progress in my research; this has absolutely nothing to do with the supposed ‘attachment’, but simply with the fact that I have not had time to learn enough yet.

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[1] 心系凉山. Maybe something like “a heartfelt attachment to Liangshan”?

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No, my emotional attachment is not for the ethnic group, but is rather an attachment to individual people and to the community. When I was writing relatively important works about Liangshan, including *Ways of Being Ethnic in Southwest China* (2001), my ‘attachment’ to the Baiwu Valley or its people had not reached that level of intensity. At that point, the Upper Baiwu was nothing but another fieldsite, it was just another chapter in the book, another case that could be used as comparative evidence. But when we started to talk about establishing the Yangjuan Primary School with Benoît Vermander and Ma Lunzy, my emotional attachment to the Upper Baiwu began to thicken, my ‘attachment’ to individuals and the community began to develop. I remember, in December 1999, when we had basically finished collecting the objects for the ‘Mountain Patterns’ exhibit, I visited Yangjuan and Pianshui with Benoît, Ma Lunzy, Bamo Ayi, and Li Xingxing. The construction of the school had already started, and we sat on a slope under the slanting winter sun eating mutton and honey and drinking beer. I said to Benoît in my primitive French, “presque trop doux” referring not only to the honey but also to the future of the community and the attitude of thanks that local people were showing to us. What we were imagining was being able to renew the community, renew the ethnic group, renew individuals’ relations with mainstream society, to create a sudden earth-and-heaven transforming renewal. Despite the fact that in the intervening eleven years this fantasy has lost its lustre, and the difficulties faced by Nuosu in mainstream society have diminished only a little, still from that day forward emotional attachment has gotten ever thicker, and the possibility of doing objective analysis has gotten ever smaller.

The next time I went to Baiwu was September 2000, for the grand opening ceremony of the Yangjuan Primary School, together with Li Xingxing. On the first evening, Li heard a local girl

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singing a song and asked about her life circumstances. He discovered that her family was poor, her living conditions were wretched, and he immediately decided to adopt the girl as a 'dry daughter'.[2] At the same time Li and I started collecting anthropological data. Individual and household genealogies, agricultural products, the migration history of different groups and so on; we diligently wrote them all down in our notebooks. For the following ten years, I have returned at least once every year, and every time I add more data, every time I meet with the teachers and the principal, talking about school finances, pedagogy, teachers, equipment and other issues. Starting in 2002, I have been taking interdisciplinary research teams, including professors, graduate students, and undergraduates from the University of Washington and Sichuan University in the fields of ecology, botany, archaeology, and geology, to come and research all aspects of the local situation. A representative but by no means complete list of research topics includes the reason for the failure of commercial apple cultivation, the social, economic, and ecological consequences of the introduction of hybrid corn, the relationship between *huajiao* cultivation and the flourishing of Chinese culinary culture, spatial and temporal concepts of local people, choice of curing methods (including doctors, self-treatment, and *bimo* ritualists), ethnoentomology (including insect specimen collections), livestock development and ecological change, the development of yak husbandry, experiences of labour migration, and the relationship between school consolidation and the inequality of educational opportunities. However, articles that have my own name on them have only been three: one on school consolidation (Chan and Harrell 2009) and two on ecology, one dealing with

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[2] The Chinese term 'dry son' or 'dry daughter' (*gan erzi* or *gan nüer*) refers to a fictive kin relationship short of adoption—the dry parent takes a personal interest in and responsibility for the dry child, but does not replace the biological or adoptive parent. It resembles the god-child relationship in European cultures, but is not necessarily begun in infancy; the relationship can be established at any time in childhood.

‘Returning Cropland to Forest’ (Trac et al. 2007) and one with historical changes in ecology (Urgenson et al. 2010). But anthropological ones, particularly on ‘Yi culture’? None at all.

