The Self and Scientism

In this paper I examine some of the background to the debate about subjectivity in participant observation; the primary methodological technique in empirical anthropological research. Earlier versions of this paper given at seminars included more detail of my own field work experience. This I have had to leave out for more elaborate analysis in future writing. Meanwhile I have presented some of my field work as examples in the approaches explored and suggested for participant observation. There is a need for more explicit recognition of field work as personal experience instead of sacrificing it to a false notion of scientific objectivity.

The problem of subjectivity in research is recognised by most contemporary social scientists mainly to forestall criticism and further argument. The traditional response is to refine the 'objective' methodology by formally eradicating the direct link between observer and observed. For example, the questionnaire method in much empirical sociological research predetermines the subject matter and questions arising and information transmitted is selective and curtailed. This premeditation and control over interaction is presented as proof of objectivity. The questionnaires are administrated by assistants (nameless and usually female) and the 'hard data' written up by (named and usually male) research lecturers. The larger the sample and the more random the selection, the more 'scientific' the findings. The people interviewed are usually willing to volunteer fundamental and unpredictable insights which are merely jotted down under supplementary 'remarks'. Even in more informal unstructured interviews the inquisitor never abandons his dominant role. Other information acquired in less formal contexts is referred to as 'impressions', and 'soft' data to be tested by the hard data. (See Young and Willmott 1962: Appendix). The method is inherently authoritarian.

In anthropological participant observation there is greater reciprocity in the exchange of information. Here the problem of subjectivity becomes explicit. The field worker, as opposed to those who analyse other peoples' material, has a peculiarly individualistic and personal confrontation with 'living' data. This close contact has made anthropologists feel vulnerable to criticism from those who employ formal techniques of distancing between subject and object. Hence the peculiar coyness which anthropologists have shown in discussing their relationship with the various people they have studied.

The participant observer does not deliberately impose preconceived notions of relevancy and ready worked hypotheses on the data to which he has access. Despite criticisms from the formalists, this absence of filtering is the source of strength. The individual is open to a complete range of information and not merely what people say they do.[1] This material is of course analysed in the

light of existing anthropological theory but not prejudicially at the field work stage. In distinguishing the methods of sociology and anthropology, Maquet (1964) has justified the use of participant observation on purely technical grounds. In non literate societies, written sources, written questions and answers were not feasible and the totality of customs largely unknown to the observer, thus requiring long stays and 'indirect' observation. Such methods were unnecessary in the study of 'one's own' literate society ... 'where the whole culture is taken for granted'. The broad difference in techniques which Maquet describes might indeed have this historical foundation, but their merits cannot be judged solely in terms of their subject matter. Participant observation is equally valid in 'one's own society'. To take the whole culture 'for granted' is also to be guilty of subjectivity, more insidious because it goes unrecognised. Both the study of the observer's and another society involve subjectivity, but of a different order. I wonder if the belief in objectivity attained by studying another society is unconsciously explained by geographical not theoretical distance.

As in dry research methodology, the participant observer does have a problem of subjectivity. This cannot be resolved by distancing, repression and short cuts to abstractions. Objectivity is an ideal model to work with, not a fact. In the study of human being by another human being, (and what better medium is there?), the specificity and individuality of the observer are ever present and must therefore be acknowledged, explored and put to creative use. It is fashionable now for authors from a variety of disciplines to give an apologia or acknowledgement of his or her ideological stance as Marxist, liberal structuralist etc. in the preface. This kind of confession is no substitute for the continuing and conscious working through of these implications. Similarly in psychoanalysis, it is not enough for the patient to be labelled; his situation has to be examined and understood through hundreds of hours of analysis. Political interests are also now made more explicit or better understood. This applies especially to anthropologists looking back on the colonial era. (Macquet 1964). Less attention is devoted to the individual characteristics of the observer as important subjective factors conditioning knowledge. In any case in anthropological research, few analytic tools or categories have been developed to explore the various forms of subjectivity.

So far, the remedies suggested by anthropologists have mainly involved greater external control rather than any creative use of the observers' individual resources. To deal with what he called 'the personal equation', Nadel considered the selection of anthropologists on the basis of 'psychological testing' (1951: 50). I wonder by what culturally loaded criteria would candidates be deemed suitable, and for which culture? As another means of 'overcoming the limitations of the personality' Nadel suggested teamwork.

While considering Fortes and Evans Pritchard's awareness that the 'passing through a single mind' of the data is peculiarly valuable, Nadel asserts: 'once more we must face the issue that science cannot in any respect, be private' (1951). Again, as is the case with many others, impersonal verification is confused with objectivity. (Macquet 1964).

