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Scientific Empathy, American Buddhism, and the Ethnography of Religion

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introduction

In a portrayal of the relations between ancient science, myth, and contemporary science, Claude Lévi-Strauss in The Savage Mind presents us with the image of the *bricoleur*, the scientist ‘of the concrete.’ ‘Adept at performing a large number of diverse tasks’ (Lévi-Strauss, 1966: 17), this jack-of-all-trades will use whatever means already lay at hand to complete her project, rather than obtaining or inventing project-specific tools as a modern engineer might do. For the *bricoleur*, any undertaking first involves a retrospective accounting of all possible tools and materials already pre-existent in her/his inventory, which s/he engages ‘in a sort of dialogue’ to ‘index the possible answers which the whole set can offer to his problem’ (ibid.: 18). ‘He interrogates all the heterogeneous objects of which his treasury is composed to discover what each of them could ‘signify’ and so contribute to the definition of a set which has yet to materialize.’ With such a strategy the *bricoleur* may be as successful as an engineer at completing a task. At any rate, the *bricoleur* always puts ‘something of himself into it’ (ibid.: 21).

Such *bricolage* may be taken as a trope for understanding some current efforts in the ethnographic study of religion. More and more scholars of religion, especially American religion, recently have turned to ethnography as an alternative to the library for the collection of data (Tweed, 2002a: 64). This interest in ethnography is attested to in a spate of reasonably new ethnographies, commentary in works such as Bromley and Carter (2001) and the recent Personal

Knowledge and Beyond: Reshaping the Ethnography of Religion, and anecdotal evidence. Julie Ingersoll (2002: 162) connects this rise in fieldwork to the rise in concern with women's issues. Yet, as with the *bricoleur*, there is a curious quality of methodological fumbling in the dark with regards to this experimentation with ethnography. Some recent ethnographers of religion lack substantial critical-methodological components in their work, as if the authors believe that simply arriving in the field and asking questions provides firm knowledge on its own. Other ethnographers, conscious of epistemological issues, have generally turned to reflexive models of ethnography borrowed from or inspired by their anthropological colleagues. Unfortunately, even among anthropologists, the constitution, epistemological weight, and ethical valence of reflexive ethnographies remain in hot debate, even while experimentation with reflexive ethnography exists pervasively. Much of the confusion derives from the nature of reflexive ethnography itself. Arising from postmodern theories of knowledge and being responsive, and hence 'reflexive,' to the being-in-the-world both of the people at the field site and of the ethnographer, reflexive ethnography resists standardization.¹ If one includes methodological critiques of the genre, it seems as if there are as many or more *bricolage* theories and practices of reflexive ethnography as there are reflexive ethnographies themselves.

One common feature of the genre of reflexive ethnography, however, is the concept of empathy. Reflexive ethnographers repeatedly speak of the need for the researcher to have empathy for the researched so that the ethnographic project can take into account invidious differentials of power or avoid the moral blindness associated with older, positivist, colonial modes of field research (see, among many, Spiro, 1992; Kleinman and Copp, 1993; and Marcus 1998). Alas, what is meant by 'empathy' in these presentations usually remains vague, unexamined, and unrefined, leading to a morass in which a major methodological tool of the genre is poorly un-

derstood and hence under-utilized. As I will describe, the use of empathy, properly grounded, can be a powerful tool for collecting more data, and more reliable data, in the field. In particular, I will describe how the use of empathy deepened the comprehension of the metaphysical and ethical dimensions of the lives of American Buddhists while it dissolved obstacles to data collection arising from the side of the researcher. Unfortunately, although reflexive ethnography is specifically conceived to avoid exploiting field subjects, the ethics of ethnography are not always straightforward. Employing empathy in reflexive ethnography raised thorny ethical questions that deserve discussion. These dimensions of the scientific use of empathy are explored in this article, which begins with the experimental ethnographic context into which reflexive ethnography fits.

methods of reflexive ethnography and empathy

Reflexive ethnographic methods are the rebellious heirs to older, classical methods of ethnography as developed in the early twentieth century and epitomized by Malinowski's (1922) 'participant observation' approach in the first chapter of Argonauts of the Western Pacific. This method of participant observation grounded anthropology's twin tasks of portraying a foreign culture to a Western audience in its unfamiliar character, while also making this unfamiliar character seem familiar to the same Western audience. This portrayal, however, needed to carry the weight of social science methods and theories so that anthropological ethnographies would be more authoritative than the genres of journalism and travelogue. Ethnographies pursued this end through two allied methodological strategies (Rabinow, 1986: 244). The first strategy evoked the ethnographer's authority due to the fact of the ethnographer's 'being there' in the field with 'the natives,' so that data were reliable because they were collected directly by the ethnographer

herself. This dimension may be seen in a variety of experiential statements, usually relegated to prefaces and footnotes. The second strategy involved the suppression of this first-person voice in the body of the text, which instead relied on seemingly objective, third-person statements like ‘The Ojibwa believe...’ Through these rhetorical strategies readers were led to believe, at least ideally, that a credentialed scientist was there to collect the data yet did not personally contaminate it.

Many writers in recent years have attacked the pretenses and fallacies of this ethnographic attitude. Vincent Crapanzano has been a leading voice in this critique. According to Crapanzano, this ethnographic paradigm is impossible to fulfill, either in the field or in textual construction, because each ethnographic project arises in the context of Western social scientific pursuit, yet it demands the depiction of a foreign world presumably on its own terms. Therefore, the ethnographer must be a Western social scientist and an unscientific native all at once. From this, for Crapanzano the ethnographer must play the role of a rhetorical ‘trickster’ in order to create a convincing, authoritative text (Crapanzano, 1986: 52). Through a critique of ethnographic texts of George Catlin, Johann Wolfgang von Goethe, and Clifford Geertz, Crapanzano reveals these rhetorical ‘trickster’ strategies which, while enriching and enlivening the text, preclude any pretense of representing positivist science or even truly representing the point of view of field subjects. Compounding this, the classical paradigm inherently involves a static, ahistorical picture of a complete social world, whereas the ethnographic encounter is always diachronic, processual, and fragmented (ibid.: 51), and societies can never be experienced in their totality by any one human being (Thornton, 1992: 18-19). The effacement of the ethnographer from the text produced by these classical methods thus is not objective science, but in fact represents a practice of ‘bad faith’ (Crapanzano, 1980: ix).

