Qualitative research techniques have long been used by social and cultural anthropologists to analyse information gathered during field work. More recently psychiatrists, psychologists, psychiatric social workers and others carrying out mental health research have felt the need of using such techniques; for example, in studying phenomenology of mental disorder, relationship of social and cultural factors to aetiology of psychiatric illnesses, evaluation of psychotherapeutic intervention, etc., where the data does not easily respond to numerical and statistical techniques. Unfortunately, not many in the field of mental health research are familiar with these techniques.

A workshop was held in April 1995 with financial and administrative assistance from International Development Research Centre (IDRC), Canada for which some of the most renowned psychiatrists, sociologists and anthropologists from India and abroad were brought together to interact with some specially selected young mental health researchers from India and other SAARC countries. This includes the presentations made by the faculty members as well as the issues raised during discussions.

It was felt that what transpired during the workshop would be of value to other mental health researches across the world. National Institute of Advanced Studies, Bangalore, where this workshop was held has great pride in bringing out the proceedings of the workshop as a book.

The book has been edited by Prof. R. L. Kapur, who organised the workshop. Prof. Kapur is a Psychiatrist specially interested in Psychiatric Epidemiology and Cultural Psychiatry. He has during his long career been the Professor of Community Psychiatry at the National Institute of Mental Health and Neuro Sciences, Bangalore, and Deputy Director of the National Institute of Advanced Studies. His current research interests focus on higher states of mental health, and particularly on contribution of Indian spiritual traditions towards achieving positive mental health.

Qualitative Methods in Mental Health Research
Edited by Prof. R. L. Kapur

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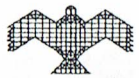
NATIONAL INSTITUTE OF ADVANCED STUDIES
Bangalore, India

QUALITATIVE METHODS IN MENTAL HEALTH RESEARCH

EDITED BY

R. L. KAPUR



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Preface

M.D./ Ph.D./ Post doctoral students in the mental health disciplines in India are exposed to a fair amount of training in survey methods and other quantitative research techniques. They have, however, little exposure to the field of quanlitative information: e.g. phenomenology of psychiatric disorder, relationship of familial and cultural factors to the aetiology and prognosis fo psychiatric illness, evaluation of psychotherapeutic techniques,etc. Young researchers today are not only unfamiliar with the standard methods used in such research, but are also largely ignorant of the newly emerging anthroplogical techniques which can enhance the quality of information obtained by these methods. Most importantly, even at times when qualitative data has been gathered reasonably well, the researcher, often does not know how to analyse it, causing loss of valuable information. It is possible that some centres in the country have teachers who could acquaint students with qualitative research methods or perhaps even inspire them to use such methods, but this is not common and most institutions do not have such expertise.

Against this background, a workshop on 'Qualitative Methods in Research' was conducted from 10th to 12th April, 1995 at the National Institute of Advanced Studies, Bangalore. The workshop was held primarily for reserachers from the mental health disciplines (psychiatry, clinical psychology and psychiatric social

work), but those in non-governmental organisations with mental health interests were also included. The workshop brought together participants from different parts of India as also from some of the SAARC countries. The composition of participants who attended the worshop was as follows: 26 from India, 2 from Bangladesh, 1 each from Nepal and Iran. We were lucky to have on our faculty panel specialists from both India and abroad with impeccable credentials and world wide reputation. The workshop included lectures, discussion and case studies as well as small group discussions on projects designed by the participants. It was hoped that by bringing together a faculty consisting of sociologists and anthropologists interested in mental health issues as well as mental health experts with special training or interest in qualitative research, one could offer a powerful resource which would encourage participants of the course to use these methods in their research work.

This volume is a collection of papers presented at the workshop. One unique feature of the workshop was that as much time was given for discussion as allotted for formal presentations. Questions, anwers and comments during the discussions which followed each presentation were of such high quality that they have been included in this volume either verbatim or paraphrased where necessary to ensure the right flow. In the last session, participants were encouraged to present research protocol of some projects using qualitative analysis which they themselves might like to pursue. Those projects were disucussed in small groups, moderated by one or two faculty members. Those, however, have not been included in this volume because the participants who made the presentations were keen on conducting the studies and it would not be appropriate to anticipate their research.

The workshop was financially sponsored by the International Development Research Centre (IDRC), Canada. Dr Anwar Islam, Health Sociologist, Senior Program Officer, Health Systems Program, Health Sciences Division of the IDRC, not only supported the enterprise but took active part in its planning. He was also a member of the faculty. I would first like to thank Dr Islam and the IDRC for their continued encouragement in planning and conducting the workshop. I also take this opportunity to thank the faculty for the workshop for making

presentations and for taking time off to write them up as papers. The participants also require to be thanked for being frank and open ensuring the discussion were of high quality. My appreciation is due to Ms Dhanu Nayak for helping to coordinate the workshop and for assisting in the editing work. I must also recognise Dr Vandana Goswami who typed out all the lectures and discussions using her bakcground in anthropology to polish the material. Dr Susmita Subramanyam assisted me in preparing the final draft and I am thankful to her. I would like to acknowledge Prof M N Srinivas, Dr R Raghuram, Prof Malavika Kapur and Dr Shekhar Seshadri for their help in planning the workshop. I am grateful to Dr Raja Ramanna, who was the Director of the Institute at the time for allowing this unique workshop to be held at the National Institute of Advance Studies, and to all my colleagues at the Institute who irrespective of their background and interests participated fully and ensured the success of the workshop.