Given this distaste for privacy in science, it seems all the more extraordinary that the anthropologist's private and personal experience of field work is not exposed to view. Since almost nothing about the people studied is dismissed as private, taboo or improper for investigation, the same should apply to the investigator. I am not suggesting that everything be then put into print or theses for public scrutiny. Already many things in field notes must remain confidential, are later dismissed as trivia or disguised. The problem is that the fieldworker's personal reactions and experience are suppressed or dismissed from the outset. A certain personal exposure may in time be seen not as professional disaster but intellectual growth. Revelation of the 'humanistic and experiential' elements of field work has been advocated on moral grounds; as an exploration of moral relativism (Swallow 1974: 58). My reasons for advocating this aspect of field work are not rooted in morality, but directly to the epistemological problem of subjectivity which is perhaps implicit in Swallow's discussion. Too often the personal is represented in opposition to the objective, when the latter merely conceals the personal in pretentiousness. This dichotomy of knowledge is reflected in the sexist division of labour and knowledge in our society. Women are less inhibited about exploring and expressing the personal element, although they are expected to apologise for this in academic debate. Women are more likely to comprehend a theory through an example or image whereas men will grasp a theory through generalisation; given the different upbringings of males and females and the exclusion of women from direct economic and political power, only indirectly obtainable through personal relations with individual men, it is not surprising that the consequences are expressed in mode of thought. Arais Nin describes the polarity in her diary:-

> 'Now analysis is revealing how little objectivity there is in man's thinking... Man generalizes from experience and denies the source of his generalizations. Women individualizes and personalizes, but ultimately analysis will reveal that the rationalizations of man are a disguise to his personal bias, and that woman's intuition was nothing more than a recognition of the influence of the personal in all thought'.
>
> (1967 Vol. II: 23-24)

In this quotation I interpret the concept intuition as culturally loaded, not as something inherent in all females. If as anthropologists we accept and explore different modes of

thought in different societies, we should not be averse to finding them within one society and in neither case does it follow that these variations are genetically determined.[2] Actually women lose out on several accounts. In a context where the specific is described as 'hard', scientific and objective fact, it's opposite is 'airy fairy' speculation, emotional and soft - womens' domain.[3] In another context where fact is equated with 'vulgar empiricism' and it's opposite is theory, women are seen to be the fact gatherers and men the theoreticians.[4]

Rodney Needham in his discussion of lateral symbolism points out that the dualism in this debate 'is a central issue in any humane discipline and as an essentially philosophical problem it may not admit any definite resolution. It has to do with the variable meanings attached to such abstractions as "fact" and "theory"' (1973: xxxi). In the case of our own society the 'fact'/'theory' dualism is transposed to the female/male division which corresponds to a political and economic actuality and is reinforced by self-fulfilling ideologies. Whatever 'female thought' may be, it is the one which is undervalued. The same goes for so called 'primitive thought'. There are parallels between the kind of thought which Arais Nin associates with women and 'the science of the concrete' discussed by Levi-Strauss (1966: 15-22). The participant observer is not at once removed from his material. His method involves working through images and anecdote.

Nevertheless whether through scientistic or sexist bias, the personal is often denigrated in anthropological monographs. The 'I' of the observer sometimes disappears altogether as though the material was acquired by impersonal procedures. The classical handbook 'Notes and Queries' (1967) tells the fieldworker that really only amateurs suffer from 'bias'. 'Scientific' training successfully obliterates cultural and personal history and presumably the self (1967: 27).[5]

Evans-Pritchard's considerable insight into both his methods and personal form of 'Nuerosis' (1940: 9-15) and indeed Malinowski's first public account of field work (1922: 2-25) have not always stimulated anthropologists to give more or even as much information about their research.[6] From the 1960's a few anthropologists have presented more autobiographical accounts. (Turnbull, 1961; 1973; Maybury-Lewis, 1965; Read, 1965; Mead, 1972; Chagnon, 1974). Earlier in the most explorative and sensitive account of the relationship between the outsider and people encountered in fieldwork, Smith-Bowen (1954), felt obliged to fictionalise events and persons and publish under a pseudonym, so re-affirming the tradition of separating the 'subjective' from the public body of academic work.

In some of the publications, the anthropologist emerges as narrator (Conrad style) and actor along side other

characters in the exotic setting. Detailed descriptions, normally reduced to the opening pages of monographs, are given of scenery and personalities. The stream of events is action packed. Often the only structural unity of the narrative appears to be the chronology of the stranger's visit. This is a new kind of ethnography based on 'true story', subject to limitations of which literature is free. Such techniques may be a welcome rebellion from the depersonalised monographs of the past, but they have not yet resolved the problems of subjectivity in participant observation research.