Many other scholars attack the classical paradigm of ethnography for its practice of the elimination of the ethnographer in data collection and finished text, for its ‘prejudice against prejudice’ (Nowak, 1984: 171). Dominant among concerns in this critique is the need to pay attention to the dialogic, not monologic, nature of the ethnographic project. The elimination of the ethnographer, intended to create an aura of authoritative objectivity, is in fact scientifically false (Rabinow, 1986: 245), as it creates an ‘illusion of objectivity without involvement’ (Herdt and Stoller, 1990: 24). With its monologic structure, the classical paradigm fails to make researcher bias, prior understandings, and prejudices an explicit part of the research process. Erasing the researcher from the ethnographic project obscures subjectivity in the research design, the research setting, the data collected, and the final report. An unfortunate residue of nineteenth century positivism, the classical ethnographic paradigm therefore is epistemologically untrustworthy and naïve. This critique insists that ethnographic epistemology be brought in line with other epistemological paradigms such as Heisenberg’s uncertainty principle in the natural sciences or Gadamer’s hermeneutic approach to the social sciences. From a more postmodern perspective, it is more scientific to highlight the dialogic encounter between the researcher and the researched both in the field and in the final report by revealing the ethnographer’s participation in both data collection and write-up. A reflexive self-consciousness on the part of the researcher is essential for textual, social scientific authority, rather than an obstacle to such authority, hence the term ‘reflexive ethnography.’

James Clifford has been instrumental in pushing this critique further. To challenge the ‘bad faith’ objectivity claimed by classical ethnographies as well as to indicate new directions for paradigms, Clifford (1986b) suggests that ethnographic texts are, in both form and content, allegorical. For Clifford, cultural anthropology’s principal process of rendering the foreign as famil-

iar to Western audiences could not be anything other than allegorical. In order to gain familiarity with the material, readers must understand foreign artifacts through allegorical comparison with their own culture, so that for the reader of ethnographies allegory is a required mode. Since ethnographic texts as read are allegorical, they are also inherently multivocal. This multivocal, allegorical nature of ethnographic texts precludes any hope of a final, settled reading or positivist truth for such texts. And this allegorical moment, for Clifford, arises not just between the reader and the final text, but inheres in textual production itself. Data collection also is allegorical for the researcher, as data collection decisions, like textual readings, demand allegorical comparison.

Due to their allegorical nature, for Clifford ethnographies always involve the invention, not representation, of cultures (Clifford, 1986a: 2). Untainted, pure representation is impossible because the encounter with field subjects is dialogic, textual production is dialogic, and textual meaning and authority are dialogic. Instead, ethnographies present unique cultural experiences deriving from the interactions of researched-researcher-text-reader. This, however, does not diminish the claim of ethnographies to scientific authority within the academy. Ethnographies are approached scientifically in terms of possessing competing voices. Scientific authority arises precisely because of the multivocal nature of the text as well as the dialogue of the text with other ethnographies. Authority is dislodged from any one individual, but rather arises in the encounter of multivocal researched-researcher-text-competing text-reader, so that reflexive ethnographies are ethnographies of ‘dispersed authority’ (Marcus and Cushman, 1982: 24). Popperian falsifiability is achieved by ‘the conflict between competing interpretations’ (Ricoeur, 1981: 213). Scientific authority results from the reflexive attention paid by the ethnographer to this multivocality and interpersonal implication at every level of project design, execution, and presentation.

The reflexive mode of ethnography clearly recognizes that data are always rich with the private intentions and meanings of the ethnographer (Herdt and Stoller, 1990: 34), so that this method of ethnography attempts to exploit the relation between researcher and researched to positive scientific effect. This may be accomplished through the use of empathy as a social scientific tool, following the lead of Max Weber. Foreshadowing Heisenberg, in his seminal essay 'Objectivity in Social Science and Social Policy' (1904), Weber claims that an objective scientific analysis of culture is impossible. For Weber all sciences use choices and concepts to predetermine their fields of data within the infinite variety of possible gatherable data, and therefore all sciences lack presuppositionless objectivity. This inherently interactive nature of all research, for Weber, is even more true of the social sciences. Culture, the principal object of social science research, is not objective for Weber, as culture itself is a 'value-concept' lacking in empirical existence. All knowledge of culture derives from 'particular points of view' (Weber, 1904: 81) and thus always involves the 'investigator's evaluative ideas' (ibid.: 82). Therefore social science epistemology inherently implicates the researcher with the researched, and the basic data of the social sciences arise from the empathic involvement of the researcher with the researched. Empathy thus provides the foundation for Weber's *verstehen* method. Without empathy there is no social science because data derive solely from empathy. For Weber, this empathy makes the social sciences more scientific, not less. Naturally, this is a disciplined empathy, as the involvement is pursued to scientific ends. Empathic involvement provides the method of social scientific data collection, and then data are then oriented to causal explanations as found in the natural sciences.

To hone Weber's concept of empathy into a useful ethnographic tool, I suggest the use of the formulations of empathy found in the work of the psychologist Heinz Kohut. Kohut's theory

of empathy is the most sophisticated and developed of any to be found in the corpus of psychoanalytic writings, as empathy is central to Kohut's revisionist psychoanalytic theories and practices regarding health, pathology, and the analytic process. As well, Kohut's understanding of empathy as a scientific tool parallels the understanding of social science as found in the reflexive ethnographic theories I have discussed. Kohutian empathy therefore dovetails seamlessly with reflexive ethnographic metatheory.

Kohut defines empathy in several places in his sometimes posthumously published work. His most common definition is that empathy is 'vicarious introspection.' Augmenting this formulation is that empathy is 'the capacity to think or feel oneself into the inner life of another person' (Kohut, 1984: 82), which resembles Freud's use of *Einfühlung*, to 'feel oneself into somebody' (Wolf, 1983: 309). Kohut's most precise definition, and the one used in the ethnographic section of this essay, is that empathy is 'cognition through the narcissistic investment of the other' (Kohut, 1978b: 701).