R L KAPUR
Editor

Introduction

R L KAPUR

Mental Health Research: Addressing the Lacuna

Qualitative data gathering has always been a part of the methodological repertoire of the mental health field, but only as an adjunct or as a pilot phase before translating the data into numerical forms which can be statistically analyzed. While this qualitative paradigm has allowed us to identify ourselves with the natural sciences, we have never felt totally comfortable with this. There is always an experiential dimension to the information that we gather, where meanings do not merely reflect the world as it exists, but are produced or constructed by persons within cultural, social and historical relationships. These meanings get diluted or destroyed when information is converted into points on a numerical scale.

My own discomfort with the purely quantitative paradigm started in 1970, when, after rigorous training in epidemiological methods in Edinburgh, I undertook a study of mental disorder in rural India. One of the exercises I wanted to carry out was to determine the prevalence of severe mental disorder in the community, with the hope that this information would help me set up priorities in terms of services which could be offered. When I attempted to do this, I ran into problems. What is severity, and from whose view point? I had planned to examine degrees of distress experienced

by the patient, by his relatives, and by the community as a whole. But how does one go about measuring distress? Asking people to mark severity of distress on a 5 or 10 point scale seemed ridiculous to me. But I did not know any other way of doing it. So, though the work was completed, I was never satisfied with this aspect of my enquiry.

In 1983, I took up a project to understand the effects of yoga on mental states, and to this extent, my own mental state. I wrote down my experiences in detail for over a year, but after the year was over, I was back again in a state of perplexity. How could I translate this massive amount of data in a concise form, which can be communicated? How do I discover major patterns, and how do I choose between the multiplicity of interpretations that came to my mind? I encountered similar problems when I tried to analyse my psychotherapy records, or the variety of interactions that occur with the family members of patients. My knowledge of epidemiological methods, statistics and controlled experiments were of no help to me. While I was struggling with this dilemma, I found that anthropology had developed techniques which could help solve these problems. So, when my colleagues and I started working on a new project on the "alienation of Indian youth", where we carried out detailed interviews with young people who have taken part in militancy across the country, we consulted anthropologists like Prof. Srinivas and Prof. Veena Das.

The problem I faced is one encountered by everyone in my profession. Most of us, whether we are psychiatrists, clinical psychologists or social workers, are trained in the quantitative paradigm. This does not help us in doing the kind of research we often want to do and for this reason mental health researchers must become sensitised to qualitative methods of research.

What is Qualitative Research?

There are two major paradigms of inquiry, each operating on entirely differing assumptions about the best way to conduct an investigation. The logical-positivism paradigm relies on quantitative and experimental methods to test hypothetical-deductive generalisations; phenomenological inquiry on the other hand, attempts to understand human experience inductively and

holistically in context-specific settings by using qualitative and naturalistic approaches. McCracken (1988) points out four main differences in the two paradigms :

> ***A difference in the way each tradition treats its analytic categories*** In quantitative research isolating and defining categories as precisely as possible before beginning study and subsequently determining the precise relationship between them is paramount. In qualitative work, on the other hand, the investigator expects the nature and definition of analytic categories to change in course of work
>
> ***Precision vs capturing complexity*** A quantitative researcher has a precise focus and studies narrow categories while the qualitative researcher looks for patterns of interrelationship across many categories instead of sharp relationships among limited categories.
>
> ***Ambiguity in data reporting*** Questions asked which allow unambiguous and ready responses characterise quantitative data. When data sought is more flexible and difficult to elicit in precision, qualitative methods are required.
>
> ***Generalisability vs specificity*** Sample construction of a necessary size in order to generalise to a larger population is important to quantitative research, but not to qualitative work-"the purpose of the qualitative interview is not to discover how many, and what kinds of, people share a certain characteristic. It is to gain access to the cultural categories and assumptions according to which one culture construes the world. How many and what kinds of people hold these categories and assumptions is not, infact, the compelling issue. It is the categories and assumptions, not those who hold them, that matter. In other words, qualitative research does not survey the terrain, it mines it. It is, in other words, much more intensive than extensive in its objectives "(p.17).

While the two paradigms offer researchers competing advantages, the final choice of paradigm must eventually depend on the aims

of the study. Unfortunately, to this day the debate continues on which of the two paradigms is more efficient! These paradigms are not always water tight compartments either and as Patton (1990) suggests one can even adopt methodological mixes across the two paradigms- "there aren't just two paradigm-dictated choices. All kinds of variations, combinations,and adaptations are available for creative and practical situational responsiveness".

The perspectives in qualitative research draw from a variety of theoretical traditions.

Perspective	Disciplinary Roots	Central questions
Ethnography Phenomenology	Anthropology Philosophy	What is the culture of this group of people? What is the structure and essence of experience of this phenomenon for these people?
Heuristics	Humanistic Psychology	What is my experience of this phenomenon and the essential experience of others who also experience this phenomenon intensely?
Ethnomethodology	Sociology	How do people make sense of their everyday activities so as to behave in socially acceptable ways?
Symbolic interactionism	Social Psychology	What common set of symbols and understandings have emerged to give meaning to people's interactions?
Ecological psychology	Ecology-Psychology	How do individuals attempt to accomplish their goals through specific behaviours in specific environments?

