Introduction

Emotions in the Field*

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THE AIM OF THIS BOOK is to help retrieve emotion from the methodological margins of fieldwork. Our task is to investigate how certain emotions evoked during fieldwork can be used to inform how we understand the situations, people, communities, and interactions comprising the lifeworlds we enter. By emphasising the relevance of emotion in anthropological research, we take up a theme that the "reflexive turn" of 1980s and early 1990s anthropology considerably overlooked. While this school explored how the ethnographer's position in the field influences the data he or she acquires (and the varying ways our identity, gender, ethnicity, and personal history affect how we understand, interact with, and write about our field sites), it left comparatively under-investigated the researcher's *states of being* during fieldwork and how these states may either enable or inhibit the understanding that fieldwork aims to generate. This relative neglect has naturally left many pages unwritten in our methodological canon. And so it is the aim of this volume to give voice to the growing chorus of researchers (within these pages and beyond) who are working to redress the imbalance. Our objective is to show how certain emotions, reactions, and experiences that are consistently evoked in fieldworkers, when treated with the same intellectual vigour as our empirical work demands, can more assist than impede our understanding of the lifeworlds in which we set ourselves down. Counting these subjective phenomena as data to be translated through careful reflection

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into anthropological insight is the central and unifying aim of this volume, the contributors to which, by building upon existing work on intersubjective and experiential fieldwork, explore new ways to achieve these translations.

Two Beliefs We Contest

To situate this volume amidst existing research, let me first survey historically how researchers' emotions have been understood in social science methodology. To preface this task, I will start by identifying two beliefs that have significantly influenced the history of modern field methods—beliefs that we contest insofar as they reject the idea that emotion can have epistemological worth. The first and most recent stems from a school of postmodernism advanced in the 1970–80s. Its intention was to demonstrate the inherently imperialistic and oppressive nature of fieldwork. It held that by submitting "others" to the anthropological gaze, ethnographers often replicated in mitigated form the exploitative dynamics of the colonial era. This critique presupposed that researchers were riddled with many barely perceptible self-interests and/or assumptions which distorted and biased their observations, leading them to construct more than to reveal their object in ways that rendered their object oppressed. As objectivity was therefore seen as an illusion, any claim researchers made to assured knowing was naïve or, at worst, politically self-serving. While many important insights emerged from this critique, and at times many sobering and useful lessons, one central argument that it implied is inevitably contested by the chapters of this volume: that subjectivity undermines the process of knowledge construction and never enables it—this is to say, *subjectivity has only a corrosive effect upon the process of research.*

The second idea that we dispute stems from what we call "traditional empiricism." This tradition drew firm lines between the researching subject and the researched object, and also defined across the social sciences what attributes of the researcher could usefully contribute to the activity of knowledge construction—namely, rationality and the capacity for detachment.[1] This approach meant that anything believed to undermine those attributes, such as encroaching feelings or affects, had to be methodologically removed or subdued. The marginalisation of emotion was consistent with the belief that subjectivity in both quantitative and qualitative research is something to be controlled and restrained, as it invariably introduces irregularities that cloud and bias research. For traditional empiricism the idea that emotion could actually be used to generate understanding was therefore simply a non-starter. Indeed, much socio-

cultural research influenced by this tradition, and keen to avoid its unsparing censure, not only under-investigated our emotions in the field but at times actively under-reported or concealed them—the irony being that the neglect of these data offended empiricism more deeply. True empiricism does not ignore the facts but is obliged to accept them, embrace them, and advance theoretical formulations upon them.

Both "postmodern" and "traditional empirical" beliefs, no matter how divergent the traditions from which they spring, share common ground on one critical point—they agree that the personal equation, wherever it may arise, is the *saboteur par excellence* of all generalising aspirations. While postmodernism by viewing subjectivity as belligerently omnipresent developed this into a radical repudiation of all universalising aims, traditional empiricism, taking the opposite route, developed ever more stringent methodological controls to create spaces in which so-called pure investigation could proceed, free from subjective distortion. The response of each made unavoidable for both one restrictive but all-pervasive corollary: subjectivity itself offers no royal road to insight, discernment, or any species of knowing.