The absence of scholarly results is not, however, the absence of results. In addition to being an advisor to the school, I began in 2004, together with students who had been to the Upper Baiwu, to raise money to support scholarships for Yangjuan graduates going to middle school, to supplement the salaries of temporary teachers, and sometimes to assist the ‘community run with official help’ Yangjuan Primary to meet its various economic obligations. Our method was to design a calendar, using a picture taken by a student or teacher from the University of Washington or Chuan Da for each month, and to sell them for charity in foreign countries. The next year, in order to comply with U.S. legal requirements for non-profit organizations, we established the ‘Cool Mountain Education Fund’ NGO. In its name we have increased our income year by year, as well as the number of scholarships provided to meet the needs of each year’s class of students moving on to middle and high school. We have hopes that in 2011 a few Yangjuan Primary School graduates will be able to enter college.

In this way, on the one hand we have increased our activism, and on the other increased the depth of our emotional connection (compare with Bonnin, this collection). Every year when I visit, it is like returning to a second home. Local people, particularly those Mgebbu and affines of the Mgebbu, are like my own people. Every time I get on the dirt road from Baiwu town to Yangjuan School, my pulse speeds up, and I cannot restrain from looking out the jeep window, are we there yet? Do I see anybody I know? Has the environment changed? If I am taking new people there, I am constantly explaining—that’s Pianshui, we’re about to enter Yangjuan, the

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school's ahead of us, I know that person, that one just now was a friend of mine. When I meet people, "haven't seen you for a long time," "is your health good?" "How was the harvest this year," "Think of you all the time" I spit out in my crude Nuosu. For the first day or two, I hear "Muga, how long have you been here, is your health good, you look so young." After a week, "Muga, when are you leaving," "when are you coming back," "when you're gone, we're not at peace." Also, I need to see everything. The school, people's houses, the valley of Apiladda; if possible I make a trip to the yak pastures up at Zala mountain, to look at the grasslands, to take in the limitless landscape, the snowy mountains in the winter, the late summer crops turning from green to yellow before harvest.

How does one analyze this? How does it become data? How do I write it into an objective, neutral text with so-called scholarly value? It is not impossible; it is rather that I do not feel any pressure, to the point that sometimes when I think of it I feel an aversion. Is objectification the denial of human attachment? Does objectification carry with it the same prejudice toward minorities as the old-style Marxist-Leninist ethnological theory of 'historical evolution of five kinds of modes of production'? Is objectification exploiting one's friends for one's professional development, exploiting one's second home? Also, to speak frankly, I am already past 60, I have hardly any professional pressure, and have earned the freedom of old age. If I have an aversion to objectification or scientific analysis, then forget about it, I do not need to write. Attachment is more important than scholarship; doing practical work is more important than analysis.

But. It is not that simple. I think in the future I will probably write a lot of things. There is a slight possibility that these things might include 'classical' anthropological analysis. A somewhat

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stronger possibility is that I will write things about traditional ecological knowledge, maybe anthropologically inclined, more likely interdisciplinary, combining anthropology and ecology, moving the object focus from people to system. Even more possible is writing a memoir; enlarging the present paper and writing it as a relatively systematic record of feelings and impressions. Perhaps a real method of participant observation requires one to first go through the process of developing ‘attachment’ to individuals, ‘attachment’ to a community, and only afterwards returning to analysis. But this kind of analysis absolutely cannot discard the emotional attachment contained in the relationship. Making anthropology scientific has to be a process that admits emotional attachment, not a process of regularization or objectification. Through the process of developing human attachments, we can move beyond pseudo-science, and begin to invent a true humanistic science. Only in this way can we green the ‘textual desert’. This is probably the most important result so far from my more than ten years of relationship with the Upper Baiwu Valley.

Emotional Oasis: Field Anthropological Self-Reflection on Emotional Attachment and Reactions to It

Li Xingxing 李星星, July 2010 (Translated from Chinese by Stevan Harrell, February 2011)

I am about to welcome the arrival of the first school celebration on the 10th anniversary of the founding of the Yangjuan Primary School. When I was discussing the program for the celebration with Professor Steve Harrell at a conference at Xiamen University, talking about the need to present a report on scientific research, I felt a bit at a loss. I have been active in

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Yangjuan for a full ten years and cannot talk about having any scholarly results. Also, I feel that will continue; it will be difficult to guarantee that there will be any real research. I originally imagined that Steve was ahead of me; how could I have known that our discussions would show that he felt the same way I did? I began to think that this was something that anthropological fieldwork ought to pay some attention to. I felt that our activity in Yangjuan itself ought to receive more attention from anthropologists, that it should not just be us going to do research, but that we need to be researched. At the same time I told Professor Steve Harrell that he ought to write something about this. Professor Harrell had even more impressions than I did, and thus we thought about writing 'Textual Desert', using this bleak title to express feelings that in reality are anything but bleak.