Perspective	Disciplinary Roots	Central questions
Systems theory	Interdisciplinary	How and why does this system function as a whole?
Chaos theory; nonlinear dynamics	Theoretical physics, natural sciences	What is the underlying order, if any, of disorderly phenomenon?
Hermeneutics	Theology, philosophy, literary criticism	What are the conditions under which a human act took place or a product was produced that makes it possible to interpret its meanings?
Orientational, qualitative	Ideologies, political economy	How is x ideological perspective manifest in this phenomenon?

(table reproduced from Patton,1990)

All these perspectives, however, share common themes:

Naturalistic inquiry	Studying real world situations as they unfold naturally, non-manipulative, unobtrusive and non controlling; openness to whatever emerges-lack of predetermined constraints on outcomes
Inductive analysis	Immersion in the details and specifics of the data to discover important categories, dimensions, and interrelationships; begin by exploring genuinely open questions rather than testing theoretically derived (deductive) hypotheses
Holistic perspective	The whole phenomenon under study is understood as a complex system that is more than the sum of its parts; focus on complex interdependencies not meaningfully reduced to a few discrete variables and linear, cause-effect relationships
Qualitative data	Detailed, thick description; inquiry in-depth; direct quotations capturing people's personal perspectives and experiences

Personal contact and insight	The researcher has direct contact with and gets close to the people, situation, and phenomenon under study; researcher's personal experiences and insights are an important part of the inquiry and critical to understanding the phenomenon
Dynamic systems	Attention to process; assumes change is constant and ongoing whether the focus is on an individual or an entire culture
Unique case orientation	Assumes each case is special and unique; first level of inquiry is being true to, respecting and capturing the details of the individual cases being studied; cross-case analysis follows from and depends on the quality of individual case studies
Context sensitivity	Places findings in a social, historical and temporal context; dubious of the possibility or meaningfulness of generalisations across time and space
Empathic neutrality	Complete objectivity is impossible; pure subjectivity undermines credibility; the researcher's passion is understanding the world in all its complexity-not proving something, not advocating, not advancing personal agendas, but understanding; the researcher includes personal experiences and empathic insight as part of the relevant data, while taking a neutral non-judgmental stance toward whatever content may emerge
Design flexibility	Open to adapting inquiry as understanding deepens and /or situations change; avoids getting tied into rigid designs that eliminate responsiveness; pursues new paths of discovery as they emerge.

(table reproduced from Patton,1990)

There are three kinds of data in qualitative research - open ended interviews, direct observation and documents. Sampling techniques are usually purposive. Quite often the sample size may be small – "the validity, meaningfulness, and insights generated from qualitative inquiry have more to do with the information richness of the cases selected and the observational/ analytical capabilities of the researcher than with sample size" (Patton, 1990,p.185)

There are variety of methodological approaches in qualitative research, among others- ethnography (Agar,1986); case study method (Hamel,1993); focus groups (Morgan,1988) narrative analysis(Riessman,1993); heuristic research (Moustakas,1990); and the long interview (McCracken,1988).

Quality and Credibility of Qualitative Research

An important issue often raised is the credibility of qualitative research? How are reliability and validity established? Patton (1990) discusses three issues related to this: the use of specific techniques and methods designed to ensure validity and accuracy of findings; second credibility of the researcher; and third, the assumptions of qualitative-quantitative research and misconceptions about this.

Methods designed to ensure validity and accuracy of findings – There are several techniques to ensure accuracy of findings. First, active attempts must be made to test rival explanations and the importance of testing out alternative classification systems, themes and explanations during data analysis and document why the researcher favoured one explanation over another. Negative cases which run counter to trends must be examined carefully. The use of various methodological and theoretical mixes or 'triangulation' of several kinds can also be useful-method triangulation includes simultaneous use of quantitative and qualitative methods; data triangulation combines different forms of data within the same paradigm (for instance, observations, interview material as well as written documents); theory triangulation involves use of different theoretical perspectives in analysis; and finally different investigators can also be called in to examine the same issue. Another technique which is especially useful is to analyse data specifically within a context by limiting

conclusions to situations, time periods, persons and contexts related to the actual sample of the study.

Researcher credibility – The researcher can affect evaluation in several ways. First, from the reaction he or she elicits from the participants of the study. This must be examined in a discussion of methodology. Second, the personal predispositions the researcher carries into the study should be explored. Patton (1990) quotes Denzin, "all researchers take sides, or are partisans for one point of view or another. Value free interpretive research is impossible. This is the case because every researcher brings preconceptions and interpretations to the problem being studied. The term hermeneutical circle or situation refers to this basic fact of research. All scholars are caught in the circle of interpretation. They can never be free of the hermeneutical situation. This means that scholars must state beforehand their prior interpretations of the phenomenon being investigated. Unless these meanings and values are clarified their effects on subsequent interpretations remain clouded and often misunderstood". The researcher must, therefore, make special efforts to cultivate 'empathic neutrality'. It is especially important to accept and report one's own emotional reactions through the process of inquiry (Kleinman & Copp,1993). Another important aspect lending credibility to qualitative research is investigator competence. Qualitative research, like any method of systematic inquiry, requires preparation and rigour. For this, professional training is essential.

