

**"Vulnerability and Objectivity in the Participant Observation of the Sacred" *Religion and Social Order*
9:225-245 (2001)**

VULNERABILITY AND OBJECTIVITY IN THE PARTICIPANT OBSERVATION OF THE SACRED¹

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*I travel'd thro' a Land of Men
A Land of Men & Women too,
And heard & saw such dreadful things
As cold Earth wanderers never knew
William Blake, The Mental Traveler*

I have spent a part of my professional life as a participant-observer of the sacred in communal contexts. As a result of these adventures, my respect for the power of this methodological technique for understanding religious social experience has grown steadily over the years. However, I have been surprised to note that, in methodology textbooks, the partnership between the participant role and the observer role is sometimes portrayed as relatively unproblematic. Like lemon-meringue or biscuits-and-gravy, the juxtaposition of participation with observation has been portrayed as one of those happy combinations that can be made to coexist easily to the mutual enhancement of each. My own experience, at least in the religious realm, is the opposite. I have found the management of the conflicting demands of participation and observation an unremitting struggle although, I hasten to add, a rewarding one. The personal vulnerability required by what I think of as in-depth participation does not (at least for me) easily coexist with the cool-headed rationality required by the norms of professional objectivity.

The conflict between the demands of these two roles raises a number of issues. It raises the issue of methodological temporality. Is it possible to be both a participant and an observer simultaneously or must one find a way to navigate back and forth between these two roles? It raises the issue of depth of role commitment. Is it desirable to plunge so deeply into the participant role that one becomes, at least temporarily, a true devotee rather than simply a behavioral participant? Is it desirable to let oneself be so deeply stirred by observations that one becomes an active witness (in the religious sense of this word) rather than simply a passively objective observer? It raises the issue of evaluation. Is it possible to bring a critical perspective toward religion to these sorts of projects without becoming an advocate or an enemy of the entity being studied? And can such a critical perspective co-exist with a stance of scholarly objectivity? In this chapter, I will discuss three dimensions upon which these

and a number of other issues have been important to my work— the personal dimension, the methodological dimension, and the ethical dimension.

I do not intend my comments in any way to be hostile or discouraging to the enterprise of participant observation. On the contrary, I would argue that, for the scientific investigation of religious experience, as a social as opposed to a personal phenomenon, it may well be the most preferable methodology. I think that the direct scientific study of deep spiritual and religious experience by this method is possible although it can be very difficult. I disagree, therefore, at least on this one point, with David Yamane who, in an important recent article, has argued that sociologists need to abandon the conceit that religious experience can be directly studied at all, and instead recognize that all we are capable of studying are *accounts* of religious experience:

I argue that when we study religious experience we cannot study “experiencing”-religious experience in real time and its physical, mental, and emotional constituents— and therefore must study retrospective accounts— linguistic representations— of religious experiences. It is in the nature of experiencing and its linguistic expression that the two are loosely coupled and therefore we do not study phenomenological descriptions of experiences but how an experience is made meaningful (Yamane 2000, 173).

Yamane's criticism of much of contemporary study of religious experience is timely and on-target. However, I disagree with his premise that the only techniques available to sociologists studying religion are interviews and questionnaires. If one accepts this premise, his conclusions are basically valid. And I think it is fair to say that Yamane's premise is applicable to the great majority of social scientific studies of religion. But I would argue that there is an alternative method of studying religions that stands a chance of circumventing this problem.

This method is the “maximum feasible immersion” technique of religious ethnography. It is useful, I think, only for studying the shared spiritual life of religious communities, not the internal spiritual life of religious individuals. Its utility rests on the fact that a religious community, in order to communicate its members' experiences among one another, must also provide openings for communicating these experiences to an attuned outsider. This method, at least as I have practiced it, calls on the researcher to immerse himself as fully and sincerely as possible into the beliefs, practices, and relationships of the religion being studied. It obviously does not require a full conversion experience and, therefore, it will always fall somewhat short of complete immersion into the religion. But it does require a sincere openness to the possibility of conversion and a willingness to let what happens, happen.

This technique, in other words, requires the researcher not only to “live with” a group of practicing religionists for a period of time, but more importantly to semi-convert to the religion for a period of time. During this time, one will certainly adopt the rituals and behavioral prescriptions and proscriptions of the group. To the extent possible,

one will also try to see and feel the universe as they see and feel it. Of course, such an attempt can rarely, if ever, be completely successful. But when this technique is partly successful, it may result in the important breakthrough of actually being able to enter the sacred time and the sacred space of the religious group at least for short periods of time. I have found the opportunity to do so enormously valuable in understanding what a religious group is up to. Indeed, at best, such a fleeting experience of sharing this space and time can be as valuable as a zip disk worth of interviews. Its value lies not in affording the opportunity to convey the experience to an academic audience. The experience itself remains largely ineffable. However, its scientific value resides, I think, in the capacity it gives the researcher to validate and contextualize more conventional observations and reported accounts.

Although I am convinced, based on my own ethnographic experience, that penetrating the sacred time and space of others can be done, I would never argue that it is easy or without potentially serious risks. Most of the committedly religious people I've encountered professionally are either amused or offended by the notion that one can put on or take off a religion as if it were a suit of clothes. I have found the suit of clothes analogy not a really accurate metaphor of my own experiences. A religion sincerely adhered to— even for research purposes and even for a short period of time— feels more like a love affair, in that immersion into it changes you, for better or worse, in ways that last well beyond the fleeting experience itself.