As with Weber, for Kohut social scientific data only arise from empathy, and the more the social sciences stray from empathy, the more unscientific they become (Kohut, 1990: 88). This empathy, as a research tool and used for scientific purposes, should never be confused with compassion or sympathy. For Kohut compassion seeks the benefit of another person whereas scientific empathy bears no such aim. In fact, according to Kohut, empathy may be used to the detriment of the other. To this end he offers as an example the skillful use of Nazi radio propaganda in 1930's Germany, which was effective because of its empathic tone and content yet failed in terms of beneficial effects.² As a scientific tool both compassion, on one side, and fault-finding, on the other, must be eliminated from empathy, which then exists as an attitude of 'evenly hovering attention' outside of purely subjective introspection or intuition.

This ‘evenly hovering attention,’ without compassion or fault-finding, must derive from a reflexive self-awareness on the part of the researcher. The Kohutian concept of empathy inherently recognizes the implication of the observer with the observed (Kohut, 1980: 483-487). Describing this specifically in terms of psychoanalytic practice Kohut says, ‘Objective truths must always include an assessment of the observer, [so that] we must, in particular, recognize our countertransferences³ and thus minimize the influence of factors which distort our perceptions of the analysand’s communications and his personality’ (Kohut, 1984: 36-37).

To employ such reflexivity the researcher must train in the use of empathy, just as any scientific procedure requires training (Kohut, 1978b: 701). Failures or mistakes in empathy are common occurrences both inside and outside of the research process and the use of empathy demands experience and learning from mistakes. The researcher becomes more attuned as the research progresses, a dynamic found even within classical paradigms of ethnography, as one learns over time what is important to the researched people and the research project. Perfect empathy is not possible but, according to Ernest Wolf, is not necessary. More important than empathic success is maintaining an empathic ambience in the research situation (Wolf, 1983: 314). The empathic effort alone may encourage research subjects to offer data, although there may be resistance at first (Kohut, 1978a: 486). And, as I will discuss, empathic mistakes can be quite revealing of data about the researched as well as of impedances to data collection arising from the researcher. The effort for empathy is what is required, and research only fails when this effort is lacking.

This understanding of the role of empathy in the social scientific process is extended and enriched in the work of Peter Homans (1989). Drawing on Weber, Kohut, and D.W. Winnicott, Homans suggests that social science research is essentially a fantasy activity. Ricoeur (1981:

256) claimed, 'There is thus a correspondence between the extension of the investigatory procedure and what could be termed the space of fantasy,' and Homans concurs. That is, for Homans the data of the social sciences arise in the psychological 'intermediate space' between the observer and the observed, just as all of our interactions with people arise in this metapsychological locus. Standing between personal inner and outer, fantasy and reality, this intermediate space is the realm of creative imagination. The intermediate space, standing between self and other, also contains the interaction of culture and nature, which for Homans is precisely what the social sciences intend to study. Therefore social scientific projects develop from beginning to end in the intermediate space between researcher and researched and the basic data of all social science pursuits are fantasy-laden data. Ethnographically, both participation and observation represent fantasy-laden activities (Homans, 1989: 340).⁴ Ethnographic projects therefore lack notions of objectivity found in the classical paradigm. Instead, they embody the sum of interactions between researcher and researched, including the psychological baggage from both researcher and researched that is brought to their shared experiences.

From this, Homans' formulations help us to understand what is meant by the use of empathy in reflexive participant observation. The intermediate space between researcher and researched is the locus of empathy for the other, and only through empathic *verstehen* in the intermediate space can the researcher understand the researched. Detachment should be eschewed as the researcher empathically enters the world of the researched, thus fulfilling the role of social science as the science of human meaning. As with Kohut, for Homans this empathy is not to be confused with sympathy or compassion. For data collection to occur properly, the researcher must be aware of what she might bring to a given research situation, and on this basis find an empathic attitude of 'evenly hovering attention' devoid of sympathy or fault-finding. After this

research moment of experience-near empathic data collection, there follows a research moment of more experience-distant theoretical reflection on that data. During this latter, theory-creation moment, empathy for Homans and Kohut alike is dropped, and the researcher adopts Ricoeur's (1970) 'hermeneutic of suspicion.' Empathy is for data collection only, not for theory formation. Self-reflexive understanding, however, is required in both research moments.

Having described the theoretical virtues of this ethnographic methodology, I must mention some of its problematic ethical dimensions. As with all ethnographic methods, reflexive ethnographies inevitably leave the researcher in an ethically ambiguous position. A peculiar danger with reflexive ethnography arises from the special 'betwixt and between' position of the reflexive researcher. The immersed, reflexive ethnographer, from the standpoint of field subjects, is simultaneously both 'one of us' and not. This leaves a great deal of room for unintentional exploitation by the ethnographer in pursuit of information. A practical example of this difficulty arises when field subjects forget over time that the ethnographer is an ethnographer and perceive him merely as another member of the group, despite ethnographer efforts to inform and remind subjects. I will revisit this theme later, when I will suggest that an empathic approach to reflexive ethnography may alleviate, but not fully resolve, the difficult ethical dilemmas of ethnographic research.

obstacles to data in the buddhist field

The preceding model of ethnographic empathy and reflexive 'dispersed authority' grounded my project to examine why so many non-Tibetan Americans from Christian and Jewish backgrounds are turning instead to Tibetan Buddhism as a religious option (Capper, 2002). To investigate my questions I undertook more than two years of fieldwork at a major Tibetan

Buddhist monastery, or *gompa*,⁵ in the United States, which I fictitiously call ‘Siddha Gompa.’ About 25 Americans and 10 Tibetans may reside at this center, which is fluid in population, at any one time. The abbot of Siddha Gompa has served as abbot for other important Tibetan *gompas* in Sikkim and India, and the resident *lama* is recognized as a *terton* (*gter ston*), or mystical discoverer of religious artifacts, as well as being a *tulku* (*sprul sku*), a uniquely Tibetan kind of avatar-cum-religious leader. An effort of Tibetans to preserve their religious heritage in the diaspora, Siddha Gompa might be as authentic and faithful to Tibetan models of residential Buddhist practice as any found in the United States.