Paradigm debate and credibility –There are several misconceptions about the credibility of qualitative methods. This involves the larger paradigm debate between qualitative and quantitative approaches. One of the claims is that qualitative approaches are more subjective in comparison to quantitative methods. In fact, qualitative rigour has to do with quality of observation made by the investigator. Patton (1990) makes the point that the emphasis in qualitative methodology is on being factual about observations rather than being distant from the phenomenon being studied- "distance does not guarantee objectivity, it merely guarantees distance". What is required is to replace the necessity of being 'objective' with being fair, balanced and conscientious so that multiple perspectives and multiple realties are considered. It is important then to recognise a second plank of difference in

paradigms – the notion of truth is not one necessarily universal, but perhaps one of perspective in the qualitative approach- "qualitative data will tend to make the most sense to people who are comfortable with the idea of generating multiple perspectives rather than absolute truth. Tolerance for ambiguity seems to me to be associated with comfort in ealing with perspective rather than expecting certainty and truth" (Patton,1990). As a matter of fact, considering the inherent complexity in living, this view is vital if the subtleties of human existence are to be explored. A third claim is the greater ability to generalise results among quantitative methods. Defending the small sample size in qualitative methods, Cronbach (quoted in Patton,1990,) says, "Generalisations decay. At one time a conclusion describes the existing situation well, at a later time, it accounts for rather little variance and ultimately is valid only as history". Human behaviour is heavily mediated by context. Finally, prediction of causality has always been described as the strong point of quantitative inquiry, but weak in qualitative approaches. Qualitative researchers, however, say they are not looking for linear causality. Instead, being sensitive to interconnections and linkages across various categories of human experience are the priority. The aim in analysis is to present rich and detailed descriptions in an orderly manner so that interconnections in the data speak out on their own facilitating deeper understanding of phenomena.

The aim of this introductory paper is to briefly introduce qualitative methods as a tool for research and make a case for the importance of using them in mental health research. Chapters which follow build on the issues raised here besides others which arise from the experience of faculty members and the participants.

The Need for Qualitative Methods in Mental Health Research

R RAGURAM

As we approach the end of this century in which significant strides have been made in the field of mental health, the task that confronts the researcher is a daunting one : to weave psychological phenomena into a grand fabric of cause and effect, which simultaneously incorporates strands of local knowledge too. Can Qualitative methods provide an answer? My own introduction to this method of enquiry provides a convenient starting point.

It occurred quite by chance and involved one of my patients who had been in hospital for many years. He had a chronic schizophrenic illness and since his social supports were meagre, continued to stay in the ward for many years. One of the persistent symptoms that he had was auditory hallucinations. Our efforts to reduce their frequency through pharmacological and behavioural strategies were not very successful. One day, while I was having coffee in the hospital cafeteria, he came up and enquired whether I was trying to reduce the voices, when I replied in the affirmative, he responded "but doctor, they are the only friends I have ... what will I do without them?

His responses were an eye opener for me. For years, I had tried to assess patients using conventional psychiatric frameworks and evaluated their experiences through detailed psychiatric

interviews in verified clinical settings. Not surprisingly, what I discovered was psychopathology. I had to just step out my professional preoccupations momentarily to unravel an entirely new dimension of psychological distress. This encounter got me interested to examine how schizophrenic patients understand and respond to symptoms on their own. Since at that time I was still unaware of qualitative methods of recording and analysing experiences, I merely asked the patients to narrate in their own words, what the experiences meant to them and how they adjusted to them. After accumulating several such accounts, I was able to categorise various coping strategies which patients employed to handle their symptoms (Raguram 1993). In the process I also discovered that a psychiatric patient interprets his illness within his most basic frame of reference - his awareness of being an individual and against the background of his personal biography. In attempting to assess these interpretations, there is a risk of evaluating them in isolation through 'objective' measures. The process though useful "is like catching rain a bucket for later display. What you end up with is water, which is only a little like rain" (Whitehurst 1979). In addition, our understanding of these phenomena also greatly hampered if we continue to map them onto a conventional (western) psychiatric framework, which emphasises a sharp distinction between inner-subjective and outer-objective states of experiences. The history of research methods in psychiatry is a catalogue of attempts by the professionals to address this dilemma. While quantitative methods operate on a premise that there is an insurmountable gulf between objects and our representations of them, qualitative methods work within the problem rather than against it. (Bannister et al 1994). Qualitative methods have emerged only recently in the mental health research as an alternative approach. Much of research in psychiatry is underpinned by a positivist conception of science. Consequently, there is an emphasis on uncovering the relationship between 'causes' and 'effects' and a preoccupation to define the dependent and independent variables. In order to excercise control over the host of variables that influence the research activity, professionals attempt to restrict their number to the barest minimum, targetting only those variables that they presume are relevant to a particular aim in a study. In the process of excluding the 'confounding' variables and in their zeal to measure and quantify behaviour reliably, researchers only succeed in making the research setting as unlike the world as possible

(Mixon 1974). Professionals do not realise that the task of constructing a research activity that is so thoroughly controlled as to prevent the outside world influencing it, is an impossible one. Since the research setting is itself embedded in a social world, controlled observational methods need to be complemented by the use of naturalistic observational techniques. Qualitative methods offer a viable alternative in this situation, since they emphasise an interpretive naturalistic approach to the subjects under investigation.