An important question is whether it is possible to cling to sincerity and avoid cynicism while engaged in total immersion into a religion. There is something about this method that seems sacrilegious to many people. Suppose one had the opportunity to join the young Jesus and his twelve disciples as a participant-observer and share their experience of sacred time and space? Personally, I would have jumped at the chance. But I can easily understand why many people feel that this stuff is too serious and profound to be contaminated by some scholar's secular-academic quest for truth. I, myself, don't share this feeling because I have never thought there was anything sacrilegious about scientific attempts to understand religious experience by any means possible.

Some would argue that it is not possible to worship God and abide by the comparative-analytical approach at the same time. I think that most religious people (including many admirable religious teachers that I've met) believe that opening one's mind and heart fully to a particular path to the infinite and holy must always go hand-in-hand with closing one's mind firmly and thoroughly to other paths. While these other paths may be, in an objective sense, equally valid, once having chosen a single path, they become distractions or even temptations.

The research technique of maximum feasible immersion into other folks' religions raises serious personal, methodological, and ethical problems. I would like to devote the remainder of this chapter to discussing each of these types of issues in turn, as they have impacted upon my own career in trying to make sense of the religious

experience through the technique of participant observation. But first, to contextualize, this in terms of my own life experience, I had better say some words about the developmental path that has led me to this somewhat idiosyncratic approach to the study of religion.

MY PERSONAL JOURNEY

What got me scientifically interested in religion in the first place was a strong curiosity about charismatic leaders and charismatic authority. This was initially fed by my experience growing up in a family of holocaust survivors morbidly preoccupied with Adolph Hitler and his strange hold on the German people. Obviously, an interest in charisma fed by a focused attention on Hitler is going to produce a more ambivalent attitude toward the subject than one fed by a focused attention on, say, Luther or Gandhi. My sense of charisma as a two edged sword, having the potential to lead charismatic followers to either heaven or hell, was very much in place in me long before my decision to become a sociologist.

I made a strategic decision, early on, that I would do better trying to understand the phenomenon by studying it on a small manageable scale rather than at the level of the mass movement or the nation-state. I initially started out studying small charismatic groups wherever I could find them. Initially, many of these were the extremely transitory political protest movements that flourished in and around Berkeley California when I taught at the University there in the 1960s and 1970s. But, gradually, I came to see that the most interesting of these groups were almost always religious. Since I already had, alongside my fascination with charisma, an abiding curiosity about "the varieties of religious experience" (James 1961), it seemed like a natural step to begin to define myself as a sociologist of religion.

Actually, religion has always terrified me especially when the Spirit is moving and has us in its thrall. I'm drawn to it like a moth to a flame but, so far, a crafty moth who has learned how to fly as close as possible to the flame without getting burned. There is, for me, a powerful counter phobic reward in 'sort-of' becoming a religious fanatic for a while and then finding my way back home to secular humanism once again. As a child I had a number of mystical experiences that were very attractive to me. Even as a very young child I was always fascinated by the knowledge of how easy it would be to slip away entirely from the socially constructed world that parents and teachers called "reality."

This might be a good point to set my own religious beliefs out on the table. Perhaps they may document (or even help to explain) my own pervasive ambivalence concerning religious phenomena. I do not believe in the existence of any god, devil, supernatural being that impacts upon the empirical world in any way we need to be concerned about. This holds also for beliefs about heaven, hell, sin, karma,

reincarnation, or day of judgement. In terms of culturally accepted categories, I would probably best be classified as a secular humanist. However, I do believe in a larger universe outside the boundaries of our senses to which we are fatefully linked, and I believe that we are connected to that world only just barely by the tips of our fingers, so to speak, and must continually struggle to maintain this vital connection. I also believe (along with many Christians) that we live in a world fallen from grace and that this fall is an abnormal and pathological condition that besets all of humanity, and certainly not a condition to which we should just quietly resign ourselves.

Once I got old enough (in graduate school in the 1960s) to discover that one could actually earn a living studying things like this, I was lost. I took to it quickly and it soon came to seem the most natural thing in the world to me to hang out for a period of time in some other folks' religion. The first NRM in which I did prolonged field work was a nudist psychedelic drug cult. This was in my second year of graduate school while I was still taking classes. The results were so weird that I have never published them.

Later, I did participant observation work with a Christian sect, the Bruderhof, which resulted in a monograph called *The Joyful Community* (1980). This monograph expressed fully my ambivalence about the joy that was the most visible product of this community's shared religious life. On the one hand, the joy was very real and capable of producing a positive and heroic attitude toward life not often found in the secular world. On the other hand, I was able to experience for myself how overwhelming this joy could be to an individual's sense of his or her own boundaries. I saw many instances in which the pressure to conform to social expectations was enormous and later bitterly regretted by those who succumbed to it. By personally participating in some of these collective experiences, I was able to see how closely their dynamics resembled the dynamics of the lynch mob or a panicky crowd in a burning theatre.

After this, I branched out into the comparative study of hundreds of different religious groups, some of which were reported on in another book that I wrote called *Alienation and Charisma* (1980). Much of my work with these groups is still ongoing. The great variety of charismatic situations encountered in these groups made me aware of the fluidity and intrinsic instability of charismatic relationships. Although from a distance, it may look as if charismatic relationships are solid and stable, viewed from inside, it is clear that they change dramatically from hour to hour and from day to day and that intense communal love can change to intense communal hatred (and vice versa) in the blinking of any eye.