Take Colin Turnball's study of the Ik (1974) which on the basis of long term observation, is an informed rival to the genre of travelogues so despised by Levi-Strauss in his own autobiographical account 1955 and 1963: 17-18). Turnbull considers it right that 'any description of another people ... is bound to be subjective'. Since he had no access to previous accounts of the Ik, he believes that he started with 'a clean slate', without 'a preconceived notion', just 'clinical observation' (1974: 13). He omits to mention his preconceived notions about all human societies which he brought in his own head and landrover. His personal asides are presented as universals which we readers are supposed to support. They are no more than his own commonplaces; our intellectual journalist reporting. His subjective exposure lacks any self analysis and he certainly can't analyse his companions. For instance, he interprets laughter as merriment never as hysterical distancing or catharsis. Smith-Bowen never made that mistake, perhaps because she let herself experience the same.

Napoleon Chagnon has attempted to preserve 'an intimate relationship between ethnography, methodology and theory' (1974: x) and in two chapters (1 and 5) gives a personal account of his fieldwork. There is virtually no relationship between these chapters and his main work which consists largely of data gleaned by the latest technological gadgets. Details of his fieldwork are justified more in terms of technical/precedural problems of data collection; i.e. which villages he could stay longest in, why he had to leave others, rather than any theoretical link between the self and others. Discussion of the observer's experience is described as 'the non quantifiable aspects of fieldwork' (162), thereby conveying the feeling that if something can't be counted, it demands no alternative analysis description. If, as the preface claims, Chagnon is trying to show how the exotic becomes commonplace (page: viii), he fails. His form of personal revelations exploits on every page the exotic or bizarre as would be understood by the North American or European general reader. The chapters make exciting and sensational reading. We are rarely presented with the range of Chagnon's inner feelings, instead his escapades and heroism in the pursuit of science. One night the natives were going to crush his skull, another time he caught a 'raging' fungal infection of the genitals,

after borrowing a man's loin cloth. The opening paragraph titillates the reader's fantasies about Shaman and savages. Familiarity in the exotic is conveyed not by his description of the people around him, but by the 'commercial breaks' for his IBM printout and the Tri-x for his Pentax. Our Napoleon from Pennsylvania is better than Biggles or the Boy's Own Annual.

Having been so unkind about others, at this point I should show myself amenable to self examination especially in relation to the kind of part an anthropologist imagines he or she is playing in the field. Among the Pygmies, Turnball was romantic, among the Ik a cynic, with the Yanomamo Chagnon appeared hero; what was I, a lone woman living with the gypsies in southern England? I could tell some stories exotic precisely because they are set not in rain forests nor deserts, but on the edge of greater London just up the M.1. My tutor, on reading my notes exclaimed, 'They're more violent than the Dinka!' I plan to describe the strangeness; but not telescoped and wrested from the commonplace and imponderabilia of everyday. For the moment let's dissect what I thought I was at, in fantasy not ethnography.

Early in my anthropological studies I was impressed by a common assertion among both men and women anthropologists that the female anthropologist in the field is not 'hampered' by her sex because she is treated as an 'honorary male'. I didn't realise at the time how deeply this appealed to deep contradictions in my own history. This belief is confirmed in 'Notes and Queries': 'Among very unsophisticated natives ... a woman may find that she is regarded primarily as a stranger and is given the status of male' (1967: 30). To my surprise and perhaps disappointment, when I entered the field I did not find this so. I had to be extremely cautious in talking to men, usually making sure that a woman was present since non-gypsy women are regarded disapprovingly as licentious and immoral, just like the non-gypsy (gorgio) stereotype of gypsy women. Women, not men, had to be my main informants and allies. The rigid male-female segregation meant that any woman seen talking alone to a gypsy man who was neither kin nor husband, was accused of sexual infidelity. I had to be careful and more conscious of myself as female and began to wonder if this was so different in some other field work situations.

Increasingly, I suspect that women anthropologists are given ambiguous status in the field, not as 'honorary males', but as members of an alien race. So where did this 'honorary male' come from? The idea has its roots instead in the anthropologists' own society where the ideals of male and female behaviour are as marked. It requires some nerve among women of my society to travel alone without plans and timetables.

"Adventure is pulling me out. When a man feels this, it is no crime, but let a woman feel this and there is an outcry."

Anais Nin (1974 Vol. 2: 51)

Now for self investigation:- In the single sex boarding school where I was educated from the age of nine to eighteen, the 'world' was divided into four 'houses'. These cosmological institutions were not represented by any buildings; they were groups of girls competing for cups in field sports, conduct, deportment and drama. Most significant to this discussion; the four houses were named Rhodes, Livingstone, Shackleton and Scott. I belonged to the last. So our models were all white colonisers or explorers. The trouble was they were all males. These heroes, not heroines set an example to which we, as Penelope was to Ulysses, could never, nor indeed, should ever aspire.