Qualitative research begins by accepting that there are a range of different ways to make sense of the world and strive to understand phenomena in terms of meanings that people bring to them. It is important to remember that when researchers start believing that they are most objective only when they maintain a distance between themselves and the people they study, they actually succeed in producing an objective account of their own professional orientations and personal biases. Qualitative methods offer a different way of working through the problem by suggesting that an objective account of the phenomenon in question is possible only through an exploration of the ways in which researcher's own subjectivity influences and structures it in the first place. While positivist approaches assume that a diminution of subjective elements in research will lead to an increase in the objectivity of observation, qualitative researchers define objectivity and subjectivity only in relation to each other. Subjectivity is a resource and not a problem in qualitative research. As the researcher also forms a part of the social world he is studying, he cannot avoid having an effect on the phenomena being studied. Hammerstey and Atkinson (1983) observe incisively that "rather than engaging in futile attempts to eliminate the effects of the researcher, we should set about understanding them effectively". At the same time in the pursuit to go beyond the `facts' to uncover personal perceptions and meanings of people, the qualitative researcher must take care not to impose a priori categories and concepts derived from his own professional knowledge onto the process of observation and data collection .... reasoning in qualitative work is inductive, moving from observation to hypothesis testing and not vice-versa.

The distinctions between qualitative and quantitative methods are frequently presented as being clear cut, but the contrasts are

more apparent than real. It would be more appropriate to consider their relationship as being complementary to each other. There are three different ways in which such a relationship can occur. Firstly, qualitative work can be conducted as an essential prelimianry to quantitative research. The second way qualitative methods can be used is to supplement quantitative work and lastly qualitative research can be employed to examine phenomenon in areas that are not amendable to quantitative explorations.

A creative dialogue between the qualitative and quantitative approaches is likely to be of immense value particularly in the field of psychiatry. Influenced by findings from basic neurosciences, the profession has shifted from a model of psychiatric disorders based on psychosocial processes, to one based on medical maladaptive diseases - this new biomedical orientation in psychiatry has led to the vigorous pursuit of diagnostic systems that have greater reliability and the development of purpotedly specific pharmacological treatments. Years ago Jaspers (1963) cautioned that, "our particular scientific contribution is not to imitate neurology and construct a system with constant cross-reference to the brain ... but to develop a standpoint from which to investigate the various problems, concepts and relationships within the framework of the psychopathological phenomena themselves". His advice seems to have gone largely unheeded. The reluctance of mental health professionals to expore the complexity of psychopatheological experience utilising innovative methods of enquiry has contributed to the advent of naive biologism (Halasz, 1994). The phenomena of mental illness are however beyond the explanatory power of any single approach. As Jaspers (1963) outlined further, if we are to have to a conception of our patients in their entirety, we need to adopt a three fold view; first, to recognise that presentation of the illness, which he terms as the science of nosology; second, to understand the individual on the background of his physical - psychic - cultural experiences, which calls the science of eidology; and lastly to view the individual in his or her life-span, the science of biographics. A truly holistic approach to the study of human experiences in health and sickness, should employ methods which integrate these three divergent perspectives. Since qualitative methods strive to pay attention to the experiences of the individual in a reflexive fashion, they can

help the professionals to reformulate and reframe the research questions to comprehensively study the inner world of the patients. For instance, employing ethnographic methods Corin (1990) carefully reconstructed the lifeworlds of Schizophrenic patients living the community and demonstrated that their 'withdrawal' can be considered as a correlate and an icon of the isolation of individuals in western societies. Kleinman (1988), vividly illustrated the manner in which assumptions of western psychiatry are revealed in the clinical narratives of patients. Weiss, Raguram and Channabasavanna (1995), operationalised the explanatory model paradigm of Kleinman by constructing a questionnaire - EMIC - to study the illness experiences of depression utilising both qualitative and quantitative methods. Though qualitative methods are being increasingly employed in mental health research, it must be admitted that they are yet to gain wider acceptance from the professionals. They have become narrowly associated with research approaches emphasising unstructured methods of obtaining data and are counterprosed to those methods which rest upon quantification, methods that have determined the shape of the discipline so far. We need not wedge a deep divide between qualitative and quantitative methods as observtion and measurement at all levels incorporate both there components. Qualitative and quantitative approaches form the Yin and Yang of research methodology! Combined together, they offer a powerful strategy to explore the experiential world of people in distress.