It is clear, then, that any project placing emotion onto an epistemologically relevant plane implies a critique of both streams of thought: firstly, by showing how the concealed and neglected aspects of the researcher's emotional experience can actually present opportunities for understanding; and secondly, by developing a new and re-humanised methodological framework which exposes the weaknesses of the old. Why we in this volume and many colleagues outside it have arrived at such a position demands some deeper historical elaboration. This I shall now advance with respect to the social sciences more broadly and to anthropology in particular. After offering this historical account, I shall then describe the exact contribution that each chapter makes before finally outlining the precise methodological position that this volume advocates: one that we call, after William James, *radical empiricism*—namely, a position that refuses the epistemological cut between subject and object, that endows transitive and intransitive experiences with equal status, and that investigates phenomena which the inductive methods of traditional empiricism were never designed to treat.

A History of Emotions in the Field

The basic rule of method in the early natural sciences was that scientists should remain detached from their object of enquiry, and through systematic observation of available data seek hidden uniformities which could be translated into

quantitative terms. The physicist, chemist, and biologist all followed a similar procedure, each observing the facts of his respective domain with varying degrees of control over the context of investigation. Methods were developed to remove distortions caused by either the research environment or the researcher (Bruyn 1966:27). Such methods, especially with respect to the researcher, were considered to restrain those incursions of subjectivity whose unbridled expression was thought to otherwise corrupt research.

When in the early twentieth century this particular approach was applied to the study of social and human life, certain problems arose in method and theory. For one, the dichotomies upon which traditional empiricism rested (i.e., observer/observed, subjectivity/objectivity, subject/object), if supporting certain quantitative methods in early sociology, anthropology, and experimental psychology, seemed only to impede research into those areas of life that resisted being quantified. It was therefore argued that since so many human phenomena could not be explored quantitatively, if we restricted our investigations to only those facts which could be measured and counted, we would be forced to omit so much from our studies of social and human life that our sciences would become somewhat sterile (Storr 1960).

As the limits of quantification and objectification were more widely acknowledged in social science research, and as scepticism spread about whether detachment could reach what is most essentially human in society, these particular methods were less indiscriminately applied in other social science domains. This development brought the advance of alternative methods that had their foundations in the phenomenological and interpretivist thinking of the late nineteenth and early twentieth centuries. Philosophers such as Gottfried Herder, Martin Heidegger, and Wilhelm Dilthey invoked the ideas of *Einfühlung* (feeling into the world), *Gestimmtheit* (attuning to the world), and *tonalité* (adjusting to the pitch of the world), respectively, giving legitimacy to the participatory methods of which Frederic LePlay's *Les Ouvriers Européens* (1855) provides an early example. These thinkers urged that participation and detachment were methodological postures that could each reach distinct species of fact, and that therefore *both* belonged in social research. The same view was also implied in Max Weber's insistence that the observer and the observed were after all constituted of the same human essence, an idea grounding the concept of *Verstehen*, or knowing through empathic attunement.

In these ideas many early anthropologists found encouragement. For although fieldwork had been undertaken in anthropology since the late 1890s

(Franz Boas led his North Pacific expedition in 1897, and A. C. Haddon, W. H. R. Rivers, and C. G. Seligman led the Cambridge expedition to the Torres Straits in 1898), there was still a reluctance to accept participant observation as a distinct method until the early 1920s. It was Bronislaw Malinowski at that time who proclaimed a philosophy of fieldwork in his *Argonauts of the Western Pacific*. He distinguished between "native statements" and the "inferences" researchers drew from insights gathered through participation. The fieldworker would document data with "camera, note book and pencil" after "joining in himself in what is going on" (Malinowski 1960 [1922]:21). Being "in touch" with the natives, Malinowski was certain, clearly marked the "preliminary condition of being able to carry on successful fieldwork" (1960:8).

If at this point participation gained legitimacy in anthropology, it was less accepted that participation could evoke in fieldworkers powerful subjective reactions and emotions which implicated the method itself. Methods were still seen to have more to do with minds than with emotions or feelings or with what Plato called *thymos* (the heart). And to the extent that this belief was accepted, reflection was inhibited upon whether the personal consequences of participation could be of any scientific value. Consider, for example, the advice received by Edward Evans-Pritchard when studying at the London School of Economics in the 1920s. Seeking guidance from experienced fieldworkers about what to expect, both emotionally and practically, during his fieldwork in Central Africa, he recounts humorously the advice he received:

> I first sought advice from Westermarck. All I got from him was "don't converse with an informant for more than twenty minutes because if you are not bored by that time he will be." Very good advice, even if somewhat inadequate. I sought instruction from Haddon, a man fore-most in field-research. He told me that it was really all quite simple; one should always behave as a gentleman. Also very good advice. My teacher Seligman told me to take "ten grains of quinine every night and to keep off women." The famous Egyptologist, Sir Flinders Petrie just told me not to bother about drinking dirty water as one soon became immune to it. Finally I asked Malinowski and was told just to remember not to be a bloody fool. (Evans-Pritchard 1973:1)