Before I arrived at my field site I previously had undertaken Buddhist practice for personal reasons. While living in Japan I practiced *zazen* meditation at a Zen monastery once a week for about a year. I had visited Tibetan temples, shrines, and religious meetings while traveling in Nepal and India. After this, I had a year of experience with one Tibetan Buddhist group and several months of experience with another in Chicago. However, my Buddhist identity, in terms of formal Buddhist practice, was quite tenuous, and I ended up dropping out of both American Buddhist groups. I was, at best, an intellectual, rather than formally practicing, Buddhist. I had little direct experience of Buddhist guru devotion as found at Siddha Gompa and, at the time of my arrival at the *gompa*, my personal appreciation of the practice tended towards cynicism.

My research strategy combined the usual anthropological sources of data collection in the form of interviews, informal conversations, and tacit observations, guided throughout by the question, ‘Why do Americans practice Tibetan Buddhism?’ However, obstacles to data collection arose almost immediately. I frequently asked questions about the religious experiences, life histories, family relationships, guru relationships, and controversial lifestyle choices of my field subjects, or, as I term them, interpreters.⁶ In the beginning of fieldwork interpreters were reluc-

tant to answer any questions at all or answer them with any depth. The first few months of fieldwork remained dry of any substantial personal data from my interpreters while I was treated sometimes with great suspicion. I began to feel that my project was in jeopardy from the futility of data collection.

Obstacles to data collection arose from two sources. There was reticence on the part of my interpreters to participate meaningfully, and there also were obstacles, somewhat unknown to me at first, from my side as a researcher. Both of these obstacles were eventually overcome through my empathic methodology and thus are germane to this essay.

The reticence of my interpreters derived first from the unsurprising yet potent observation that people often are unresponsive to the kinds of questions that I asked, such as, ‘Have you had any mystical experiences?’ Interpreters were understandably shy about revealing information about such personal questions. In the beginning, those most open to answering my questions were so because they had little of value to share. Others responded by parroting doctrines gleaned from a variety of New Age sources. Quality responses were difficult to gain.

Another source of reticence from interpreters derived from an anti-intellectual atmosphere at my field site. Although some interpreters at Siddha Gompa possessed advanced university degrees and valued institutional education, many did not, having happily ended their educational careers before or right after college. Also, among virtually all interpreters there was a shared perception, openly and commonly expressed, that the research of academic Buddhologists was invalid and untrustworthy because academics do not practice Buddhism, they only write about it. For most interpreters at the *gompa*, Buddhism represents a set of practices designed to bring about personal transformation and ultimately liberation from *samsara*, the round of rebirths that is fraught with dissatisfaction. When practiced, Buddhism is quite powerful in effecting

such personal transformation and ultimate liberation, but it must be practiced to be effective in this way. Therefore, not to practice Buddhism is not to understand that Buddhism tells you that you must. To be a non-practicing scholar is to miss this most essential point of the Buddhist teachings. The non-practicing scholar can at best, from this point of view, produce work that is untrustworthy or irrelevant.

Another source of reticence from my interpreters involved the sociology of Siddha Gompa knowledge. Buddhist teachings and practices at the *gompa* follow the hierarchical model of the guru-disciple relationship to a high degree. At Siddha Gompa it is commonly taught and understood that one achieves liberation only in and through one's relationship with a Tibetan Buddhist guru, or *lama*. Unstinting devotion and service to the *lama* provides the ultimate soteriological ground. Naturally, in this model of spirituality, the authority of the *lama* is paramount. At the *gompa*, *lamas* are seen as possessing intellectual knowledge of teachings that others lack. Even more, *lamas* have pursued the practices and enjoyed the experiences necessary for enlightenment, enriching this knowledge. To maintain hierarchy, such knowledge is regarded as the sole possession of *lamas*. It sets *lamas* apart from ordinary practitioners. Unfortunately, my interpreters perceived me as a threat to the exclusive knowledge they ascribed to their beloved *lamas*. I was the only non-*lama* who aspired to write about Buddhism or teach about Buddhism, even if such activities of mine would occur for very different Western audiences, with very different agendas, and in very different settings, such as the secular undergraduate classroom. For my part, I took great pains to avoid playing the role of competitor to *lamas* and consistently downplayed what limited knowledge credentials I possessed. For my project I was there to be the student of *lamas* just like other practitioners and I knew that my knowledge of Buddhism, especially in terms of what was important to practitioners, was vastly inferior to that of Siddha

Gompa *lamas*. Yet some interpreters ascribed a competitive role to me anyway. Part of this role ascription both derived from and resulted in a lack of understanding of my research motives despite my efforts to be clear on this point. The worst example of this was a rumor which floated through the *gompa* for several weeks which claimed that I was there to write a hostile ‘exposé’ about Buddhist practice in the United States.

A second obstacle to data collection derived from my own side, from the social and psychological baggage, mostly unconscious, that I brought to my field site. Despite the fact that I had done some previous fieldwork, it was not the scale that I was then attempting, and I was nervous about my capabilities to perform such an investigation. As a reflexive ethnographic *bricoleur* I was unsure of my location(s) (Tweed, 2002b) in the *gompa* social realm and I feared making major mistakes. Therefore, my demeanor was much more stiff, distant, and reserved than it usually is outside of the research setting. And, used to being at least somewhat personally trusted outside of the research setting, I found myself responding to my interpreters’ distrust of me with conscious and unconscious distrust of my own. This, of course, inhibited Kohutian empathic ‘evenly hovering attention.’