Discussion Session

MODERATOR: R L KAPUR

❍ *I have three questions and one comment. Firstly, do qualitative and quantitative data merge somewhere? Secondly, do you have any words of caution regarding any assessment errors that might creep in? Thirdly, don't you think that when we deal with human beings, intervening variables such as emotions and our own subjective biases come in? Also, there are complexities of terminology in social sciences: double alienation, disjunctured dilemmas, binary oppositions, analytic inductions, hermeneutics, semiotics, positivist epistemology, hypothetico-inductive methods on teleological purposes, and heuristic assumptions. If somebody could simplify such terms in the next two days it will help us to do qualitative research better.*

The linkage between qualitative and quantitative data can occur for limited purpose. Qualitative data can, to some extent, be reduced and made manageable through quantitative methods of data reduction. We attempted that in trying to understand explanatory models of depression. We identified a group of patients who had guilt as a major component of their depression. 'Guilt' is a professional inference that is drawn by experts. But how do people actually perceive and report guilt? We assessed how people expressed their guilt. To do that we went back into the texts of their reported statements and prepared a catalogue of people's own descriptions. We linked up a set of statements which were quite insightful about people's own perception of, and response to, guilt.

As for assessment errors like Type 1 and Type 2 errors, we have to come to terms with these in any kind of statistical manipulations that we do. The kind of errors which you point out are also linked to your comment that qualitative research could be biased. It is very difficult to be unbiased and uninvolved in qualitative research. Your scope or domain will increase once you do qualitative research and you may find yourself as an instrument of change, even with regard to the social factors that impinge on the individual. That opens us up to the larger agenda of what our professional role could be. We have been talking about the linkages between

psychiatry and the social sciences. Once psychiatry expands its horizons to the social sciences, the conventional kind of roles that we have become accustomed to may not be feasible. To take an example, for a depressive person, will our role be to merely relieve his depression, or relieve the kind of social factors that cause the depression?

Regarding your comment on jargon, we all face this problem. The problem is not of jargon, but of language: different groups of professionals talk different languages. When we view it as jargon, we feel it is something incomprehensible. One of the purposes of this workshop is to familiarise ourselves with different expressions and language, and I hope that this jargon will become understandable as the days proceed.

❍ *I will take you back to the beginning of your presentation when you began talking about how your personal interest evolved in the qualitative field. In my brief interaction with you, I have often observed you asking patients for their narrative accounts, or their stories, or the poetry that they have been writing. Could you reflect upon how you have actually been able to contextualise and make more meaning out of these different narratives acquired from them?*

A particular patient is still fresh in your memory. He was a creative writer with a schizophrenic illness, and your own understanding of the problem became enriched once you started examining his literary creations. I cannot really say whether understanding will be more or less meaningful, but it will become a little more comprehensive in terms of incorporating other aspects of his life which we don't conventionally really consider in a clinical encounter. Whether it will become more meaningful for the patient or the therapist must be measured in terms of what will be the practical outcome of this enhanced degree of understanding.

❍ *The question that I have is with respect to the point that you made about having to be as unobtrusive as possible when we interact with our clients. Each one of us has a script in our life, a story, just as our clients have. You have mentioned that this intrudes into our observations and I was a little concerned whether it may also intrude at the level of interaction. My script elicits a certain*

interaction from a person; so what I will be eliciting from that person may be a little different from someone else. It is said that we lose order in order to find order. But I think we, as mental health specialists, first need to be as stable as we can be so that when we try to lose ourselves in what the patient is saying, and come back to ourselves to put the necessary structure from the apparent chaos, we have that inner stability. Otherwise I'm sure we would be rather schizophrenic in our inferences.

I don't think we can run away from scripts, whether they are personal scripts or professional scripts. The very essence of our work in mental health fields tells us that there are multiple scripts. The very fact that human behavior can be understood by a psychiatrist from one method of approach or one framework, and in a totally different perspective by a clinical psychologist or a social worker tells us that scripts exist in the minds of people, and that scripts are influenced by training. So I don't think that we can erase scripts and go out in the world and find something like a clean slate.

Answering your second question about order and disorder, I will suggest that in order to confront disorder, you yourselves must be disordered! If the framework is heavily biased in terms of ordering what you see, you lose the essence of what you see. That which does not fit order, which in fact may be a most insightful moment in our learning, often escapes us.

MODERATOR: It seems to me that we are still trying to look for some kind of a sterilised, pure knowledge in these kinds of interactions. What does it really matter if you get different descriptions of the same event? Could we not combine all these different ways of looking at the same thing to obtain a much richer description of the phenomenon? I would like Dr. Raguram's comment on that.

This, in fact, is an elaboration of what I said earlier, that scripts do differ, and different scripts give us a more comprehensive idea of what we study. But what is the descriptive power of a particular method or approach? If you are posing the question that when there are conflicting ways of viewing things, which one is more powerful than the other, then I would say that is the purpose of the entire workshop!

MODERATOR : Yes. I was responding to the great concern I am picking up about introducing biases. But we introduce biases even in the most objective kind of research. Because, to be objective, we have to restrict information. And the very fact that we are seeking some kind of a restriction in information is itself a bias. Let us not try to seek an 'unbiased view', but instead use the variety of biases together to enrich the whole interaction.

Then I think that the interviewer has to spell out his biases in his report - that I am an introvert, or I am high on my mood today and so on.

Even if it is not spelt out, it becomes quite obvious.

MODERATOR: To my mind, it does not matter.

If we give too much importance to subjective experience, we have to limit it somewhere as far as reality testing is concerned. How do you evaluate this in qualitative research?