While Evans-Pritchard no doubt enjoyed raising smiles with this quote, there was also a more serious point to be made. At the time of his apprenticeship little was being said about the experiential consequences of participatory research. If participation was accepted, its personal effects for the researcher

(and ultimately the work) were not. This particular predilection for emotion to be borne but never broached remained widespread throughout the first half of the twentieth century, for during this period anthropology was fully aware that the "personal equation" tested its scientific place in the academy. As Dumont has told us, the founders of modern anthropology such as Franz Boas, Alfred Kroeber, Alfred R. Radcliffe-Brown, and Evans-Pritchard were highly conscientious about how they represented themselves as researchers—for "it was the status of anthropology as science which was at stake with them" (Dumont 1978:7). Anthropology was not alone with these concerns, as the misgivings of psychoanalysis and analytical history at that time indicate. Psychoanalysis, for instance, revealed its anxiety by regarding as inadmissible the powerful subjective reactions evoked in analysts by the analytical relationship. Thus the analyst's "countertransference" (i.e., his or her emotional reaction to the patient) was until the 1950s largely perceived as a nuisance or as something to be eliminated. Freud would not acknowledge a subjective influence that he felt would render his aspiring science unscientific in the eyes of his peers. Analytical history, too, had its earnest denials—grand historical works by Arnold Toynbee, Oswald Spengler, and earlier by Karl Marx all relied on the belief that exact historical method sufficiently removed the personal equation, and thus if one proceeded correctly, further reflexivity was not required. This belief was compounded by the myth endemic to the socio/human sciences of the day: that conceding subjectivity was conceding status, and possibly privilege and position.

The late publication of Malinowski's fieldwork diary symbolised this disquiet within anthropology, for although it was written in the 1920s, it did not finally emerge until the safer ground of the 1960s. The diary contained all the emotions and experiences which Malinowski excluded from his formal methodological writings. Indeed, it was not until the 1950s that the experiences and emotions that participant observation evoked were reported at all. And yet even when such reports did emerge, as with Malinowski's diary, they were still safely shorn from mainstream anthropology and relegated to personalised fieldwork accounts. For example, in 1954 Laura Bohannan published her novel/account of fieldwork *Return to Laughter*, behind the pseudonym Elenore Smith Bowen. Presumably this nom de plume accorded security enough to flout a taboo which many honoured. Others were emboldened and followed suit. Gerald Berreman's experiences in India found articulation in *Behind Many Masks* (1962); K. E. Read documented his vexations in *The High Valley* (1965);

Hortense Powdermaker's experiences in the southwest Pacific and Madagascar found outlet in her *Stranger and Friend* (1966); and Claude Lévi-Strauss' *Tristes Tropiques* (1963) disclosed his troubled ruminations in Amazonia. Although these confessional accounts brought awareness of fieldwork's emotional underside, the experiences they documented were largely lyrical reflections that were detached from any systematic enquiry into their implications for method or theory-making. In short, these works, as Paul Rabinow accurately noted, "all . . . [clung] to the key assumption that the field experience itself is basically separable from the mainstream of theory in anthropology" (Rabinow 1977:5)—a thought earlier stated by Joseph Casagrande in *In the Company of Men* (1960)[2] and later echoed by Morris Freilich in *Marginal Natives* (1970).[3]

During the 1970s attitudes changed somewhat, as the experiential became slightly more formulaic. Solon Kimball and James Watson's *Crossing Cultural Boundaries* (1972) and Peggy Golde's *Women in the Field* (1970) opened the decade with insightful compendiums on the human face of fieldwork. Golde's edited work touched upon certain vagaries of culture shock, guilt, and the need for identity protection, while Kimball and Watson's volume comprised papers telling "off-the-record" stories of anthropologists at work in the field and beyond.[4] Also at this time a few interesting articles on the psychology of the fieldwork experience were published, though their impact was minimal. Scholars such as Morris Freilich (1970), Barbara Anderson (1971), Dennison Nash and Ronald Wintrob (1972), Deirdre Mentiel (1973), and Carole Hill (1974) dealt closely with the researcher's identity in the field—its shifting position explaining for them certain personal field crises that anthropologists may undergo.