Perhaps more problematic than these obstacles, I provided other obstacles to research deriving from a clash of metaphysical world views. The academic womb that gave me professional birth and socialized me is highly rationalist, in which motives and actions are explained following secular psychological and sociological theories. Despite previous experiences with American Buddhists and other alternative religious groups, I found it difficult to empathize with seemingly irrational stories involving the unrestrained omniscience and omnipotence of Tibetan *lamas* as well as with the pervasive employment of Western and Eastern astrology for understanding the behavior of others. As David J. Hufford puts it, ‘The truth claims of folk religion and the

basic thrust of scholarly hermeneutics of a religion constitute a primary, fundamental conflict of interest between the scholar and those studied' (Hufford, 1999: 300). Because of this conflict, I lacked the capacity to fully empathize with the more 'spiritualized' world views found at Siddha Gompa.

My own unconscious processes also spawned a clash of moral perspectives. Unknown to me at the time, I arrived at Siddha Gompa with an irrational, unconscious expectation of finding a sort of moral utopia, in which practitioners would unfailingly manifest Buddhist compassion and loving-kindness. The sometimes distant and impersonal ethos of the *gompa*, along with natural and understandable human frailty and imperfection, destroyed this unconscious expectation rather quickly. My disappointment, not fully recognized and thus unintegrated in my personality, was not lost on my interpreters. They noticed, even if only in an inchoate way, that I had failed to properly mobilize empathy in the research setting.

blending in for insight

To overcome these obstacles to data collection, it became clear to me that the only way to get real data from my interpreters was to become more of an insider. I had to follow George Devereux's (1967: 195) dictum that to get beneath superficial details, the fieldworker must be cathected by the group being studied. Likewise, empathy declines the more dissimilar the observer is from the observed (Kohut, 1990: 86; Crapanzano, 1980: 141). Empathy arises from 'identification by way of imitation' (Freud, 1955: 110). I needed to employ the 'sympathetic magic' of fieldwork, in which the ethnographer appropriates knowledge through mimesis of the researched (Coleman, 2002: 76). To understand Buddhists with scientific empathy and honesty, I needed to open myself to becoming more of a Buddhist.

For this venture I enjoyed direction from some reflexive ethnographies. Jeanne Favret-Saada (1980) lent bearing from her study of French witchcraft phenomena, which consisted of wars of words, of accusations and counteraccusations of bewitchment. While this discourse lay at the heart of her field environment, she found herself denied access due to her ethnographer status. Her interpreters offered her only trivial, misleading, untrue, or nonexistent data. After several months in a fallow field of data, she changed strategic course. In her words, she had to 'get caught' in the discourse. She adopted a more active, participatory role, in which she was willing both to accuse and to be vulnerable to accusations of witchcraft. This entrance into the essential phenomena of witchcraft she studied allowed deeper, more meaningful information to emerge from her interpreters, and her apparently stalled project suddenly came to life. An important aspect of this situation is the fact that her subjectivity, following enriched participation, allowed her access to data that otherwise would have remained hidden. Her own experience of witchcraft became her most important datum, *pace* Devereux (1967). As she put it, 'For anyone who wants to understand the meaning of this discourse, there is no other solution but to practice it oneself, to become one's own informant, to penetrate one's own amnesia, and to try and make explicit what one finds unstateable in oneself' (Favret Saada, 1980: 22). For Favret-Saada, ethnographic reflexivity runs psychologically deep. The more the ethnographer can access and uncover her own unconscious, the richer her data will be.

As another example, in her study of British witchcraft practitioners Tanya Luhrmann (1989) faced problems similar to mine in that her interpreters at first were not forthright with her. She responded by becoming a witch herself. She adopted as fully as possible ways of thinking, practicing, and living that were like those of her interpreters. Of interest is the fact that she did not just 'blend in' or assimilate with her field site socially only, but psychologically as well. In

this serious adoption of the identity of a witch Luhrmann found that her subjectivity opened up realms of information that otherwise might remain hidden. Only through her own psychological assimilation and experience could she understand what the ‘real’ life of witches entailed, so that her own subjectivity served as a locus of ethnographic data. She became her own informant. However, this psychological ‘blending in’ ended with the subject of metaphysical world views. Beliefs in magical actions and powers that were taken for granted by other witches could not be embraced by Luhrmann, as they conflicted with her rationalist academic world view. This inability to embrace her subjects’ world view was a barrier to full participation for Luhrmann, as it was the only aspect of her experience that defined her as an ethnographer in contradistinction to her non-ethnographer subjects (Luhrmann, 1989: 320-321).

As discussed, for my project it was obvious that I had to ‘get caught’ or ‘blend in’ myself. Discourse at Siddha Gompa had marginalized me precisely because I was not a Buddhist practitioner like everyone else. If academics are distrusted because they do not practice, only by taking my interpreters’ views with empathic seriousness and participating fully could I overcome my interpreters’ ascriptions of ethnographer identity. I had to gain, through empathy, the role of a trustworthy person with whom one could speak meaningfully. And, following Favret-Saada and Luhrmann, this participation would likely reveal a subjective realm both rich in data and fertile for further empathy with my interpreters. More intensive Buddhist participation likely would allow new worlds of membership experience (Preston, 1988: 10-11). But this ‘blending in’ risked violating the entrenched anthropological stigma against ‘going native.’⁷ I took this risk by recognizing the ‘part artist, part scientist’ nature of religious studies research (Doniger, 1998: 76). In Wendy Doniger’s terms, to be a good academic ‘sage,’ I had to release my inner spiritual ‘hunter’ that the sage attempts to repress, since ‘good sages’ always have ‘good hunters’ inside

them, anyway (O’Flaherty, 1988: 10-12).

Although I had previously dropped out of Buddhist groups in non-research settings, this time I had to self-critically look within to uncover my possible resistances to Buddhist practice. Had I been previously too narrow-minded, unappreciative, or self-reliant to get involved? Following Kohut, Favret-Saada, and Devereux, I needed to explore my own inner resistances and recognize my countertransferences so that I could become my own informant. In grappling with difficult personal psychological issues, I began to understand my Buddhist experiences in a new light. I came, in a personal way, to a new understanding, appreciation, and enjoyment of my own Buddhist life.