PERSONAL ISSUES

Attempting to study religion in this way has raised many personal issues for me over the years. In this chapter, I will need to limit myself to a discussion of just four of these: (1) the need to balance emotional and spiritual sensitivity with mental toughness;

(2) self-doubt in negotiating transitions between the mundane and the transcendent; (3) the appropriate degree of candor to practice in explaining one's objectives to the members of the religious body being studied; and (4) problems of re-entry into academic life after a time of sharing religious experience that resist all attempts at full academic explanation. Let me discuss each of these in turn.

When I started my career, I did most of my research solo or with my wife. As I got older and had more resources at my disposal, I found myself spending more time training and supervising graduate student ethnographers. This has enabled me to cover more territory and to attempt to test some of my comparative theories. But, on the whole, I didn't like this change. Training and supervising is a lot less fun than actually doing ethnographic work. In addition, there are new challenges that change the nature of the enterprise itself. One cannot take the physical risks with the lives of students that one might be willing to take with one's own. So there is a shift in the direction of studying tamer groups. Also there is always the risk that a student will "go native."

I have been discouraged by the experience of having two of my graduate students convert to the religious movements they were studying. One actually dropped out of school to devote herself fully to the NRM. The other finished his Ph.D. but never got very far in terms of academic career because he placed the corporate needs of his group foremost. These were adults free to make their own decisions and who is to say that an academic career is more valuable than life within a religious movement anyway. I, myself, was poised a few times at the brink of making a similar decision. Still I felt guilty and continue to feel somewhat guilty about these events especially because neither of the two groups these students joined seemed particularly wholesome to me. I will always wonder whether I fully discharged my responsibilities as a mentor in allowing these students to expose themselves to the risks of maximum feasible immersion before being certain that they were sufficiently self-possessed and cognitively tough to resist the lures of pseudo-spiritual seduction.

A second personal issue has to do with self-doubt. Sometimes I think that I may have learned nothing worth telling from my ethnographic adventures. Some of them have been surprisingly boring. The experience of doing religious ethnography, at its worst, is something like being a house guest at a large family gathering where you know you have been invited because they feel obligated to have you, not because they want to be with you. Such experiences can be awkward and somewhat demeaning. And yet these weeks of unrelenting boredom are punctuated, more often than not, by unexpected moments of joyful and terrifying contact with the holy.

I have mixed memories of my field work. I remember walking alone in a communal cornfield in the moonlight and feeling so close to God that I could have reached out and hugged Him if I had dared. I remember fasting for three days and then dancing for hours with a congregation of whirling dervishes and suddenly, all at once, spinning out from the world and from my own little self and learning for the first time

experientially the meaning of the term "God intoxicated" I remember a Bruderhof lantern festival in mid autumn that helped me to really understand (in my gut as well as in my mind) for the first time why the church is sometimes called the bride of Christ. And I remember marathon yoga and meditation sessions that were so powerful that they felt like they were taking the top of my head off.

But I also remember the many toilet stalls where I furtively took notes, the toilet being the only place in many religious communes where there is privacy. And I remember also all the tedious and repetitive doctrinal lectures that sometimes had to be attended in a posture of alert sincerity when I wanted so desperately to go to sleep and the sometimes mind-numbing ritual practices that often had to be learned before I could be taken at all seriously by a new group. I remember hanging out, way out in the wilderness late one full moon night, with a bunch of ecstatic nature-worshipping hippies and suddenly realizing with horror that they seemed awfully similar in many ways to Charles Manson and his gang.

There is no practical way that I have found to eliminate self-doubt from the participant observation of religion by the maximum feasible immersion method. Sociological research is intrinsically an activity of the profane world. Therefore, the deeper one becomes immersed in the sacred world of the group being studied, the more the underlying epistemological and evidentiary assumptions of profane-world academic activity will come to be seen as questionable at best and ludicrous at worst. If I ever came to feel totally at ease for long periods of time about the validity of the scientific analysis of religion, I would conclude that it was time to retire from this line of work. For me, chronically recurring self-doubt is acceptable as a cost of doing business.

A third issue has to do with candor. I have always disapproved of infiltration as a technique for getting access to a religious movement and I have never practiced it. I have always been up-front from the very beginning (or at least by the time of the second date) about my academic credentials and my academic agenda. Nevertheless, I have never had difficulty getting accepted by the religious groups I have wanted to study. I have often wondered why. I would like to believe that, perhaps, it is because my spiritual sincerity shines through so clearly. But I think more accurately it is because these people tend to see me as spiritually pathetic. Just as some men are reported to evoke in women the urge to mother them, I seem to evoke in gurus and prophets the urge to disciple me. I have a vivid memory of getting ready to leave a religious group with which I'd stayed for a short time. The leader followed me out to the parking lot and said, "You know, Ben, I think you are going to find that university awfully cold and meaningless when you get back to it. You really ought to consider staying here. You are a very very needy soul."

Over the decades, I have carefully nurtured the conditions for mutual trust between me and the groups I have studied. I always ask for feedback, both during and after the fieldwork, concerning me and my research team— how the group members feel

about the questions that we asked and the way we depicted ourselves. Beyond that, I have always protected the confidentiality of the individual research subjects, the religious movements being studied, and the names of their charismatic leaders. That in itself has gone a long way toward building trust. Interestingly, the only time I didn't follow this rule was with the Bruderhof, a religious movement I studied very early in my career, before I realized the importance of protecting confidentiality in this way. I'm sure it is no coincidence that the Bruderhof is the only group I have studied with which my current relationship is strained. The only scientifically valid reason for not practicing this sort of confidentiality that I can think of is to allow other scholars the ability to attempt to replicate one's studies. Of course, for any serious request to do such a replication, I would ask the group's permission to make an exception to this rule of confidentiality.