Brought up only to marry or beget a Scott or Livingstone perhaps I found my journey into Gypsydom especially exhilerating, although I rejected entirely the idea of being a coloniser. I hurtled down motorways in my fifteen hundred weight van, I loaded heavy scrap iron and dwelt with strangers and nomads, so escaping the strictures of the domestic role allotted the females of my own kind. In addition, this rite de passage made me an honorary male among those back home. By this experience, the female anthropologist not only achieves equality in her own society, she might unconsciously feel a confident separation from the domestically burdened wives and mothers in the society she is studying.

Whereas the female anthropologist, not accompanying a husband, is rejecting her conventional destiny by the act of fieldwork, the white male anthropologist is completing his (See also Levi-Strauss (1963: 42) for a discussion of the function of travel among young French men). That is why a little more self awareness in motives might be helpful to both male and female anthropologists in the presentation of the self in their fieldwork confessions.

It is ironic that the man who first developed and gave scientific status to participant observation in anthropology also kept a personal diary, which has caused only embarrassement or been overlooked as an invaluable adjunct to field work by his colleagues and academic descendants.[7] Malinowski's diary (1967) is a remarkable case study of the concealed subjectivity in field work method and general ideology, distorting both evidence and theory. Still today personal and cultural conflict in the field are relegated to the anecdotal and oral traditions of faculty gossip (Swallow, 1974). Thanks to the consent of Malinowski's widow we have a record of his on the spot reactions. Among other anthropologists their reactions are usually only recalled after field work and therefore changed in their retrospective autobiographies.

Unfortunately Malinowski did not use his diary in the way which I shall advocate - as a means of exposing and exploring subjectivity. Instead he used it as a privatised escape from the fieldwork situation to maintain a 'sane' and familiar internal order in an alien land, and as a punitive stimulant to his rigid work ethic. His dependence on the norms and values of his own culture were at risk; for example he was worried he might forget about academic commitments and the obligations of sexual fidelity. The diary became an internal dialogue with a culture-bound conscience where the private and subjective were artificially separated from the professional and objective. On the one hand he was publicising and eliciting appreciation for the Trobriander's institutions in his official writing, on the other, he was contending with his hostility to an alien race in terms similar to those of his colonial contemporaries.

There are four aspects which it would be useful to isolate in Malinowski's diary.

1) Attitudes to women and sex (the two for him were rarely separated).

2) Personal feelings about the Trobrianders.

3) Interaction with local white men.

4) Ideas about keeping a diary.

1) He expresses a longing for a white woman, in particular his future wife whom he considers has 'the miraculous power to absolve sins'. Raymond Firth cites this as proof of the depth and sincerity of his love (1967: xviii). This woman is identified with white civilisation (Malinowski 1967: 148). But he also has lustful feelings for another white woman with whom he cannot break contact. His conflict seems to be the classical one in western civilisation between the pure woman (wife) and the sexual fiend (whore). On a day to day level, Malinowski is confronted with his feelings towards the black women who became the victims of his projections and concepts of the whore. Sexual relations with them are seen as 'whoring' and 'sloshing in the mud' (181). Sometimes he confesses to his sense of their beauty (255) but bitterly regrets having 'pawed' one (256). His diary serves to goad his conscience, and control any deviation from his self imposed sexual code and that of his own civilisation. This can sometimes only be achieved by negating sexuality in women: 'Moral tenets: I must never let myself become aware of the fact that other women have bodies, that they copulate' (1967: 249). The tendency for women to be seen mainly as sexual objects may well have encouraged anthropologists to avoid or underestimate them as persons and informants (see Ardener's discussion 1972: 137-138).

For a long time I was guilty of a kind of sexism in my own field work. At first I considered my segregation among

gypsy women only as disadvantage. I was always trying to get through them to the men where I presumed all the action was. It was only gradually that I actually became aware of the important political and economic role of the gypsy women in their own right and precisely because they were women. My initial prejudice I have tried to rectify (Ohely 1975). My field work mistakes arose partly because of a belief in the separation of my 'personal' political views on feminism and my 'objective' role as researcher in another culture. After the London Womens' Anthropology Workshop in 1973, I suddenly saw that the two were interconnected.

2) Malinowski's feelings about the Trobrianders

In its strongest aspects, he might now be accused of racialism because of his use of the word 'nigger', just as he could be accused of sexism in his indiscriminate use of the word 'whore'. The first time 'nigger' appears in the published text (1967: 154) there is an evasive footnote giving Webster's definition, and designed rather naively to take away the full impact. If as the editors claim, the word was non pejorative for Malinowski, one wonders why he should have so assiduously excluded it from his public texts.