Speaking from our field experiences in Yangjuan, it seems like the longer one undertakes fieldwork, the more difficult it is to realize our original research intentions (for more on this see my work *The Xieluo Tibetans* Li 2008b). I have used this kind of experience to warn students, stating,

> The shorter the period of fieldwork, the easier it is to write articles, and the longer the time the more difficult it is to write articles. Because when the time is short, things are very novel, you have a lot of impressions and a lot of ideas. As soon as the time gets longer, the things you discover you don't know increase, the questions you discover increase, and the more complicated things get, the harder it is to act and put pen to paper.

I have also told students that every time I go back to the same place it is certain that I will have new impressions and new results, and this demonstrates that fieldwork is like an endless cave,

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the longer you stay in there the more likely you are to encounter limitless problems. The impression you get at the time is that they are problems of skills and abilities.

When we had just started going to the field site of Yangjuan—the area that Professor Harrell calls "The Upper Baiwu"—with Professor Ma Lunzy, Professor Harrell designed a collaborative research project. With everyone's agreement I introduced this project at a conference at Zhongshan University in 2001 on "Commemorating the 100th anniversary of anthropology's transmission to China." I introduced the project this way:

> As anthropologists we have happened upon the opportunity to help the village of Yangjuan expand its educational work, and we have already participated in the process of Yangjuan village's response to change. Ma Lunzy, Stevan Harrell, and I have cooperated in establishing a school. And in the environment outside of the school, we have each quite naturally taken things into our own view, incorporated them into our own specialist activity. Afterwards we have begun to proceed together, not each one taking what he wants, each one doing his own thing, but instead making Yangjuan into our common field site, not only on behalf of the construction of the school, but also on behalf of the entire development of Yangjuan, we have together taken on the burden of Yangjuan's problems, together designed our cooperative project, together produced a new experiment in anthropological fieldwork.

Our collaboration has some conspicuous characteristics: Ma Lunzy is a Yi scholar, a member of the Mgebbu clan in Yangjuan, and thus a native scholar. I am a Han scholar whose ancestral home - where I was born - is in north China, while I grew up and have worked in Chengdu. Steve Harrell is an American scholar of a different race from us. You could say that our cooperation is a cooperation of people from different races, citizens of different countries, different *minzu*, and different places. Even though the three of us are friends, even though we are researchers working in the same area and even working in the same field observing the same subjects, even having similar objectives, still in our thinking, behaviour, and feelings, we are all influenced by

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our own social, cultural, and educational backgrounds to various extents. We also exhibit certain limitations determined by our cultural and ethnic psychologies. Thus our observational perspectives and the recognitions and judgments, as well as the way we solve problems, must necessarily express all kinds of individual differences. Facing similar facts, we may each have completely different thought patterns and judgments.

Our collaboration, in addition to being designed with role reversals between investigators and subjects, cannot avoid including our own reciprocal role reversals. As participant observers, we are not only required to establish our own independent work methods, but also need to actively carry out the work of mutual communication, including exchanging information, scholarly dialogue and emotional connection; we need to create mutual understanding. Thus we are all within the purview of each other’s observations. The three of us recognize that we are at the same time observed; we recognize that one person’s observations are subject to the others’ evaluations, and at the same time we offer advice to each other.

We anticipated that, through dissection of the situation of Yi society in Yangjuan at the micro-level, we could understand historical changes in the entire Yi society and culture of Liangshan. At the same time, through experiencing the real life of Yangjuan village, through directly participating in the process of disturbances and responses, we would also necessarily experience changes in our own roles, and to the greatest extent possible make a little contribution to the development of Yi people in Yangjuan village.