In many respects the early 1970s marked a kind of unfulfilled high point of psychological reflection on fieldwork—the apogee of which was George Devereux's (1967) penetrating discussion of the relationship between anxiety and method usage. As Michael Jackson discusses in chapter 1 of this volume, Devereux stood alone in broaching the interplay of method and emotion: methods were not tools for gathering knowledge, he would flatly assert, but rather psychological devices used to confirm our biased perceptions and the stability of our outlook. Certain applications of method could act as a "defence mechanism" bolstering steadiness in disorienting conditions and ordering phenomena down preconceived conceptual channels. As method usage could thus subordinate unfamiliar cultures to the familiar epistemic visions in which these methods were rooted, we would often bring to the cultural facts the theories we claimed to derive from them.

When reading Devereux's work we are forced to consider how it undermined a core assumption precious to the traditional empiricist—namely, that if methods affected the researcher's subjectivity, they did so only in a very specific and controlled way: by creating in the researcher a detached and uninvolved relationship towards the object studied. The traditionalists believed that their methods rather tempered than enervated sentiment, rather quelled than aroused subjective response. This idea that methods could subjugate or even efface personality was one most easily received in the natural sciences. For here the personal reactions that methods provoked rarely appeared exaggerated or conspicuous, especially because the long tenure of scientific training made experimental activities so routine to scientists that performing them rarely upset their states of mind. Moreover, if one's research activity provoked any marked emotion at all, there were always other sciences that could explain it away. Laboratory workers' depression could be located in factors unlinked to their scientific activity, just as the biologists' *maladie du siècle* could be traced to anything but the psychological posture that their training and practice compelled them to adopt.

Thus, while the traditional empiricists asserted that methods essentially restrained sentiment, they were less ready to admit that these very same methods could *evoke emotions of a different order*. For had they recognised that the application of certain methods could generate new emotions, then the view that methods effaced feelings and personality would at once have become untenable. Thus traditional empiricists were careful to assert that it was only the link between method and mind that mattered. For them, methods created new states of mind (clear, rational, unencumbered by affect) and never new states of emotion. This was echoed in idioms depicting method usage as an essentially "technical" and "intellectual" affair: users "took up," "applied," or "discarded" these "tools" as one would solid items from shelves. And if these detachable apparatuses implicated persons at all, they did so only cognitively: to fail methodologically was primarily an *intellectual* failing—one traceable to mishandled procedure, misunderstood or misapplied design. The principle *cognitio fit per assimilationem cogniti et cognoscentis* (knowledge comes about through the assimilation of the thing known and the knower) was left everywhere unconsidered.

The irony here is that the traditional belief that method usage implicates only the intellect draws upon the old "faculty psychology" idea (i.e., that individuals can be partitioned into discrete components of "intellect," "emotion,"

"memory," etc.), rejected by many of these same traditionalists.[5] Many traditional empiricists did not reflect that to link method to a discrete "intellect" is no less problematic than to relate it to pure "emotion." "Intellect" and "emotion" are not analytical distinctions reflecting actualities of the "soul," but are rational categories imposed upon the total context of experience. Thus, when these categories are reified in the domain of methodology they come to support a kind of intellectualist myth: that methods function independently of the *total* personalities wielding them. This myth ignores, as William James long ago stated, that passion, taste, emotion, and practice cooperate in science as much as in any other practical affair (James 1995:40). Thus exercising method can be nothing other than a total psychological happening, for not only do we adapt personally to what methods dictate, but particular methods are most fully realised in those personalities best able to apply them. In this sense the popular yarn that the "obsessional" is always a more effective laboratory scientist than the "narcissist" (who would rather parade the results) strums more than only a humorous chord. It underlines an insight emphasising the importance of personal suitability and/or the process of its construction through professional training or socialisation.

Late 1970s/Early 1980s

Admitting the link between emotion and method gained a little more credence in the late 1970s, even though these admissions somewhat veered away from the psychological links being made in the early 1970s. By the late 1970s a number of discursive works appeared, such as Paul Rabinow's *Reflections of Fieldwork in Morocco* (1977), Jeanne Favret-Saada's *Les mots, la mot, les sorts* (1977), Jean-Paul Dumont's *The Headman and I* (1978), and Vincent Crapanzano's *Tuhami: Portrait of a Moroccan* (1980). While still reflexive, and in some measure psychological, these works explored fieldwork as an "intersubjective practice"; in other words, here ethnographic knowledge was seen to surface out of interaction and dialogue between subjects. Reflecting upon the emotional effects of intersubjectivity, however, was subsidiary to revealing and philosophising upon the contours of a dialogic approach. Describing and understanding the role of emotion was thus secondary to challenging a model of objectivity that denied the value of dialogue and intersubjectivity.