The related questions of candor and trust will be answered differently by every researcher. I can only say that, for the kind of long term work that I like to do, these issues are very important. And, if in doubt on any specific decision, I will always try to err on the side of the decision that will help to foster long-term trust.

The last personal issue that I want to discuss has to do with the return to academic life after a period of time spent in the field. Returning to the university setting while still seeing it through the eyes of the religious group can be a sobering and discomfiting experience. What Carter has aptly called the life of the serial apostate (Carter 1998, p. 233) can be disorienting at times. Loss of fanaticism is experienced as an emotional loss, which is one reason why, I think, many of those who have once anted-up find it hard to leave the table. But even aside from the emotional bereavement, the appropriate time to disengage from the religious group and return to academic life is not always clear. Snow (1980) has written helpfully on this topic but the ultimate decision is always a judgment call by the ethnographer. More often than not, I am nagged by doubts as to whether I disengaged too quickly. There always seems to be something more that could have been learned.

It has also been discouraging, and quite surprising, to learn that my reports of fieldwork among religious groups have not always been welcomed by some of my academic colleagues in the sociology of religion. The sociology of religion has been plagued by pressures toward advocacy roles in the public arena. Many religious movements have generated well organized counter-movements giving collective voice to people who believe they have been harmed by one or another religious movement. The struggles between religious movements and counter-movements have spilled over into litigation and public policy debates, sometimes quite dramatically. It has been difficult for scholars to resist pressures to align themselves, as "experts," with one or the other of these camps. It has always seemed obvious to me that academic researchers should vigorously resist such pressures because they are sure to be the death of professional objectivity. Sadly, however, the prevailing norm in this academic field seems to be: "If you aren't with us, you're against us." Therefore, my insistence on

strict avoidance of public advocacy positions has left me distrusted by and alienated from both of the polarized camps into which much of this field has been divided.

I sometimes wonder why I so stubbornly persist in swimming against this current of academic fashion. Part of the reason undoubtedly has something to do with arrogance. I have always identified with the epigram by George Bernard Shaw that, "My specialty is being right when other people are wrong." But the greater part of my reason has to do with a conviction that this sort of polarization is toxic for scientific progress and that, therefore, a moderate middle position has to be upheld, even if only at first by a few.

On the whole, at least in my more upbeat moments, I remain firmly convinced that, despite the personal costs, the sort of knowledge of religious meaning systems that one obtains through ethnographic work is deeper and ultimately more valuable than any other that I know. Also, the thrill has never gone away from me, especially the thrill of what I think of is "the ethnographic moment" when one experiences the cognitive and emotional reality of stepping (or falling) into the sacred time and sacred space shared by the group being studied (Eliade 1959). This ethnographic moment is the linch-pin of this sort of research project, but it is impossible to plan for it or to predict when or if it will occur. At least I have never found a way to hurry it or even to facilitate it. The only preparation that works for me is to keep myself in a receptive attitude toward being swept away by this different form of perception.

The scariest thing about the ethnographic moment is watching your research agenda quickly recede into a distant country of laughable irrelevance. For this loss, the only thing I can advise is, let it go! With any luck, it will come back to you immeasurably enriched and deepened. After all, that is the whole point, is it not, of why one would chose to adopt this methodology rather than the more efficient techniques of survey research? Still, there is no way to deny that it is disconcerting to watch what had been so important, and central to one's identity, drop away so quickly and totally.

Religious ethnography, as I have tried to practice it, is often a highly disorienting activity. Entering into other people's religions can be damaging to one's construction of reality. It is hard to think of any analogous occupation that produces the same sort of cognitive and emotional stress. The closest analogy I can think of was the work of a psychiatrist who taught a seminar that I attended in which he entered the world of a different psychotic patient every Wednesday for two hours. For those two hours, each week, this psychiatrist came to empathize with his patient so completely that he became temporarily psychotic. I hope I won't be misunderstood as implying that religion is in any way equivalent to (or reducible to) psychosis. I don't believe that. But it does seem to me that stresses of entering the psychotic world of another are similar to the stresses of entering the intense religious world of another.

Nevertheless, I have never felt more on-target intellectually, in religious ethnographic research, than when I was poised at the very edge of becoming a convert.

I still meditate, from time to time, using techniques I learned doing research in the Sangre de Cristo Mountains of New Mexico. And, when I pray, I most often do so in the attitude of thanksgiving rather than supplication that I learned in the Catskill Mountains with the Bruderhof. There is no doubt in my mind that the experience of doing participant observation in religious groups has changed me in a profound way— I think for the better. But I will always wonder what sort of person I would have become if I had been willing just once to totally, fully, and permanently commit to any one of these religious collectivities.

METHODOLOGICAL ISSUES

I think that our emerging speciality of religious ethnography (Zablocki 1980; Barker 1984; Carter 1990; Van Zandt 1991; Lucas 1995) is a vocation which differs in important ways from the older anthropological vocation of cultural ethnography and even more so from the community based field studies in sociology associated with the Chicago School. I don't mean, of course, that it is completely unique. There are many areas of overlap and I am grateful for much I have learned from my anthropology teachers and colleagues beginning with, but certainly not limited to, my early teacher, the notable Africanist, Neville Dyson-Hudson. However, I believe that a neglect of the ways that the ethnographic study of religion poses unique challenges not faced by other ethnographers has led to some confusion in the practice of religious ethnography.