From a disciplinary perspective, we of course consider our own collaboration in itself very important. We thought that through this kind of cooperation we could complete a few

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experiments in a few areas. For example, from each of our perspectives, knowledge and judgments, we could derive some reliable information on social and cultural changes in Yangjuan over the last 100 years. We would seek different original causes produced by each of our observations and knowledge, and from our understanding of these causes create critical views and evaluations of our research itself. We were seeking the 'multivocality' and harmony of collaboration. We were seeking the real results of this 'multivocal' collaboration and from these results, the ability to turn around and critically evaluate this kind of collaboration. At the same time, we could also advance our critical understanding of the efficacy of ordinary anthropological methods and of new methods that we had uncovered. An even more profound quest: we could attempt - through the collaboration of scholars of different racial, national, and local backgrounds, through the collaboration of scholars with men and women, old and young, of Yangjuan - to achieve an effort that transcended differences of roles, places, ethnic groups, and cultures.

The introduction of this project looked completely believable. As we went to work in Yangjuan again and again we were untiring. But the voice of this project's research became weaker and weaker as time stretched on, to the point that there was no talk, nobody brought it up. At first glance, the origin of this kind of situation seems to lie in the degree of difficulty of the specialized techniques, produced by the fact that the longer the time, the more things need to be clarified. Actually, this is not it. Factors producing what Steve has called the 'textual desert' are many and complex, and it seems as if all the relevant factors link to a single thing; emotion.

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Thinking about it now, when did this subtle change that happened to us in Yangjuan begin? I cannot immediately say, I just know in my own mind that there were gradual changes in our originally determined research plans. I kept thinking of topics such as ‘I and my Yi daughter’, but planning to talk about this topic and this genre is already no longer purely research. As far as Ma Lunzy’s and Steve’s thoughts, I cannot know them in detail. Only when I read ‘Textual Desert’ from Steve did I realize for sure that Steve did not act because of an ‘aversion’. So I can say for almost certain that no matter what specific events led us to change, the fact that we established some kind of emotional connection with Yangjuan was a firm line that we crossed in our process of change.

Regarding the place of emotion and affect in anthropological fieldwork, I myself think we can proceed from several angles. To begin with, if one is not just initially attempting fieldwork, is not hopelessly biased, and is not obsessed with professional advancement or recognition, then in long-term field activity it is impossible that there will not develop some kind of emotional interaction between observer and objects (or people and their environment), impossible that there will not be something on the order of positive affect. If the time lived in the field is short, host and guest have separate - or we could say opposite - positions. Between them are relations of investigator or collector and investigated or data subject. ‘I’ discover in front of me unlimited fascinations, as if on horseback collecting a huge pile of fresh data that can be arrayed, cut apart, and used any way I want (Figure 13.4). Writings originate from the independence of the ‘I’. Or maybe actually it is not that ‘“I’ temporarily forgot the real

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relationship between ‘me’ and ‘him’,[3] forgot ‘my’ own cultural point of origin, forgot that ‘he’ and ‘I’ alike have their criteria of observation, reflection and evaluation in ‘my’ mental knowledge, and feelings, but rather that the time is just too short, there has not been time to touch the deeper places in the soul. As time gets longer, humanity appears, and ‘I’ cannot but discover the spirit of touching the subject, cannot but discover the affect that is made clear by feelings, cannot but discover the faces of ‘I’ and ‘he’ in the mirror—the reflection of the ‘self.’ In this way, the longer the time, the more one feels the sensation of being in Lushan,[4] the more one feels the sensation of unity in diversity, of the brevity and yet the continuity of life itself. If this causes one to really pursue something about the ‘other,’ then the more conducive the situation is for one to deeply enter the ‘other,’ the more conducive it is to deeply enter those deep and immeasurable parts of the ‘self,’ the more conducive to becoming involved in the whirlpool-like field of ‘affect,’ incurably falling into a black cave that only the spirit can reach, that is endless and overflowing like the spirit itself.