Following this, I pursued ‘blending in’ through several activities and some welcome good luck. First, I attended as many teachings and practices at the *gompa* as possible. At Siddha Gompa *pujas* (deity yoga meditational and worship services) were performed daily at 5 AM for the deity Green Tara, at 5 PM for the protector deity Mahakala, and at 7 PM for Chenrezig, the ‘Great Being’ of compassion. I attended these rituals regularly. Over many weekends, *lamas* gave teachings on Friday night, Saturday, and Sunday, and I attended these as well. It was common as a part of these weekend teachings to have ceremonies for *abhisheka*, or ritual initiation into various subcults of Tibetan Buddhism, on Saturday nights. These I attended, giving me the full spectrum of formal ritual experience that the *gompa* had to offer.

This led to my authentic participation in the post-ritual sharing of subjective experiences of ceremonies that was nearly a ritual in itself. On these occasions interpreters came to understand my personal inner life and motivations more clearly, alleviating tension and building friendships. I shared both my growing appreciation for and still-cynical distance from our common ritual experiences. In so doing I discovered I was accepted for being thoughtful, for at-

tempting to be and some times succeeding at being appreciative, and sometimes for harboring and voicing doubts that interpreters shared but were too timid to voice on their own. I never developed the devotion to a *lama* that is the central practice of the *gompa*, but I came to better understand those who did and to genuinely enjoy other practices such as the *Chenrezig puja*. Over time, my involvement in these practices afforded me more of an ‘insider’ status and dispelled the ‘hostile exposé’ rumors.

Along with this formal Buddhist participation, I was offered a further opportunity to ‘get caught.’ I had from the beginning offered my services for voluntary labor and had worked several *ad hoc* jobs in the kitchen and wood shop. Six weeks into my fieldwork, one of the American managers of Siddha Gompa offered me a more stable position as a volunteer fund raiser, in which my duties involved depositing donations into the bank and paying *gompa* bills. This unexpected stroke of good luck obviously increased my knowledge about and understanding of the *gompa* from an insider’s perspective. It also offered numerous opportunities to show that I respected and cared for my interpreters, their ways of life, and their *gompa*. Working relationships became friendships that offered information. And I had to work within the value confines of interpersonal relationships at the *gompa*, which include Buddhist compassion for others, patience, tolerance, and the inappropriateness of anger. In so doing I was able to observe the dynamics of these values in action in a way that a more distanced ethnographer never could, as well as replace my foolish, unconscious moral expectations with more realistic and empathic modes of moral perception and experience. In sharing my moral experience, and my moral failings and apologies, with my interpreters, I came to understand my interpreters more, allowed them to understand me more, and created, without even really trying, bonds between people.

As time went on, interpreters began to trust my research more. Even though I was careful

to inform or remind interpreters of my status as ethnographer, I came to be perceived by some more as a Buddhist who also was an academic, rather than primarily as an academic with a shallow, intellect-only interest in Buddhism. For many of my interpreters, I had been ‘caught.’ My data became richer. People began telling me about mystical experiences, Buddhist lives, and everyday activities in much greater detail. Questions that previously had been difficult to approach answered themselves without being asked. Had I not taken an initial empathic stance toward fieldwork, much of this likely would have eluded me.

The empathic approach also allowed my own obstacles to data collection to diminish. By becoming my ‘own informant’ and empathizing with interpreters, the difficulty I shared with Luhrmann regarding divergent metaphysical world views became instead a realm of ethnographic meaning. My greater empathic participation allowed me to see the less rationalized, more ‘spiritual’ world views of Siddha Gompa in action in its members, which revealed important perspectives about *gompa* culture. I saw how the supposed omniscience of the spiritual teacher, who would know even from thousands of miles away that one is shirking chores, was functionally employed by interpreters to overcome laziness or shoddiness toward, and even develop pride in, work projects. Likewise, this invisible omniscience of the *lama* provided a sense of security, an emotional safety net, for interpreters who faced personal difficulties, not unlike an emotionally close but geographically distant friend in the secular world. Astrology provided interpreters with helpful roadmaps for navigating the confusions and obstacles of social living. My empathic involvement allowed me to comprehend that, contrary to much psychological theory, these behaviors and explanations of reality were often positive and healthy adaptations for interpreters, as they offered practitioners symbols that were psychologically integrative and ‘progressive’ in Obeyesekere’s (1990) sense.⁸

In recognition of these boons for interpreters, the problem of divergent metaphysical world views appeared in a different light and actually lent multivocality to my final project. Ethnographic data arose by my rejecting the ‘methodological atheism’ (Berger, 1967) so commonly found in religious studies, as instead I adopted what might be called ‘applied methodological agnosticism.’ I walked a metaphysical tightrope, later well portrayed by Fiona Bowie (2000: 10) in terms of the ethnographer’s suspension of disbelief in ethnographic investigation, ‘without feeling it necessary to enter into discussions of truth and falsity.’ Based on my empathic responses to interpreters’ experiences, ethnographically I held both sets of world view, the *gompa* and the academic, in scientifically creative multivocal tension, rather than adhere to only one world view and thus privilege it. As with Luhrmann, in the end I could not fully embrace the various ‘spiritualized’ world views found at Siddha Gompa and remain a scientific ethnographer.⁹ But I could, following Homans, empathize with *gompa* world views for data collection, constantly ‘moving across’ (Tweed, 2002b) boundaries of *gompa* and academic metaphysical perspectives. Then I grounded my experience-distant theoretical analyses within academic world views.

Also, in empathically trying to make explicit what I found unstateable in myself, as Favret-Saada suggested, I unraveled obstacles to data collection deriving from my foolish unconscious expectations of a moral utopia. In so doing I discovered important data that otherwise I might have missed. In making sense of the moral disillusion I experienced, I came to see that many interpreters possessed the same expectations. Practitioners discussed from their own side, and even acted out, what I felt. Yet working at the *gompa* also allowed me to appreciate how very difficult it is to fully embody its values. This led me to appreciate other interpreters who seemed not to succumb to the expectation of moral perfection and its concomitant disillusion as

much. They expressed an explicit recognition that every person at the *gompa* was struggling along the Buddhist path and was prone to mistakes as part of the struggle. The two sides of this issue, the expectation that others be morally perfected and the recognition that they are not, remains a tacit ideological tension in *gompa* culture, manifesting in different ways in different people. Had I not originally been so irrational and naïve in my unconscious expectations, and had I not responded by empathically ‘blending in,’ this tension may have escaped my notice, or I may have perceived some interpreters as immoral or pathological in an apparently theoretically grounded and rational, but ultimately mistaken, way.

ethical dilemmas

Although the empathic method of reflexive ethnography augmented and enriched my data, difficult questions remained about its ethical status. I found that the reflexive ethnographic method left me in an ethically ambiguous position that prompted difficult questions. Problems arose from the reality that if interpreters remain constantly aware of the ethnographer’s role, one has done a poor job of ‘blending in.’ Successful community assimilation by the ethnographer, in my experience, inevitably involves interpreters’ losing sight of the ethnographer’s role.