When I was a graduate student and told my very quantitative sociology professors that I wanted to do ethnographic work for my dissertation, they were sympathetic but at a loss as to how to help me. Naturally enough, they sent me to anthropologists for guidance.² Although these gave me lots of very useful advice, their frame of reference was one of going into the bush for years, not going into the Catskills for months. I leaned heavily on their *modus operandi* at first. But gradually, as I become more confident of my footing, I found myself abandoning important parts of it as not relevant to what I was trying to accomplish.

I think my experience in this regard is fairly typical of that which faces any sociologist who decides to immerse himself in the day-to-day spiritual life of a religious movement. There are no immediately obvious role models in sociology, so we turn to anthropology. As Tom Robbins has put it: "Direct observation is used frequently by sociologists to study new (religious) movements, and the prevalence of this approach represents one way in which the growth of new religions has 'anthropologized' the sociology of religion." (Robbins 1988, p. 15). However, the fit of anthropological methods to the theoretical goals of the sociology of religion has never been a comfortable one because the task of understanding the spiritual practices and beliefs of our compatriots is not the same as the task of understanding the cultural patterns of people strange to us.

But how exactly does religious ethnography differ from cultural ethnography? In order to answer this we need first to free ourselves from the common misconception that the particular form of cultural ethnography practiced by anthropologists is synonymous with ethnography *per se*. Christopher Herbert has demonstrated that, far from being the prototype for all participant-observation methodology, cultural ethnography, as an anthropological method, arose in a specific time and place as a result of a specific set of scientific challenges. The very concept of the fragile and holistic tribal culture, according to Herbert, was a 19th century reaction to the depredations of imperialism and the attendant missionary notion of the deprived native— an “unstable amalgam of empiricism and metaphysics” (Herbert 1991, p. 300).

Herbert goes on to quote two influential writers, one from the early 19th century and the other from the early 20th century who helped shape our notion of the participant-observer's role in an alien culture. First, Harriet Martineau who wrote on this subject as early as 1838:

Her appeal is to the great faculty of sympathy. . . . She lays down as her cardinal rule that the observer must have sympathy [with the people under observation] and his sympathy must be untrammelled and unreserved. . . . She makes it clear that the prized attitude of 'sympathy' can in fact only be defined as a kind of psychological emptying, a voluntary creation of a vacuum into which the ideas and affective forces of a foreign people can rush. . . . This state of perception can scarcely be achieved by an act of will, however; it can only be achieved . . . by taking the vertiginous existential leap of actually living among them and thus to some extent . . . becoming one of them. (Herbert 1991, p. 153-154, 175)

And then, Bronislaw Malinowski, almost a century later:

'This goal is. . . to grasp the native's point of view, his relation to life, to realize his vision of his world.' The mission of ethnography thus defined can be fulfilled only through an act of almost violent self-effacement on its practitioners' part: one needs to divest oneself wholly of European values and prejudices in order to attain sympathetic understanding of one's exotic subjects. (Herbert 1991, p. 151)

Because of this historical legacy of an understandable reaction against the insensitivities of the early 20th century imperialist world view, cultural anthropologists came to define their role in the following terms: sympathize with everything seen in the field and dare to criticize nothing. In this respect, they became the opposite of Joseph Conrad's fictional character, Kurtz who recoils from the brutality of the Congo's primitive culture by exclaiming: “the horror, the horror.” Even such acts as cannibalism, genocide, ritual murder of widows, female infanticide and female circumcision were not to be criticized but were to be understood in the only way they could possibly be understood, in the context of their unique cultural settings.

The tendency to apologize for even the worst excesses of some contemporary new religious movements (NRMs) on the part of some ethnographically oriented religious scholars can perhaps be understood as a carryover from these anti-imperialistic academic norms. To the extent that we model ourselves after a Ruth Benedict or a Margaret Mead, there will be a reluctance, I think in this case misplaced, to be willing to look at any aspect of the life of a religious movement outside of its hermeneutic context. Some scholars of new religious movements, out of allegiance to the norms of classical anthropological field work, have shown a tendency to adopt the religious group being studied as if it were a newly discovered tribe in danger of cultural extinction and to vigorously defend its unusual cultural practices against those who are not able to view them in an appropriate cultural context (Lewis and Melton 1994; Palmer 1994).

But, in identifying too quickly with the interests of the religious group being studied, the religious ethnographer may overlook the enormous differences between the kinds of challenges posed by the two types of ethnography. The cultural ethnographer is faced with the task of living with people with an unfamiliar diet and technology, unfamiliar customs governing every day life, and, usually, a totally unfamiliar language. Simply learning the language is rightly considered a major ethnographic achievement. The religious ethnographer, by way of contrast, is faced with the task of living with people pretty much like herself but practicing a religion that may seem totally alien. (Or, perhaps even more challenging, practicing a religion that is only slightly different from his own). It is one thing to attempt to immerse oneself in that religion as fully as possible and to remain maximally vulnerable to its appeal. It is quite another thing to allow the appeal of religious solidarity to override one's own perception of right and wrong. Maximum feasible immersion should never be confused with maximum feasible credulity.

It is important to recognize that, at least in the early phases of field research, our problems, as religious ethnographers, may be diametrically opposed to those of cultural ethnographers. The core task may be one of analysis rather than synthesis. The greatest challenge may be, not to see how all these apparently arbitrary bits and pieces fit together into an understandable but seamless whole, but to take what looks like a seamless whole and analyze it into its quite diverse components. For example, what looks like a time tested integral religious ritual complex in a NRM may, in fact, involve a mandala from South America, a mantra from Tibet, a Benedictine hierarchy from medieval Europe, all oriented toward a prophecy from the charismatic leader's last acid trip. Hexham and Poewe (1997) have demonstrated that such combinatorics are the rule rather than the exception within contemporary NRMs and I myself have observed them many times. The first task then is recognition of the possibly disparate origins of the religion's component parts. Of course this is not to say that it is not also important to later learn to recognize how these components have been fashioned into a new spiritual whole which, at least to its practitioners, is far greater than the sum of its parts.