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Speaking from a different world-view, I have this kind of a recognition: the most miraculous thing created by the activity of the creatures of this universe is that human culture can create perception of culture itself, that the spirit can reflect the spirit or have self-awareness. Even more miraculous is that this kind of self-awareness, mirror image or self-recognition can give birth to and mobilize emotion. This kind of emotion has no relationship to daily bodily functions, but is a kind of pure, intangible emotion, such as attachment, bereavement, or love; it is also a kind of emotion not created for the universal spirit that only humans have, unborn and undying, without beginning and without end. Place, culture, language make people produce difference, while ‘emotion’ or what Steve has called ‘attachment’ can cut through differences without obstruction. In Yangjuan, under the late evening starlit sky, every time a certain melody is sung, without using linguistic tools to understand the meaning of the lyrics, it is as if all of us immediately understand each other, all differences are obliterated by the dance melody. This is to say, so-called ‘humanity,’ society, culture, are all unimportant; the only important thing is ‘emotion.’

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[3] Pronouns consternate a translator here. Li Xingxing just has 我 (I, me, my, mine, self) and 他 (he, him, his, sometimes they, them, their, and before 1920 when Chinese guys decided they needed their language to be “modern” and have gender-specific pronouns, formerly also she, her, hers; also “other”). In English, we have to choose.

[4] The original quotation is 不▪▪山真面目，只▪身在此山中, “I can’t know the true face of the Lu Mountains, if only because I am inside the Lu Mountains myself,” from a poem by Su Shi, also known as Su Dongpo.

Finally I am going to return to scientific research. I very much enjoy Ken Wilber’s discussions in *A Brief History of Everything* (1996) about ‘the four quadrants,’ ‘the two hands of God,’ and ‘upper and lower.’ He narrates how science invades other domains. He also points out that the key is in the fact that science, which has an ‘invasive nature’, not only can pursue truth in its own domain, but also ‘issue orders about ultimate reality’, even to the point of denying the existence of other kinds of truth.

I maintain that ‘emotion’ is certainly a kind of ‘ultimate reality’. The longer our time in the field, the more we perceive that the concept of researching ‘the other’ loses its utility. When you open the door of wisdom, ‘the other’ is in yourself. Emotion becomes the place of mutual yielding and mutual knowledge. Here there is often nothing to say about science. Science’s attempt to invade realms runs into a different kind of resistance. Science in the face of fresh life and emotion appears wooden and inert and exudes a powerless limitedness.

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Anthropological research is a stretch of emotional oasis; science cannot completely contain it. If one rigidly wants to fill up the field with analysis and research, then one can only turn the field into a wasteland. The field is a garden that fulfills the spirit, and its ‘ultimate’ reality does not admit scientific interference. The field that can transcend science can serve as an oasis in the anthropologist’s spiritual text. I remember at a recent conference in Xiamen, I used PowerPoint to present my research report in the style of the *Shanhai Jing.*[5] When I said that my report lacked profound scientific research, Professor Wang Chiu-kuei - who coincidentally became my ‘boss’ as the chair of this session - replied “there’s no need for research, there’s no need for research, if you can just go there that’s enough”. He repeated “no need for research” twice in a row. This was not a joke but a very earnest evaluation. Briefly, this was an appreciation of the data I presented; his meaning was that to depend on the data itself that you collected by traveling that route is already enough. If you think about it more deeply, it is equivalent to saying that research is a bonus thing, or even something created by affectation. ‘To go there is enough’ was actually to say ‘you just need to be in real life, it’s enough to be in real life’. Life itself is natural, the tree of life is evergreen. Research, on the contrary, can sometimes become a kind of ruin.

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[5] This ‘Collection of the Mountains and Seas’ is a classical Chinese text over 2200 years old. It is a geographical and cultural account of pre-Qin China as well as a collection of mythology.