MODERATOR: Again, you are probably looking for some kind of a reality beyond what appears in the interaction. The question which we are trying to pose is, can we have a reality which is not context-related? Is there any such reality? In fact, we have physicists here who now agree that there is no reality which is not context-specific. So why are we so worried about that? This question is likely to appear again and this is the purpose of the workshop. We are all set into a particular mode of thinking, and this new approach which we are discussing is going to make us very anxious, and I do not mind that.

- *When a person comes to us with distress, he is keen on symptom alleviation. Do we have a right to go into his scripts? To go into the scripts of a human being, you need to be a poet. Are all psychiatrists poets? What is the mechanism by which a psychiatrist would become a poet? There is a flurry of thinking after your talk, and I am really uncomfortable.*

When we say that the patient needs symptom alleviation it is the professional who sets that agenda. What I am saying is,

let the agenda be set by the needs of the patient. And once you make the patient a participant in the process of distress alleviation, you will necessarily have to go into his own frames of reference. Qualitative methods will give you a kind of introduction and insight into these frames of reference to some extent.

I think it is important to recognise the distinction between clinical work, qualitative methods, and qualitative research. It is a matter of personal choice to what extent a professional would wish to use the spirit of qualitative methods in clinical work. There are some practitioners who are quite comfortable in prescribing: they may address one or two family issues and leave it at that; there are others who wish to go beyond that.

- *My question is with mental health professional using (the methods of) the social sciences. Social sciences is a very broad term, covering many disciplines. Are we not broadening our scope much more than earlier? How much of these do we use in our routine work in research and how do we interpret all this information that we collect from the various disciplines?*

- *There is an array of emotions about what has been said. I think this process (of using qualitative methods) should begin right at the level of training. As students, we find a lot of importance given to quantitative methods. I want to know what teachers like Prof. Raguram are doing about it: are they disseminating this kind of observational technique right at the beginning, or do we have a seminar like this much later into our career, when we already have a set mind, which then makes us uncomfortable? Similarly, with reference to academic papers being accepted for publication, very often it is the papers with lots of statistics that get readily accepted; not much importance is given to qualitative descriptions. So we are in a way forced to adopt quantitative techniques.*

Both questions are somewhat related. As you are aware, the integration of social sciences with mental health or mainstream psychiatry, though conceptually very attractive, practically occurs very little extent, even in institutionalised settings. I feel this has more to do with problems of integration within professionals rather than clients, who can incorporate different

kinds of frameworks with much ease. So if an integration has to happen in practice, it has to happen within the professional first, and it has to be manifest in his clinical work. As regards whether I incorporate it in training, you will probably get an unbiased answer by asking my students, many of whom are among the participants today.

- *Either the whole can never be the sum of the parts, or we have not understood all the parts and the interactions between the parts. Which do you think is more amenable in your context?*

A quick response to the very provocative question is, I hope that in psychiatry the whole can never be expressed as just a sum of its parts.

- *The specific focus here is towards an understanding of qualitative methods in research, though one could apply it in a clinical set up as well. We are trying to understand the kind of research paradigm to choose and how to develop that, the kind of skills and expertise which are needed to do qualitative research, and so on. However, if one were to look at qualitative methods as an approach to clinical practice, it is a totally different issue. I do not think that can really be taken up in a workshop like this because it could become contentious.*

MODERATOR: Thanks for reminding us of this, though at times there will be a cross-over. It is very difficult to separate research activities from clinical work.

In terms of the comment on the distinction between qualitative research, clinical work and qualitative methods, as has been pointed out, qualitative methods are in fact a part of our clinical work. The focus of this workshop is primarily on qualitative methods as applied to research.

The second comment I want to make is to point out the movement from participant-observation to extended case studies to survey methodology. Survey information which is quantitatively depicted can accrue out of qualitative research and I can make available some research findings which are documented, which have used this methodology.

Keeping in mind one of the previous questions, I have a fear in mind that if the quantitative method can make the Kappa a tyrant, then can the qualitative method make the observer, with a script behind him another tyrant?

Yes, he can. There is nothing called a neutral method. It all depends on the person who wields the method in a particular way.

❍ *I am especially sympathetic to qualitative methods, more so because my background is pure science. Quantitative methods are seen as more intrusive than qualitative methods. I would like to suggest that the qualitative method too is an intrusive process, though the intrusion is perhaps much less than the quantitative method. And once you allow that it is intrusive, then it becomes a question of choosing where to stop. This implies that the character of qualitative analysis will be similar to that of quantitative analysis in one sense. This leads me to the conclusion that the real, "true" qualitative method is a mentally disturbed patient talking to another mentally disturbed patient. The psychiatrist as a mediator here is a very intrusive process and qualitative methods have a problematic structure in this particular context.*

I am provoked by the comment that qualitative methods are themselves intrusive. It is perhaps as intrusive as injections which we take. I do not think that we can plan an intervention without getting into the territory of another person; the moment you encounter the other person's suffering, your job as a clinician is to minimise the discomfort. We cannot have a totally non-intrusive method of studying another person.

❍ *In all systems we look for an ideal, though the real approach may be far away from it. In mental health it is essential to establish a rapport between a doctor and the patient. There must be instances in recent history where a doctor, through very patient effort, has been able to totally help a patient resolve his/her problem. To what extent has the doctor's own experience in unraveling the problems of the patient been recorded in detail for others to be guided?*

This is more in the nature of a general comment which we may take up as the seminar proceeds.