The impact of these works was doubtless served by the critical politicisation of objectivity that was burgeoning in the same period—a critique grounded in the existentialism of Nikolai Berdyaev and Jean-Paul Sartre, and

developed in the Frankfurt School by Jürgen Habermas and Theodor Adorno. These thinkers rendered objectivity doubled-edged. While to the researcher objectivity brought status and results, it could also oppress its subjects, supporting a growing dehumanisation and "rationalisation of man." It achieved this by subordinating divergent knowledges, technologies, and perceptions to its all-consuming, totalising vision: the mythopoetic, non-rational and inspirational elements of individual and social life were being increasingly demystified by rational inspection. The implications for anthropology of such scientific colonisation were traced by Dell Hymes and Eric Wolff and others in the late 1960s and early 1970s. In their view, anthropology must cease objectifying its subjects, become political, and show its subjects how they could overcome the conditions of their own oppression. Anthropologists could do this by teaching their subjects to objectify the causes of their oppressed state for the purpose of understanding and removing them. This petition initiated much of the Marxist anthropology of the 1970s as well as the reflexive turn of the 1980s, where objectivity, as the *Writing Culture* school (1984) claimed, was at heart a textual construct.

Revealing the duplicities of objectivity, if not wholly legitimating research into subjectivity, did generate more interest in it in the 1980s. For example, the decade was flanked by two works showing how emotion in the field is not mere gratuitous interference but could constitute an entrée into knowing. Rentano Rosaldo (1980) argued that emotion could be used as a prism through which the perplexities of difference could be discerned. He reflected upon how his wife's sudden death in the field sensitised him to the source of headhunting among the Illongot. He understood their "grief that could kill" only after suffering the deep woe of his own personal loss. Unlike detachment, where one learns culture from afar, Rosaldo believed that through exploring this unsought-for and tragic affinity, he had winched up culture to apprehend human unity underneath, just as a tapestry's interconnections are revealed only if the embroidery is turned and viewed from its underside. In a similar vein, and at the end of the decade, Tanya Luhrmann's (1989) work on contemporary witchcraft illustrated how the interestedness of the anthropologist often meant learning from the inside (*inter-esse* means, after all, "to be in") and afforded experiential recognition of a binding sentiment integral to the life of the community. In both accounts emotion was not believed to be antithetical to thought or reason, but was seen as a source of insight that could later be disengaged and communicated through anthropological reflection.[6]

Late 1980s/Late 1990s

The assertion that some of the most profound and intimate modes of apprehension could be generated through the emotional domain was complemented by a stream of work emerging from the 1980s. Authors such as Cesara (1982), Stoller (1987), Wengle (1988), Jackson (1989), Heald (1989), Obeysekere (1990), Wikan (1992), and later Hastrup (1995) and Crapanzano (1998) stressed the collaborative nature of fieldwork, a mutuality that changed both parties. John Wengle developed the psychological ideas of the 1970s showing that understanding was forged via processes of primary and secondary identification. Kirsten Hastrup, asserting that culture is learned through a process of "gradual familiarisation in practice" and that this familiarisation has its subjective concomitants, expressly linked praxis and affectivity and thus implicated subjectivity. Gananath Obeysekere in particular redefined the concept of detachment: "It is not a reversal to *methodological objectivism*," but rather is, and to invoke T. S. Eliot, "a recollection in tranquillity"—it is "the capacity to stand outside the experience and to mould the experienced into pregiven stanzic forms" (Obeysekere 1990:227–228). The collapsing of the subject/object, observer/observed distinctions that these works implied was explicitly articulated in Michael Jackson's *Paths towards a Clearing* (1989). He contrasted traditional empiricism with *radical empiricism*, which rather investigated the *interplay* of the dichotomous domains. Knowledge is born of this space *between*, teased from the fabric of its interactions and intersubjectivity. Insofar as objectivity bars entrance to this space, it is defensively used to protect ourselves from "the unsystematic, unstructured nature of our *experiences* within that reality" (Jackson 1989:3).[7]