Malinowski's use of the word in a private context is interesting to the reader because it indicates that he was a carrier of the stereotypes and underlying values of his own culture, even though he wanted to take the Trobriand culture seriously. Obviously the strain and stress of field work, for example, the personal isolation and 'alien' life style are bound to bring out the 'worst' in anyone, but that does not explain away the cultural form which the 'worst' takes. Irritation with members of one's own race and one's own sex would be expressed differently; the peculiar faults of the individual rather than his or her social category would be exaggerated and deframed. In examining the dilemmas faced by field workers, a psychologist, Wintrob (1969) tends to examine problems of ambivalence, racialism and questioning of motives more as symptoms of stress rather than as valid problems in themselves. Malinowski's 'racialism' cannot be described simply as stress, it must also be explained in terms of white man's 'culture'.

The contrast between Malinowski's professional or intellectual aims and his private feelings, which reflect his own culture, appears in a single page. He considers composing a memoir on 'the value of Ethnographic Studies for the Administration ... above all the knowledge of a people's customs allows one to be in sympathy with them, and to guide them according to their ideas' (note there is the presumption of 'guidance'). Then after been misinformed about a kula expedition, Malinowski expresses 'hatred for the niggers' (1967: 238). He is not sufficiently self conscious to set his annoyance at the natives' independence in the context of white/black relations.

As a role whose country suffered Austrian rule, Malinowski was, as Lucy Mair suggests (1957: 232), able to identify with oppressed minorities. However this may have blinded him to his status in Melanesia. Deracine he became a cosmopolitan communicating with Europeans, Americans and Australians and speaking their language. His identification was limited mainly to white men of the industrial world, so his racial status and origin became more important than his nationality. In the field therefore, he was identified by the Trobrianders as a colonial and they obviously concealed information from him which might prejudice their position. Elsewhere I have examined how the Trobrianders' apparent ignorance of paternity, as told to Malinowski, might be explained by his status as white man unwittingly associated with the missionary's decrees on sexual behaviour (Oxford Womens' Anthropology Symposium 1975).

Malinowski makes an explicit connection between his personal reactions to a Trobriand individual and the policies of a colonial oppressor (1967: 279). This is followed in a manner comparable to free association, by a reference to sex between the races, more importantly his dismay at a white woman's relations with a black man. Racism is thus interwoven with sex and sexism, and all are symptoms of white male chauvinism. Whereas white male/black female sexual relations may be 'sloshing in the mud'; a regrettable past-time, black male/white female sexual relations, in any analysis of white man's racialism, is the ultimate taboo, since it undermines the presumption of white male 'supremacy' and white female 'purity'. In this racialist and sexist system, where the male is considered superior to the female, it also gives alarming superiority to the black man over the white woman. This passage in the diary of an individual indicates the links explicit or unconscious between the particular or subjective and the general.

3) Interaction with local white men.

Malinowski's diary reveals a similar ambivalence or hostility to white men in the Trobriands as he does towards women of any race and the native Trobrianders. To the lieutenant governor he is obliged to be deferential for the sake of his visa, but regrets that 'paying attention to this crew simply banalises my work' (1967: 128). Firth praises Malinowski's thumbnail sketch of this 'legendary figure' (1967: Introduction), but I am left dissatisfied. This man at the 'apex of the official pyramid' could have been considered as worthy of as the Trobrianders as a subject of study. Malinowski accepts the white administrators' 'power over the natives' (1967: 167). Yet he is always trying to eradicate them; 'What is terrible is that I am unable to free myself from the atmosphere created by foreign bodies: their presence takes away the scientific value and personal value of my work' (1967: 163). On the contrary, his amnesia towards the white administrators had considerable repercussions on the 'scientific' value of his analysis. The

Trobriand society was over represented as a functional whole, with economic and political self-sufficiency.

The Trobriand Islands were inhabited not only by white administrators but also missionaries and traders. Instead of pursuing the consequences of these immigrants invasions for Trobriand society, Malinowski sees his relationships with the white men as personal intrusions on his objective research. He continually tries to avoid intensive conversations with whites, especially with the trader Raffad whom he finds so intelligent and sympathetic that he fears the man might become his 'main subject of study' (1967: 264). The ideal model of the isolated, simple society didn't exist, even at the outset of intensive anthropological field work.

Like Malinowski, I found myself, at the beginning of field work, trying to blot non-gypsies or gorgio administrators from the landscape. I saw them as useful sources of background information, a way in to the gypsies, rather than as important constraints within gypsy society. If I had more self consciously analysed my personal desire to disassociate myself from these petty gorgios, I might have recorded everything about them and treated their words and actions as equally if not more 'exotic' than those of the gypsies. Again I have tried to rectify this in later analysis (Okely 1975a: Ch. 2).

4) Malinowski's ideas about keeping a diary.