Agar has maintained that the two common elements of all ethnographic research are the apprentice role and the search for pattern (Agar 1996, p. 243). I think that, for the religious ethnographer the opportunity to apprentice oneself to an informant is not always as essential as for the cultural ethnographer although often I have found it to be the best way by far of sharing the core of the religious experience. But the importance that many religious ethnographers place on the search for pattern is often done in unthinking mimicry of the anthropological role model. We have all been stirred by the heroic quest of the mid century cultural anthropologists to capture before they were lost forever the shards of what Ruth Benedict (1946) described as the unique, fragile, and irreplaceable "cup" that was that society's culture. But we really must ask whether that quest, however heroic it might have been for vanishing tribal cultures, is really the appropriate model for our own work.

I think it is not. Although the proper vocation of the cultural anthropologist is to discover and appreciate each "cup" of culture in all its fragile uniqueness, a better metaphor for our own vocation is as a "taster of the stew." Frankly, most new religious movements are just too new to be all that fragile or all that unique. Although each NRM is unlike every other in the way it combines its ingredients, the ingredients themselves, for the most part, come from traditional and familiar cupboards. Occasionally there may be a genuinely original message, or prophecy, or ritual, or myth mixed in with all that is borrowed. But this is not as common as the NRMs themselves would have us believe. True religious innovation doesn't happen all that frequently, certainly not a thousand times per decade. Maybe it happens a half a dozen or so times per millennium.

In my work with NRMs I have more and more come to see myself as just this sort of cook tasting stews and trying to discern both their ingredients and the uniqueness of how these ingredients have come to be combined. I think this metaphor is doubly appropriate for the reality of what we encounter when we do ethnography with NRMs. In one sense it is appropriate because of the combinatoric nature of most NRMs discussed above. In another sense, it is appropriate because of the very high rate of membership turnover that characterizes NRMs. It is not at all uncommon to do field research with a NRM and then come back a year later only to find out that a quarter to a half of all the old members have left and a quarter to a half of all the current members weren't there last time. Even granting that the leadership circle tends to turnover its members less rapidly, a religion takes on its flavor not just from its leaders. Each member who leaves and each new person who joins adds an important new flavor to the stew that is the NRM. Methodologically, it is important to avoid the common error of defining a religion solely in terms of its beliefs and not also by its social structure and network of relationships.

The craft of the cultural ethnographer, I conclude, is vastly different from the craft of the religious ethnographer. The former traffic in gestalts. The latter grapple analytically with the mundane consequences of the ineffable. Our problems are different than theirs but no less formidable. For them mastery of the language in all its

nuances is the key. Our lot is to try to understand the consequences of individual and collective experiences that, by their very nature, can never be conveyed in any language. But only by doing so can we make the connections that must be made of the events and structures in those aspects of cult life that can be explained in words—their rituals, their recruitment practices, their sexual deviancies, even their mass suicides.

These observations, if correct, have important implications for how we train graduate students to do religious ethnography. Anthropological texts as Michael Agar's, *The Professional Stranger* (1996) and Ruth Behar's, *The Vulnerable Observer* (1996) are enormously helpful in such training. But we must move beyond them. Some familiarity with methodological techniques for dealing with membership turnover in groups (Price 1977), although of no interest to cultural ethnographers, is clearly indicated. Psychological theories of religious experience (James 1961; Rubin 1994; Beit-Hallahmi 1996) can also be quite useful although these contain their own pitfalls if relied upon too exclusively. But even more important, we need to teach our students to think of themselves less as passive styrofoam containers trained to carry home something very fragile, and more as active gourmards trained to taste concoctions that are rich, baffling, and constantly changing and not to be afraid to speak up critically, moreover, when they find a particular stew to be too salty or too bland for their taste. This too is important data.

However, the critical and evaluative nature of religious ethnography is only half the story. Equally important, and much more difficult, is the maintenance of spiritual and emotional vulnerability. In Bruyn's classical text on participant observation (Bruyn 1966), he correctly identifies a major challenge facing all ethnographers. This is the challenge of striking the proper balance between personal involvement and detachment. He argues that the effective participant observer must fully share in the life experiences of his subjects without ever becoming totally engulfed by these experiences. The problem, however, is that intense religious experiences tend to be oceanic and there is no way to really experience them without being overwhelmed by them. So the ethnographer must always remain somewhat detached, but the religious ethnographer misses the essence of what she is looking for if she insists upon always remaining somewhat detached. This is something of a bind.

The way out of this difficult bind that works best for me is to try to maintain a soft heart and a hard head when confronting powerful religious experiences. Admittedly, this is easier said than done. Practically speaking, what I have found most effective is to promise myself that I will always continue to write analytical memos of the sort suggested by Glaser and Strauss (1967) no matter how irrelevant one's experiences in the field make these appear. I find that these analytic memos constitute a critical link to the temporarily abandoned academic self that I often feel when in the grip of a quasi-conversion experience. In addition, the analytic memos themselves will be a fascinating record of one's inner spiritual life during field work after one has returned to the academic world.

Here is a slightly edited fragment of one such “analytic memo” that I wrote to myself immediately after having surrendered to the mystical, gnostic, neo-Sufi world view of a commune I had been living with for about five weeks. The particular spiritual experience that triggered this was called “the opening” by this particular commune.