At this point it is important to note that this stimulating stream of thought which stemmed from the 1980s was by no means the main tributary of writings on field research. In fact, we might even say that such periodic and scattered research on field emotions constituted a number of smaller subaltern streams in anthropology, which, when flowing into the mainstream,[8] were overwhelmed by the greater methodological tide whose source was in the 1970s. This dominant tide was largely advanced by sociologists who attempted to systematise fieldwork into a series of more positivistic research procedures and strategies. The trend gained pace in the 1990s and early 2000s by offering work that, if it discussed emotions at all, did so only from a traditional standpoint—e.g., it offered advice about how emotions could be "managed" and "tamed" in ways that would free fieldworkers to undertake more unclouded research. This dominant treatment of emotions in field research is one unfortunate symptom of what

we shall call "codification"—that is, the process by which participant observation has been increasingly formalised over recent decades into a series of neat research strategies and procedures more or less positivist in orientation. While it is sensible to recognise that such codification has had an important role to play, by being rooted in traditional empiricism it has been one of the essential factors animating resistance to the study of how emotion and intersubjectivity can be of empirical worth. Accounts by sociologists such as Jorgensen (1989), Shaffir and Stebbins (1991), Lee (1995), Kleinman and Copp (1993), Quinn Patton (2002), Adler and Adler (2000), Lichterman (2002), and Klandermans and Staggenborg (2002) when considering emotion at all, largely did so with the intention of offering guidelines about how researchers could navigate and control difficult field experiences that were commonly reported. The same may be argued for work by De Vaus (2001), Handwerker (2001), Spradley (1997), Hammersley and Atkinson (1995), and more recently in anthropological fieldwork volumes and texts by de Laine (2000), DeWalt and DeWalt (2002), Walsh (2004), and Bernard (2006)—all of which are very useful pieces in themselves but not works that open new doors to the pertinence of affectivity and emotion.[9]

While codification evermore dominates official fieldwork manuals written not only for ethnologists but for psychologists, cultural theorists, sociologists, and educators as well, many anthropologists have remained privately if not always publicly committed to taking seriously the value of fieldwork's intersubjective and experiential dimensions. Many of these anthropologists share an affinity with feminist theorists who have fought to retrieve emotion and subjectivity from marginal spaces. The abandonment of emotion into zones of pathology, radical and racial otherness and into the feminine, the outlawed, the exotic, the mad, or the bad is part of a wider traditional empirical movement in which the emotional, as Catherine Lutz has criticised, is "considered as an unfortunate block to rational thought" (1990:104).[10] If emotion is linked with irrationality, and the irrational with a kind of distorted vision, then emotion is simply grit in the eye of rational inspection. The syllogism misleads (as all syllogistic fallacies do) when empirical work produces data which contradict the syllogism's first premises. And such data now increases, if still only on the margins.

For instance, Lynne Hume and Jane Mulcock's (2004) recent volume shows with impressive clarity how ethnographic discomfort and awkwardness can be sources of insight and revelation; Antonius Robben (2006) has revealed the dangers of "ethnographic seduction" in his interactions with powerful

generals—seduction which can disarm critical detachment; Jean-Guy Goulet and Bruce Miller (2007) show how "extraordinary" field experiences (visions, dreams, illuminations) can be epistemologically informative; Linda Green's (1999) work illustrates how her own fear offered a way of understanding that of Mayan widows in Guatemala; and Michael Taussig's (1992) metaphor of a "nervous system" accounts for how ethnographers connect emotionally, viscerally, and intellectually with their fields. In addition, Ruth Behar (1996) and, more recently, Gina Ulysse (2002) have offered experience-based fieldwork narratives which attempt to give greater credence to the emotions and experiences that inform understanding.

While these works keep reflection on the emotional alive in a few corners of our expansive discipline, their numbers are not sufficient to stem the dominant tide which has seen systematic work into the researcher's consciousness significantly and precipitously slow since the early 1990s. To what extent this trend reflects a growing need in mainstream anthropology to present the face of participant observation in terms attractive to the current funding market is at present a moot point. But as the growing audit and regulatory culture increasingly privileges and monetarily rewards the kind of "trade-research" that C. W. Mills (2000 [1959]) feared would ultimately dominate all social science,[11] the imperative grows to continue the critical commentary on traditional empiricism's tendency to underplay the scientific, personal, and political consequences of the affective dimensions of fieldwork.[12] Any radicalisation of empiricism must take to task the traditional myth that methods purify subjectivity. It should rather ask how far methods *mould* subjectivity, not into patterns that efface all emotion (for this indeed is impossible) but into patterns that produce emotions of a different order, and also into attitudes too often prone to privilege only cognitive learning and cognitively driven procedures in social research.