For example, during my time at the *gompa* it was universally known that I was teaching part-time at a local college, so that my academic ambitions were quite open. I often parted from friends at the *gompa* by explaining that I had to write up field notes or do project-related research reading. I freely conversed with *gompa* residents about the events and problems of my academic life. I offered my writings to interpreters for their reading and comments. Frequently asking people for formal interviews helped to mark me off as a researcher as well. I took pains to inform new interpreters about my ethnographic project. When treated blindly as an insider by the

forgetful, I reminded interpreters of my ethnographer status. Nonetheless, however, at least some people forgot about my status as an ethnographer at least some of the time, despite my efforts.

In my *bricoleur* efforts to avoid ethnographically exploiting interpreters, I unintentionally increased this amnesia about my researcher status. Because exploitation derives from a lack of mutuality, I thought, I must be sure to give back to the community. I volunteered a great deal of free labor to the *gompa* and turned down offers to pay for my services from *gompa* managers, who ironically attempted not to exploit me. More personally, as I indicated above, I shared my own biographical details, experiences of rituals, and moral concerns with my interpreters. I volunteered the same kinds of information about myself that I sought from them. As a side effect, these actions fueled one-sided interpreter perceptions that I was a colleague, not an ethnographer. Interpreters, unsurprisingly, did not maintain the simultaneous awareness of my dual roles of ethnographer and ‘insider’ like I did, so that the more I gave back to the community because I was an ethnographer, the more interpreters forgot that I was an ethnographer.

This created difficult dilemmas regarding unintentional exploitation by the ethnographer. An interpreter who lacks awareness of the presence of the ethnographer, despite ethnographer efforts to inform and remind, seems not as voluntary as a research subject as someone who is aware. Is it fair to collect information from forgetful and hence vulnerable interpreters? Is it permissible to use information gained under these circumstances? If so, how much information, and for what purposes? Conversely, is not this ‘insider’ information often the most honest? Is it fair to the academic pursuit of understanding to exclude information gained from honest familiarity?¹⁰ In the end, can one “blend in” and not subtly exploit the researched or betray academic standards of honesty and reliability?

In my own response to these questions, I gropingly used the two-step empathic method. I empathically remained open to interpreters' points of view when deciding what counted for data, then I freely analyzed in a scholarly manner what data had passed the empathic gate keeping. Some data I rejected for use because they obviously violated trust relationships. I considered other data, gleaned in the course of conversations in which I empathically considered an interpreter to be forgetful and vulnerable, to be more private and hence more worthy of my protection. When possible I asked interpreters if I could include these data. With one exception (see below), when in doubt about the ethical status of these data, I excluded them. In my finished study, such data appeared mostly as a background to generalizations and interpreters were directly quoted from such material only when I had clear permission to do so. I provided added protection by fictionalizing all names, including that of the Buddhist center, and remained quite vague about the center's location in the United States. Unfortunately this imperfect response to the salient ethical dilemmas was sound only to the extent that I could balance my empathy for and loyalty to my interpreters with my loyalty to reliable scholarship.

The reception of my interpreters to my finished work remains relevant to assessing the success of this approach. To my good fortune, my study was considered quite acceptable by many interpreters who read it. One manager even requested a copy to be kept at the *gompa*. Several interpreters appeared to take pride in what they perceived as academic and cultural acceptability, gained from my work, for their non-mainstream Buddhist practice.¹¹ By employing the power of presentation with academic honesty and rigor to the fortuitous favor of these interpreters, several of them actually seemed happy that I possessed such power. For the most part, my approach resulted in mutual satisfaction for both interpreters and the ethnographer.

However, one interpreter, Lucy, was clearly offended by the way in which I presented her

life at the *gompa*. In creating this presentation I betrayed no confidence and primarily relied on her formal, voluntary interview data, and her expressed offense appeared to derive from my portraying her in an unflattering light, not from feelings of exploitation. Yet my presentation was informed by experiences during which she may or may not have been aware of my role as ethnographer. Should I have excluded her case study from the start, excluded data gained when she was presumably “forgetful,” or altered my presentation to please her? Since the experiences with Lucy that I cited were corroborated by other interpreters, and thus were public knowledge at the *gompa*, was Lucy’s awareness of my status as an ethnographer relevant in a practical sense? These issues troubled me. But without her case study, complete with unflattering dimensions, my findings would have been misleading and unreliable, as her case study represented the strongest counterexample to my overall hypothesis. Also, some interpreters likely would find my presentation unrealistic and hence impoverished had I been silent about data like Lucy’s. Therefore, for better or for worse, Lucy’s case study appeared unchanged in my final report.