It's fair to say that the experiences induced by these religious practices are probably the result of fatigue, meditative disorientation, and peer pressure. But does that make them any less genuine? I felt what I felt and it was real! The Sufi master would say that it doesn't matter what path you take to get to the truth as long as you get there. Logically, I can't deny the possibility that my entire academic career has just been an excuse to get me, indirectly, to a spiritual crisis I would have been too stubborn and rigid to get to directly. There is nothing in this experience that would prevent me from viewing it through an academic perspective. The only trouble is that the academic perspective seems so inadequate to it. My training is in making careful distinctions. But “the opening” makes it clear that all distinctions are just a product of the monkey mind— a holdover from a necessary aspect of our evolutionary heritage that we have long since outgrown. The proof of this is the fact that all distinctions are based on the illusion of either/or thinking. So, once I've transcended this either/or perspective, I'm still totally free to make academic comments about this commune. But how can I? Because anything I say will be true. But its contradiction will be equally true.

ETHICAL ISSUES

Religious ethnography seems to me to lend itself to three potential ethical abuses, each of which can severely limit its methodological utility for studying new religious movements. These abuses are judgmentalism, self deception, and deception by the group being studied. In the final section of this chapter, I would like to say a little about the dangers of each of these as I have experienced them in my research.

Judgmentalism

If taken to extremes, my earlier comments about the ethnographer of religion as a taster of stews rather than as a preserver of cups could destroy the research agenda in the service of criticism. One might risk becoming more of a religious critic than a student of religious meaning systems. This I try hard to avoid because an overly critical stance may impede the kind of vulnerability needed to enter the ethnographic moment and really understand the meaning system being investigated. Avoiding this trap is often a difficult challenge because new religious movements are often surrounded by controversy and contentious elements both within and without.

Most new religious movements that I have encountered are experienced by me as having aspects that are heroic, others that are quietly sincere, others that are

delusional, and still others that are self-serving or even consciously fraudulent. The closer one gets to fully experiencing the religious life of a group, the clearer the distinctions among all of these aspects will become, although the likelihood of significant academic misclassification among these categories will always be high. I think it is useful to try one's best to classify the aspects of a collective religious life according to these categories but one should be extremely wary of evaluating the group itself on the basis of these classifications. If one becomes disillusioned because of the fraudulent aspects, the religious truth encapsulated by other aspects of the group's collective practices and beliefs will never become fully evident. Therefore, it is essential to notice and record the more manipulative aspects of the collectivity being studied, while recognizing that they are nothing more than the inevitable downside of any human enterprise. The religious ethnographer, in my opinion, has no business trying to protect his research subjects by downplaying or explaining away religious malfeasance whenever it is observed. But the ethnographer must be equally wary of using this knowledge to become nothing more than an advocate for schismatic elements within the group or anticult elements opposing the group from the outside. Although it has come to seem a little outdated, there is much still to be said for the norm of scholarly neutrality.

Self Deception

Stone has argued that "new religions often serve as spiritual inkblots: reports of movements may tell us more about the observers than about the observed" (1978, p. 143). My own experiences partially bear this out. I find that I have always been drawn toward those religious groups whose practices encourage dramatic spiritual encounters and altered states of consciousness. Groups of quietistic numerological mystics taking serene pleasure in the concordances that they discover between current world events and numbers mentioned in the Bible have always bored me. Their activities remind me too much of my own statistical work with my own data sets. Such religions are not exotic enough. They do not supply enough of an adrenaline rush. This by itself is not much of a problem. Any ethnographer has a right to choose the sites she wishes to study. It becomes potentially a problem, however, when the very same religious movement engages in activities that I find fascinating and others that I find boring. For all I know, the ones that I find boring may be the more significant to the group itself, although I may overlook them or pay them scant attention.

All this underscores the importance of teamwork in religious ethnography. This is a subject that has not gotten nearly enough attention in discussions of this methodology. I think I share a predilection with many ethnographers for solo work. It's more fun to encounter a new religious group on your own than with a team. But I have come to believe that doing so, most of time, is unacceptable self-indulgence. Even one other partner in the field can help greatly to assure objectivity and to provide a

counterweight to one's inevitable tendency to see in a religious group what you came there wanting to see.

The suggestion that one work with partners assumes that the research partners are capable of disinterested observation and do not bring to the study a prior agenda. Where this is not the case, as Balch [1997 #2139] has pointed out, the mutually reinforcing prejudice of the partners can actually work against objectivity. In the example cited by Balch, a minority of researchers on a team of scholars, who saw things differently from the majority, were put under tremendous pressure to conform to majority opinion. This well known phenomenon, known as groupthink (Janis 1982), is a danger especially in the encapsulated cognitive environments that are characteristic of many new religious movements. Teams of researchers going into the field would do well to build into their operating procedures specific mechanisms for protecting the voices of minority dissenters.

Deception by the NRM

Potentially even more damaging is the potentiality that a religious group will pull the wool over your eyes. It's been known to happen (Yablonsky 1968). As Ayella has cautioned: "Most important, the researcher should be critical of access, asking the question of why the group has allowed the researcher in..... One should question the kind of access one is being given, ever conscious of the possibility of sanitization or impression management." (Ayella 1990, p. 565)

Religious communes are often so tightly orchestrated that it doesn't take much skill to give a gullible ethnographer what appears to be full unrestricted run of the place while still making sure that she sees only what you want her to see and talks to only those who you want her to talk to. When we do religious ethnography, we can never be entirely sure that we are not innocently tiptoeing through the tulips, seeing only the idyllic side of the religious life totally oblivious to the serious human rights abuses that are simultaneously occurring, to switch metaphors, below deck.