As an important aside, as different academic fields aspire to reveal dissimilar dimensions of reality by means of their distinct methods, it hardly needs reiterating that when one discipline falls unduly servile to the admired methods of another, or else bends itself to fit a popular epistemological trend, it may not only compromise its own internal methodological development but also darken the critical and informative light that its maturation could have shed on neighbouring research procedures. Because "reality" tends to unfold in response to the particular set of methods by which it is studied, our formal understandings of the "real" are always somewhat bound by the limits of the methods we employ. The danger, of course, is that those aspects of reality which

sit beyond the reach of the specified method, by being seen as methodologically inaccessible, are somehow depreciated in their empirical existence. This is true with regard to what is researched and with regard to what aspects of the researcher are deemed methodologically useful. In this sense, methods constrain both what can be discovered and what spheres of subjectivity are viewed as empirically useful in the act of discovery. One way to loosen such a double closure is to bring within our investigative scope new dimensions of human and social reality by devising new modes of learning (cognitive and non-cognitive) by which they may be apprehended.

The Individual Chapters

In this spirit of opening rather than closing enquiry, let me turn to the chapters of this current volume—chapters that seek to build upon the existing strengths and insights of scholars whose work on emotions and method has not always been appreciated in mainstream sociocultural anthropology. Each individual contributor attempts from his or her unique standpoint to advance thinking on the use of emotion in varying domains of fieldwork. By necessity, the resulting inventory is far from exhaustive, and avowedly more exploratory than didactic. The volume on composite presents differing solutions to the problems arising when the traditional cords constraining the use of emotion are cut. It focuses on how certain disavowed and disassociated experiences can be shown to have heuristic, epistemological, and practical currency—experiences which, from the standpoint of traditional empiricism, have been viewed as impeding rather than assisting social research.

Many readers will doubtless be able to think of experiences and theoretical points that are under-emphasised in the coming pages. They may point to our exclusion of a more sustained consideration of how subjectivity, the field, and emotion are defined[13] and may call attention to how we could have further elaborated on how different anthropological traditions (national, thematic) have variously responded to the problem of subjectivity. Some might have hoped for a larger inventory of the subjective experiences viewed as corrosive to the research project, and there may be questions as to what extent encouraging the researcher's introspection may introduce into anthropology some of the common dilemmas that have historically inundated academic psychology. That there are many excluded standpoints we accept, but not without hope that these absences will encourage others to devote their energies to explicating themes that the limits of this volume have forced us to omit or, at least at times,

to pass over cursorily. This, then, is an admittedly limited intervention but, we are convinced, a timely and necessary one so far as a radical empirical approach can reveal certain hidden potentialities of field research. Having now acknowledged some missing elements, I will outline our positive contributions.

Part I

Our first section comprises chapters by Michael Jackson, Vincent Crapanzano, James Davies, and Francine Lorimer. Each chapter applies varying psychoanalytic and psychological ideas to the understanding of the researcher's subjectivity in the field. In this they show how psychological theory can inform anthropology in novel ways—not in the traditional sense of aiding research into the origins and meanings of social and cultural phenomena but in illuminating how certain field experiences may be rendered methodologically pertinent. Even those who argued that psychology had little to offer sociology or anthropology (e.g., Emile Durkheim and *L'Année sociologique*, we recall, held that as society was more than the sum of its individual parts it could not be reduced to psychology) would be hard-pressed to dismiss the relevance of psychology in aiding understanding into the researcher's subjectivity—for researchers, after all, are psychological beings, and by Durkheim's own admission "subject matter" for psychology. Using psychology in this way nonetheless raises certain problems: how far can psychology go in assisting our understanding when certain psychologies may presuppose "concepts of the person" which are to some extent situational? In this sense, if we do interpret our field experiences in terms of a favoured psychological perspective, is there a danger that when in the field we will create ourselves in this perspective's image rather than in the image of the person embodied by our hosts?