As I have elaborated above, Malinowski used his diary more as an escape from the field than as an intellectual tool in research, yet in one astonishing passage he recognises it's potential:-

> "a diary is a 'history' of events which are entirely accessible to the observer, and yet writing a diary requires profound knowledge and thorough training; change from theoretical point of view; experience in writing leads to entirely different results even if the observer remains the same - let alone if there are different observers! Consequently we cannot speak of objectively existing facts: theory creates facts. Consequently there is no such thing as 'history' as an independent science.'
>
> (1967: 114).

It is regrettable that he did not extend the diary's function as the link between subjectivity and 'scientific' participant observation.

As in social anthropology, the discipline of psychoanalysis is exploring the problem of the analyst's subjectivity, 'notably because the treatment has come more and more to be understood and described as a relationship' (Laplanche and

Pontalis 1973: 92). The technical term 'Counter-Transference' refers to the analyst's unconscious reactions to the individual patient. Freud stresses that 'no psycho-analyst goes further than his own complexes and internal resistances permit' (1910 cited in Laplanche & Pontalis). Hence every analyst has first to undergo analysis. Techniques of dealing with counter-transference take several forms: to reduce it as far as possible by personal analysis; to exploit it in controlled fashion as a guide to interpretation. The analyst's unconscious is seen as the ideal means to understanding the patients' unconscious (Laplanche and Pontalis 1973: 92-93).

Social anthropology might explore analogous methods. Subjectivity as influenced by individual personality, cultural history and gender should be analysed not repressed, and exploited for finer observation and interpretation. It would be of additional value if the anthropologist had undergone personal psychoanalysis, but this is not the core of my suggestion. Whereas the patient is battling largely with his personal history, the anthropologist is also battling with his cultural and social history. And here psychoanalysis has something to learn from anthropology. The anthropologist as participant observer is, like the psychoanalyst, involved in a relationship; this time between the self and many others; between two cultures. There is a problem of cultural counter transference.

The methods which I tentatively advocate for confronting and making creative and theoretical use of this relationship are as follows:-

(a) Self-analysis

(b) The Diary

(c) Autobiography.

(a) Whereas a diary is usually a record of conscious thoughts and experiences known to the author but concealed from others, the kind of self analysis recommended by Karen Horney (1962) demands the discovery of unconscious links in thought and experience. 'This fundamental disinterest in the self is one of the great difficulties in self analysis (1962: 144) and 'the real difficulty is not that of intellectual understanding but that of dealing with resistances' (1962: 146). In this context I would describe both Malinowski's and my own disinterest in administrators, white men or gorgios as a cultural resistance.

Since thought moves faster than the pen, it would be useful to jot down key words, concepts and images, as well as apparently disjunctive free associations. All these may bring insight at the time or at a later date; a structural analysis of the self.

(b) The Diary as a means of self exploration should be regarded as an essential part of field work methodology.

It could be the place for the key words and jottings of self analysis, but in addition the place for more conscious thoughts and experience. No anthropologist would consider writing a retrospective monograph about the people studied if he had taken no notes at the time of participant observation. Details of conversations and events are lost with each day of delay in recording, so that the fieldwork risks falling back on pre-field work presumptions. The same goes for any description of the self in the field. Moreover the very act of recording stimulates and developes ideas at the time.

In 'Notes and Queries' the use of a journal is suggested merely as a supplement to note taking, its junction being mainly to record the chronology of events and seasons. There is a single mention of 'self', (no other concession to subjectivity), but no indication that it's description be in other than medico-spatial terms; it is lost in the 'weather' and 'special events' (1967). Audrey Richards (1971) recommended the use of a diary along similar lines but her additional comments hinted at its potential for self examination.

In my own field work I recorded all my data in diary form, thereby avoiding the problem of pre-conceived categories. Several copies were made of each typed page and my material categorised in ways which often appeared relevant long afterwards. I bitterly regret that I rarely thought it 'professionally relevant' to record or analyse at length my personal reactions and dilemmas as they occurred. In my postgraduate training examination performance was assessed mainly in the Malinowski paper. I was encourage to read virtuallly all his articles and books except the diary.

There was another reason for my dedication to a science which excluded the self: my notes were to be examined by my employers, a research organisation, some of whose members had expressed grave doubts about the 'reliability and 'objectivity' of anthropological methods. At the first stage of field work I had to conceal the fact that I was taking notes at all. Instead everything was supposed to be recorded in massive questionnaires. These I hid in a suitcase under my bunk and later sabotaged by giving them to social workers to administer. The Gypsies gave brilliant and ambiguous answers which I was told despairingly couldn't possibly be coded.