Such deception can be exacerbated by the very empathic identification with the group we generally try so hard to achieve. Robbins has argued "Direct observation tends to sensitize the careful observer to changes in a movement as well as to internal conflicts, factions, geographical variations.... Yet...the empathist may end up adapting the 'official' or organizational definition of reality and become desensitized to coercive, manipulative, and other ironic nuances in social processes within a movement." (Robbins 1988, p. 15-16).

The only way I know of to be reasonably sure of avoiding this sort of deception is the practice of careful triangulation of data sources. Fortunately, many religious movements lend themselves quite easily to such triangulation. As I mentioned earlier, many religious movements, especially those that have been around for a decade or more, have spawned oppositional counter-movements. Three quite different sources of data are readily available: the insider's perspective of the movement adherent, the

insider's perspective of movement opponent, and the outsider's perspective of the academic participant observer. When any one of these perspectives tends to disagree with the other two, this is a clue that there may be something very interesting going on beneath the level of naive appearances.

CONCLUSIONS

In conclusion, I would argue that, despite its drawbacks, religious ethnography remains an important methodology for studying new religious movements. A student of religion willing to risk his own construction of reality, backed up by a reliable support team, and willing to spend sufficient time (usually months or years, not days or weeks) in the field will be able to learn more about religion using this methodology than any other I know. This is especially true when this method is supplemented by the simultaneous use of more traditional research methods such as in-depth interviewing and standardized questionnaires. I have always tried to bring multiple research techniques to bear upon the study of new religious movements. Although I remain committed to participant observation as a method of choice, I have never been willing to rely on it alone because of its intrinsic dangers of subjective distortion.

Religious ethnography, using the techniques of maximum feasible immersion, cannot be engaged in without a willingness to assume significant personal risks. One must be prepared to be profoundly-- even permanently-- changed by the ethnographic experience. Otherwise, the experience will inevitably be viewed, both by oneself and others, as partially insincere. Cultural ethnography makes great intellectual and physical demands on the researcher. The religious ethnographer experiences these obstacles while, as well, having to deal with intrinsic emotional and spiritual assaults upon self-conception. The greatest challenge is to maintain a balance of vulnerability and objectivity.

As I have tried to show, there are significant methodological obstacles as well. The vulnerability of the researcher, along with the extraordinary degree of milieu control and impression management afforded by the totalistic structure of many NRMs leads to the potential of serious methodological pitfalls centering on issues of deception. Therefore, I have come to believe that religious ethnography is best practiced in groups of two or more people with robust personal identities and strong existing ties to significant others outside the collectivity being studied. Such teams should ideally have the luxury of being able to spend significant amounts of time totally immersed in the world of the NRM with as few mundane distractions as possible.

Finally, even when the researcher is able to overcome these personal and methodological obstacles, I have found there to be significant ethical issues that begin to surface even before the research begins and that linger after the research is completed. For me, these ethical issues have been focused on problems of deception

and problems of judgmentalism. I have found the period before beginning research very delicate. While naturally wanting to present myself to the religious community in the best possible light, I have found it important to do so without deceiving the potential research subjects about my research agenda. Moreover, temptations to deceive do not disappear when permission to do the study has been granted by the religious group. They continue throughout the life of the research project. After beginning research, these problems are added to by the opposite problem: the danger of being deceived by the religious community. All religious communities that I have encountered are concerned about their public images. Many will consciously or unconsciously attempt to portray themselves to the researcher in the best possible light in order to enhance these public images. I always find that, the more I immerse myself into the spiritual life of a community, the more inclined I become to want to accept its presentation of self uncritically. I have always found it very hard but very necessary to resist this inclination.

Even after the research is completed, the ethical issue of judgmentalism lingers on. I have found the line between objective criticism and moral judgment to be a tricky one. It is made especially so by the highly polarized structure of the sociology of religion field. I find that I need to keep reminding myself that maximum feasible immersion into a religion falls far short of complete immersion. Maximum feasible immersion does allow me to see, with what I hope is some degree of accuracy, both the positive and negative elements that always coexist in a religious social structure. But maximum feasible immersion can never turn an outsider like me into a real insider. Recognizing that, at best, my account of a community's religious life will be one of a privileged outsider helps me to try my best to avoid advocacy roles either in support of a religious group or in support of the group's opponents. I wish I could say that it has given me an enduring sense of humility about the subject I study. But, at most, it has given me a tenuous hold on an attitude of epistemological humility which deserts me at times and which I have had to continually struggle to regain.

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NOTES

1. I want to thank two dear friends and colleagues, Lew Carter and David Bromley, for their inspiration and help in the writing of this chapter. I found the continual introspection required for this account to be very difficult and anxiety-producing. I doubt very much if I would have seen it through to the end without the wise encouragement and the practical suggestions of these two scholars.

2. I do not mean to imply by this that there is not a long and healthy tradition of ethnographic research within sociology itself. Urban ethnography was and is a key feature of the Chicago School of sociology and, according to Snow (Snow, David A. 1999). *Assessing the Ways in Which Qualitative/Ethnographic Research Contributes to Social Psychology. Social Psychology Quarterly* 62:97-100., ethnography is currently the most influential of all the methodologies that constitute the area known as "qualitative methods" in sociology. However, in terms of the issues being discussed in this chapter, these ethnographies can be assumed under the general category of cultural ethnography.