This is one question among others addressed by Michael Jackson in his opening chapter. Jackson shows that in using psychology, anthropologists need not necessarily do violence to local facts by reducing all field experience to these homegrown understandings. One can use both psychology and local epistemologies to unravel field experience (the use of the local, after all, has always been an important learning resource). Jackson shows this by building upon George Devereux's idea that much anthropological knowledge is an outcome *both* of disinterested observation *and* of the observer's struggle to allay his anxieties and find his bearings in a new environment. Jackson focuses on what he calls fieldwork's "liminal phase"—that psychological phase marking the period between separation from our familiar lifeworld and our more comfortable integration into the

new environment. The stresses of liminality, disabling and disorienting, are often unconsciously managed by researchers. One common way to manage the unstructured morass is to precipitously objectify and intellectually systematise the disorienting scene. While that strategy may help to "magically reorient ourselves to situations that seriously undermine our sense of self," when used defensively it can impede those insights that often arise when we allow ourselves to experience, slowly and non-defensively, the struggle to adapt. Jackson shows how, by turning to Kuranko oneiromancy to assuage liminal anxieties (rather than turning to objectification), he was able to understand the importance of dreams and portents in Kuranko life as well as to apprehend one situated, cultural solution to the general human experience of not-quite-fitting-in. He thus ventures beyond Devereux by showing that insights won through personal attempts at adjustment can illumine not only aspects of oneself and the world to which one is adjusting but also dimensions of the human condition itself: "In this view, the hermeneutic circle encompasses *three* horizons: that of one's own world, that of the society one seeks to understand, and that of humanity" at large.

Jackson's emphasis on learning by both studying our internal reactions and using local epistemologies is developed in a different direction by Vincent Crapanzano (chapter 2). Crapanzano starts by contesting those circumscribed and incomplete notions of participant observation that under-emphasize the importance of taking seriously our emotional responses. This reflective stance must consider both how field emotions affect the data we collect, frame, and interpret and how emotions are often structured by, and arise from, the field encounters themselves. Thus there are times when we can understand our emotions "transactionally": not as private phenomena but as "shared" or quasi objects that hover "in the between of an encounter." Here Crapanzano develops previous work on intersubjectivity by arguing that it is often framed by what he and recent psychoanalysts have called "the Third"—namely, a "meta-pragmatic" ordering principle, authorising the various "pragmatic, indexical, communicative, and interpretive maneuvers defining the encounter." The Third may be dominated by an overriding cultural concept, by a symbol (a god, a totem) or by one of the subjects of the encounter. Whatever dominates, the Third will influence what and how things are experienced by all parties to the encounter. Emotions do not necessarily emerge only out of "self," or even out of self in interaction with other (intersubjectivity); they may also emerge out of the structures that surreptitiously shape these intersubjective interactions. By recognizing that our emotions are thus influenced,

we can direct our analytic attention to discerning the nature of the very structures that structure these interactions.

James Davies, in chapter 3, accepts two principles developed by Jackson and Crapanzano: firstly, following Jackson, that if methods are used solely to stabilise the self they can obstruct anthropological learning; and secondly, following Crapanzano, that participant observation as a method should oblige an interest in our states of being during research. Davies shows through the analysis of one so-called "anomalous" field experience that the disorientation it brought, the way in which the fieldworker managed his disorientation, and how the disorientation altered his perception of the field, all point to experiences more widely encountered in field research than is generally acknowledged. Dwelling on the strategies by which we often manage unfamiliar and uncomfortable states and experiences (such strategies may include Jackson's "objectification"), Davies argues that what should concern us is not that anthropologists regularly perform such strategies in the field, and that these strategies are differently employed by individual ethnographers at different times, but that these "strategies of withdrawal" are often performed spontaneously, without either the full recognition of the fieldworker or a full appreciation of the methodological implications. To the extent that this spontaneous and unconscious use of protective fieldwork strategies remains oblique, masked and under-formulated, our ability to learn is significantly impaired, for such strategies often inhibit the immersion which is essential for anthropological understanding.

Francine Lorimer, in the section's final chapter, rather than discussing certain obstacles to knowing (Jackson, Davies), takes forward Crapanzano's call for emotional reflexivity by showing how the psychoanalytic concept of transference can be used to translate so-called uninformative emotions into revealing facts. One method of obtaining social insight she identifies as reflecting upon our "countertransference" reactions—i.e., our emotional responses to the researched. During her fieldwork in a psychiatric hospital in Denmark, Lorimer's countertransference to the patient, Caroline, helped her grasp how the relationships fostered between patients in the ward created insidious cycles of relatedness which, although promoting contact between patients, sustained certain self-destructive styles of relating that these very patients entered the clinic to overcome. Here the psychiatric space engendered clinical outcomes opposite to those it worked to attain. Lorimer thus questions whether the true value of antidepressants resides in changing our biochemistry in ways that blunt the habitual and destructive styles of relating that can sustain clinical depression.