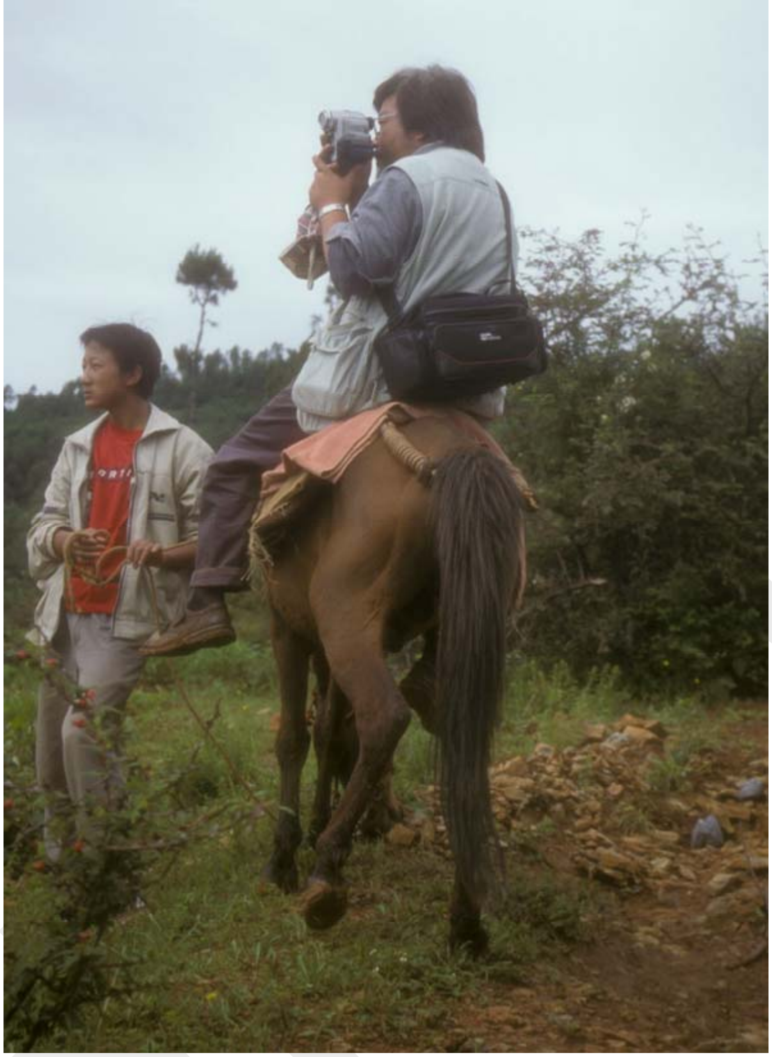

Figure 13.4. A different type of horseback ethnography. Li Xingxing films a trip to the Yak pastures at Zalashan, 2001. Photo by Stevan Harrell.

The anthropologist’s fieldwork is actually just life itself. It is not that one cannot or does not analyze or research, it is just that one cannot ignore the blind spots of analysis and research—emotion and the ‘field of affect.’ The ultimate point expressed in Professor Steve Harrell’s ‘textual desert’ above, ought to be the spiritual feeling he received from thinking deep in his heart, and this ought to receive people’s unrestrainable applause. He notes “Perhaps a real method of participant observation requires one to first go through the process of developing ‘attachment’ to individuals, ‘attachment’ to a community, and only afterwards returning to analysis. But this kind of analysis absolutely cannot discard the emotional attachment contained in the relationship. Making anthropology scientific has to be a process that admits emotional attachment, not a process of regularization or objectification. Through the process of developing human attachments, we can move beyond pseudo-science and begin to invent a true humanistic science”.

What is ‘the process of developing attachment’? Is it not just the process of anthropological fieldwork; ‘only real anthropologists can pass here’? What is ‘making scientific that admits to emotional attachment’? What is ‘a true humanistic science’? This leads me to think of Hugo, Beethoven, and Chaplin; to think of literature, music and the arts. Anthropology is not only an introduction to all humanistic science, it is also a foundation stone. The anthropologist’s field—the emotional oasis—is broad and unbounded, and bears all the spiritual seeds of the human sciences.

Since Professor Harrell and I first stepped into ‘The Upper Baiwu’ or Yangjuan Village, we have been active in the field for over ten years already. Even though we have not been able to realize our initial hopes for a ‘multivocal’ performance, still there is our soul-baring revelation, which could also be thought of as composing a trans-oceanic duet.

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