During some three months follow up field work I kept a diary, which of course could never recapture the details of earlier responses. A proper development of simultaneous self-analysis awaits my next project. However, I can explore to some extent the third and final method:-

c) Autobiography

Here the writer attempts to describe and recreate the stages of past experience (Abbs 1974:6-7). The presentation of the past will vary in accordance with the present time chosen to examine it. Retrospective analysis of field work will give another dimension to contemporary analysis and the diary. Perhaps the final product should consist of an analytic combination of all three, the aim being that self analysis should have influenced and enriched the research at all stages. It is debatable how far the autobiographical exploration should be a self contained section; at the very least it should be recognised as an integral part of published research. Pocock's valuable 'Idea of a Personal Anthropology' (1973) recognises and explores a person's assumptions about his own society, embedded in written texts and recorded interaction with another people. In this paper, I have chosen to concentrate more on the refinement of self consciousness in the field situation, the actual process of interaction.

In the creative use of autobiography, anthropologists can learn from literature. The greatest writers have often had to work through most explicitly their youthful autobiographical experience:- Tolstoy in Childhood, Boyhood and Youth; George Elliot in 'The Mill on the Floss'; James Joyce 'In Portrait of the Artist'; and D.H. Lawrence in 'Sons and Lovers'. Philosophers have felt compelled to write autobiographies in addition to, and separate from their main work:- Rousseau, J.S. Mill, Sartre and De Beauvoir. For others the autobiography has stood as their single product for example; O'Sullivan (1933).

So far I have emphasised the methodological advantages of self analysis and autobiography in anthropology. The experience and a full and creative record of it are valuable in themselves. The anthropologist, entering another society crosses also a boundary of self definition. Some novelists have dealt with this experience most successfully in recording the passage between youth and adulthood. (Balzac, Stendhal, Flambert and Hesse).[8] Unfortunately the anthropologist's rite de passage between two cultures has largely been defined only in the context of his natal culture. That is, he is said to undergo a painful and isolating experience in a criminal area before he or she returns as a full member of the academic club. This witticism thus disposes of the experience. The anthropologist is then said to enter the field in order to return, he or she is not said to be in anthropology in order to enter the field. Both Malinowski (1967:161) and Levi-Strauss (1963:17,43) tried to play down field work as a life experience, although their own evidence contradicts this pose. Perhaps Castenada has aroused such interest precisely because he is prepared to abandon a formal objective purpose for new and personal knowledge on the other side. (1970).

Field work is a dramatic contrast to the private, sedentary and academic demands of university existence. Practical and manual skills may be greatly valued, also the ability to interact with a wide range of people. In participant observation in a non literate society, my usual manner of dress, accent, past education were sources of stigma. Details of my past, important

to me, were irrelevant to the gypsies, other details to which I felt indifferent were to them most meaningful. All this can be both shattering and exhilerating.

My main field work has been within the geography of my own society. I was travelling through or camping in towns I'd known before, but in this different context the landscape was transformed. When I knocked on doors asking for 'any old iron, batteries or rags', I often came face to face with people of my own 'background' and social class, but they were aliens and they treated me as one. Often I suffered a profound alienation. After crossing an ethnic boundary it seemed I belonged nowhere. (See also Lawrence 1935 end of Chapter one). This cannot be eradicated by self-analysis although better understood and used imaginatively. If you let go you see aspects of yourself as mere props. You are made aware of your 'personal anthropology', its flaws and its virtues not just through retrospective nor even verbal analysis, but through action. Dedication to objectivity is exposed as the ego of your own history.

Judith Okely.

Notes

1. Willmott and Young (1962) in their 'classic' conceded: 'For the most part we can only report what people say they do, which is not necessarily the same as what they actually do' (1962:14).

2. Not in one's own society at least is anyone obliged to accept and encourage this dichotomy as politically permanent. I would agree with Levi-Strauss that 'the anthropologist who is critic at home and conformist elsewhere is therefore in a contradictory position' (1963:384). But I disagree with his suggestion that the anthropologist should take no action in his own society for fear of 'adopting a partisan position' elsewhere (p385). Levi-Strauss' ideal objectivity is falsified since acceptance of the status quo is as much a subjective stance as intervention.

3. It is not coincidental that Swallow's paper appears in the special issue on 'Women in Anthropology' (Cambridge Anthropology 1974), and that the earliest draft of my paper was first given to the Women's Seminar at Oxford.

4. Even in the discipline I get a sneaking feeling that obscure abstractions are considered among some contemporaries as the sole proof of intellectual power as opposed to the infinite mental intricacies of field work problems. Is field work destined to be another female occupation like social work?

5. As it turns out, the ensuing pages of Notes & Queries occasionally reveal some amusing examples of colonial paternalism and wholesale generalisations about 'other' peoples, which social anthropology was itself trying to discredit:- 'The unsophisticated native is often suspicious of all strangers' (1967:29); 'patriotic flattery may be us useful' (1967:33); 'women can be just as offended by the offer of (to them) unsuitable beads as are European girls' if given presents suitable for elderly women' (p.33).