Ethical questions therefore remain. Must interpreters be pleased with the final report to avoid exploitation of them? If so, must they all be pleased, or only some of them, and in what ways? What about situations in which academic honesty and rigor do not lead to fortuitously positive presentations of interpreters? If interpreters are able, through their approval of the work, to determine the contents of the final scholarly product, does this not limit scholarly reliability and the unfettered, critical search for understanding? Is pursuing reliability an ethical duty, as well as a scientific duty, for a scholar? Left with all of these questions, I must agree with Brown (2002: 133) that ethnography intrinsically is an ethically messy business.

conclusion

As I have discussed, the continued expansion of the use of ethnography in the study of religion, combined with the effects of postmodern epistemologies on ethnographic theory, has led to substantial methodological confusion. In response, individual ethnographers behave as *bricoleurs*, availing themselves of the tools they find at hand to create case-by-case ethnographic strategies. These efforts commonly appeal to the need for the ethnographer to empathize with field subjects, although the nature of this empathy remains poorly explored. This essay has pursued an amelioration of this situation by offering a model of ethnographic empathy, fully coherent with the theory of reflexive ethnography, in terms of the methodological observations of Weber, Homans, and especially the psychologist Heinz Kohut. Using Kohut's model of empathy in terms of a reflexive 'evenly hovering attention' for data collection, I have explored possible gains of this model in the field in terms of overcoming obstacles to data collection posed by Buddhist field subjects. As well, I have highlighted gains derived from this model in terms of overcoming obstacles to data collection resulting from researcher difficulties with world view conflicts as well as with the psychological baggage that any researcher unavoidably brings to the research situation. Ethical difficulties with this method have also been explored, albeit with more questions than answers.

Ethnographers, particularly of the reflexive ilk, will certainly continue to be *bricoleurs* to some degree. Perhaps this presentation will help to reduce the confusion, even terror, that ethnographers often feel as they 'put something of themselves' into their field research projects.

NOTES

¹ Marcus and Fischer (1986: x) discuss this in terms of a current ‘exhaustion with a paradigmatic style of discourse altogether’ due to ‘the enduring anthropological problem of what constitutes an adequate account of another culture in the face of contrasting interpretations’ (ibid.: 3). Bromley and Carter (2001: 5-6) tell us that, ‘There is no longer assured congruity of or conformity to conventions in the way field research and narrative construction are conducted...[individuals are] working from divergent perspectives, making ad hoc methodological decisions, and facing difficulty in determining what and how to report.’

² A reader of a previous draft of this essay objected that what is termed ‘empathy’ here is not empathy at all, because for this reader empathy is anti-utilitarian, without an ulterior motive such as data collection. This reader mistakes compassion or sympathy for empathy. Compassion and sympathy are intrinsically concerned with the welfare of another person. And compassion and sympathy may, and perhaps even must, lack a utilitarian motive. On the other hand, the definition of empathy of ‘cognition through the narcissistic investment of the other’ (Kohut, 1978b: 701) gives no account as to why this cognition may or may not occur. Empathy for Kohut intrinsically concerns discerning contours of the experience of another person, their welfare aside, and as such may be quite utilitarian. Kohut’s example of Nazi radio propaganda reveals precisely this difference. As well, should empathy always be without utilitarian motives, it could never be a tool in self psychological psychotherapy, in which discerning the contours of the experience of another is incumbent upon a therapist who bears the quite utilitarian motive of healing the client.

³ The countertransference is a psychoanalytic concept denoting the unconscious attitude of the therapist toward the client.

⁴ This understanding, in terms of ethnography, is perhaps extended by Robert J. Thornton, who trenchantly critiques the basis of many classical ethnographies for engaging in the ‘fiction’ of ‘ethnographic holism.’ He says (1992: 18-19), ‘Social wholes cannot be directly experienced by a single human observer. The vision of the scope and the scale of social life that extends beyond what we can experience must be imagined...while we can experience social relationships in which we are involved, and witness a few of those in which others are involved, it is manifestly impossible to witness or experience society.’ From this Thornton declares, ‘Like the imaginary “frictionless space” in Newtonian mechanics, these ulterior images of wholes are not directly accessible to either the author’s or his subject’s experience. They can only exist in the imaginations of the author, his informants, and his readers. This is the “essential fiction” of the ethnographic text’ (ibid.: 19).

⁵ *Gompa* (Wylie Tibetan *dgon pa*) is commonly glossed as ‘monastery.’ This translation is inadequate, as a *gompa* may contain both ordained and lay of both genders, even in positions of power, unlike Christian monasteries. I retain the Tibetan word in recognition of this social and religious institutional arrangement that is unique to Tibet.

⁶ I term my informative fieldwork subjects ‘interpreters,’ rather than the more traditional ‘informants,’ following a style established by Herdt and Stoller (1990). My terminology reflects my reflexive ethnographic methodology. In a reflexive environment, the idea of key informants who offer objective, expert material from inside a cultural complex is nonsensical. Rather, research subjects offer their points of view from their subjective social and psychological location within culture. It is an ethnographer’s primary task to account for their subjectivity along with the unavoidable subjectivity of the ethnographer. Therefore, ‘interpreter’ appears a much more satisfying term, as it more fully represents the role of subjectivity on several levels for data gathering and analysis.

⁷ Trinh Minh-ha ably describes this stigma: ‘The *proper* anthropologist should be prevented from “going over the hill,” should be trained for detachment in the field if he wishes to remain on the winning side..[To become] a member of that cult has been seen as a loss, a loss of objectivity, for the man changed sides and—why not say it—betrayed his own kind.’ (italics in original; quoted in Neitz, 2002: 42).

⁸ On this see Frank Salamone’s patient Robert, whose ‘New Age ideas and activities did not represent a pathological fixation in primary process thinking (“magic”), but, rather, served a healthy object/selfobject function essential to his continued growth’ (Salamone, 1995: 336).

⁹ For example, I personally continued to understand practitioners’ actions as arising from their own psychological and social dynamics, rather than from the mystical activities of lamas or astrological determinism. I could, however,

embrace the world view of karmic causation in a limited way.

¹⁰ Spickard (2002: 249) claims that 'ethnography's chief intention' is 'presenting an honest portrayal of others' lives.' This appears to imply an ethical duty to scholars, not just to interpreters, for the ethnographer. Misleading suppression of data for the sake of interpreters may violate honesty and trust. It may even harm interpreters in a larger perspective. For example, Donald Lopez (1998) describes how Tibetan people have been harmed not just by unrealistically negative, but also by unrealistically positive, portrayals of them in Western writings. For Lopez, unrealistically positive portrayals create, in the long term, unfair expectations of and negative outcomes for Tibetans who may or may not have benefited from these portrayals in the short term.

¹¹ Following Coleman (2002), this represents an example of interpreters appropriating the academy as it appropriates them.

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