6. Raymond Firth (1936 republished 1963:10) in his very discreet description of himself and his methods yet feels obliged to apologise for 'this somewhat egoistic recital'.

7. Raymond Firth (1967 Introduction) considers that the diary 'in it's purely ethnographic sense cannot be ranked as more than a footnote to anthropological history'.

For Geertz (1974) the diary exposes any previous claim that anthropologist's had some 'unique form of psychological closeness, a sort of transcultural identification with our subjects'. He neglects both self-analysis and biography as techniques for understanding the interaction.

8. Here I refer specifically to Balzac's 'Les Illusions Perdues'; Stendhal's 'Le Rouge et Le Noir'; Flaubert's L'Education Sentimentale; and Hesse's Demian.

Bibliography

Abbs, P. 1974. Autobiography in Education. Heinemann Educational Books, London.

Adams, B., Okely, J., et al. 1975. Gypsies and Government policy in England. Heinemann Educational Books, London.

Ardener, S. 1975. Belief and the problem of women In La Fontaine 1972 and Ardener 1975.

Ardener, S. 1975. Perceiving Women. Malaby Press, London.

Castaneda, C. 1970. The Teachings of Don Juan, Penguin, London.

Chagnon, N. 1974. Studying the Yanomamö. Holt, Rinehart and Winston, U.S.A.

Evans-Pritchard, E.E. 1940. The Nuer. Clarendon Press, Oxford.

Firth, R. 1957 a) and 1963. We the Tikopia Beacon Press, Boston.

..... 1957 b) Man and Culture Routledge and Kegan Paul, London.

..... 1967 Introduction In Malinowski 1967.

Geertz, C. 1974. "From the Native's Point of View": on the Nature of Anthropological Understanding. In Bulletin of the American Academy of Arts & Sciences Vol. XXVIII No.1. 26-45.

Henry, F. and Saberwal, S. 1969. Stress and Response in Fieldwork. Holt, Rinehart and Winston, U.S.A.

Horney, K. 1942 and 1962. Self-Analysis. Routledge and Kegan Paul, London.

La Fontaine, J, 1972. The Interpretation of Ritual. Tavistock London.

Laplanche, J. and Pontalis, J-B. 1973. The Language of Psycho-Analysis. Hogarth, London.

Lawrence, T.E. 1935. Seven Pillars of Wisdom, Jonathan Cape, London.

Levi-Strauss, C. 1963. Tristes Tropiques. Atheneum, New York.

..... 1966. The Savage Mind. Weidenfeld and Nicolson, London.

Mair, L. 1957. Malinowski and the Study of Social Change. In Firth 1957 b).

Malinowski, B. 1922. Argonauts of the Western Pacific. Routledge and Kegan Paul, London.

..... 1967. A Diary in the strict sense of the term. Routledge and Kegan Paul, London.

Maquet, J. 1964. Objectivity in anthropology. In Current Anthropology 5:47-55.

Maybury-Lewis, D. 1965. The Savage and the Innocent. World Publishing Co. Cleveland.

Mead, M. 1972. Blackberry Winter. Touchstone, New York.

Nadel, S.R. 1951. The Foundations of Social Anthropology. Cohen and West, London.

Needham, R. 1973. Right and Left. Chicago Press, London.

Nin, A. 1974. The Journals of Anais Nin 1934-1939. Quartet, London.

Notes and Queries on Anthropology 1967. Routledge and Kegan Paul, London.

Okely, J. 1975 a) Gypsy Identity In Adams, Okely et al 1975.

..... 1975 b) Gypsy Women: Models in Conflict In Ardener, S. 1975.

O'Sullivan, M. 1953. Twenty Years A-Growing. O.U.P. London.

Pocock, D. 1973. The Idea of a Personal Anthropology. Paper given at the Dicennial Conference of the A.S.A. Oxford.

Read, K.E. 1965 The High Valley. Scribners, New York.

Richards, A. 1971. The Use of a Diary. Paper given at the Institute of Social Anthropology.

Smith Bowen, E. 1954. Return to Laughter, Gollanz, London.

Swallow, D.A. 1974. The Anthropologist as Subject In Women in Anthropology, Cambridge Anthropology Vol. I. no.3.

Turnbull, C. 1961. The Forest People, Reprint Society, London.

..... 1974. The Mountain People, Picador, London.

Wintrob, R. 1969. An Inward Focus: A Consideration of Psychological Stress in Fieldwork In. Henry and Saberwal.

Young, M. and Willmott, P. 1962. Family and Kinship in East London. Penguin, London.