❍ *Although I appreciate the idea that disease is a story, a narrative, it is too close to what we have seen recently in anthropology, in the definition of culture as a "text". After two decades of a limited definition of culture, we have young anthropologists going to the field and producing ethnographies in which they essentially talk about their inner experiences and feelings in the encounter with other people. The question of reliability of accounts becomes important as soon as we raise the definition of disease as more than just a series of symptoms. We end up with the very difficult problem of how to articulate the contrasts of proximity and distance, co- partnership and being different. We forget that we stand on the boundaries and that we have, in one single enterprise, to put together observations, although facts are always constructed. This is the real problem: how is it feasible to stand on those boundaries? The problem is not the fact that participation speaks with different voices; this is true, because reality is multiple, and we all have different scripts. How do we combine and articulate between those two series of contrasts that we have established? If we can successfully do that and combine science with poetry, that will be the real human science we are looking for.*

How boundaries are created within encounters - it is a very important and ticklish issue: whether boundaries are a product of collaborative effort and are generated out of the needs of the clinician as well as the client, or whether boundaries are defined by the need and agenda of the clinicians. The latter is often the norm in clinical practice. But once the boundaries are delineated conjointly, then perhaps the kind of issues which you raise may not really come to the fore.

❍ *We have to really understand what is clinical and what is research; it is very important to make a distinction between the two. We should also understand that we are not setting up qualitative and quantitative methods as competing methods; they must complement each other. There is no research which is exclusively either qualitative or quantitative. Even in qualitative research, we have to put an order at the end so that we can generalise and produce something that can be replicable.*

❍ *Further, when we are talking about a clinician, the only thing we can ask is whether a clinician is culturally sensitive, or sensitive to gender issues. I have noticed here that we are always referring* to a

psychiatrist and a patient as "he" rather than as "he or she". In a clinical setting, it is important to see whether the clinician is sensitive to the cultural context of the patient, and whether there is a multi-disciplinary team along with the doctor, including perhaps a social scientist and a psychologist, to help understand the total context of the individual and then make a treatment decision. Apart from that, we have to understand the research context and not confuse the clinical setting with the research setting. We have to be careful not to oversimplify things from one to the other.

Regarding the distinction between clinical and research work, I also highlighted in my presentation that these two perspectives are complementary. If you want a clinician to be culturally sensitive, the qualitative method of approach is the best possible way by which to assure that sensitivity.

- *I want to put on the table two issues which we could take up. One follows from an earlier comment on the difficult relationship between the whole and the part. In my own research I tend to privilege the fragment rather than the whole, because in fact, the parts may not add up to the whole. We have seen this in research again and again that the macro does not match the micro. Even this vision of the whole as having a boundary is a very problematic one because it takes a very architectural model of what is a whole and what is a part. So let us introduce a degree of uncertainty as essential to the project rather than something which we are essentially going to resolve. If we have to have any other theory of truth which is not a correspondence theory of truth, you cannot have fact-value distinction, not because of any great moralistic reasons, but for very strong epistemological reasons. It is increasingly becoming clear that this distinction is very difficult to maintain. So I think we could introduce some uncertainty and work around it.*

- *The second thing I was going to say is that I was fascinated by the first image, but rather than assuming that the fish's vision and the goat's vision are going to come to a total vision of water, it is possible that the fragments actually relate to different moral worlds. And when and how these moral worlds will communicate is a very difficult issue.*

- *The last point I want to make is that, clearly here, one is assuming that the patient's narrative is available to the doctor. But it is important to remember that the doctor is part of the patient's narrative and his/her family narratives. One of the things we may be interested in is how psychiatry is experienced in everyday life. When a psychiatrist authorises a particular diagnosis, there is also a social knowledge which is generated and it has very interesting implications for family narratives. I just wanted to point out that for the psychiatrist, the truth has many ways of making its appearance.*

CONCLUDING REMARKS BY MODERATOR: Thank you. You have added a very important component to this discussion, and this will perhaps allow us not to feel too anxious when we do not end up with what may be called a 'complete information': perhaps we can't reach it. This question is going to come up many times during these days and we will take it up as we go along.

Qualitative Methods of Research in Cultural Psychiatry

ROLAND LITTLEWOOD

We can examine the scope of the term psychological illness by seeing it as representing a spectrum along which we find rather different reactions: at one end there are those patterns which we can understand as due to an underlying biological disease process (cerebral tumour, delirium tremens, 'symptomatic psychosis'); at the other end, patterns such as parasuicide or phobia where it is difficult to postulate a simple biological cause and where we prefer rather to examine personal experience and motivation within a particular cultural context.

Disciplines such as psychiatry (or clinical psychology) employ two quite distinct disciplinary paradigms which derive from rather different modes of thought. At the 'biological end' we understand matters naturalistically: through the study of cause and effect processes in a natural world which exists independently of our human awareness but of which we can obtain some knowledge. By contrast, at the 'cultural end' we invoke such characteristically human attributes as intention, memory, identification, imitation, action and deceit. The former is well-known to us as the approach of the natural sciences, the latter is that of the humanities including historical, literary and political studies. Whilst the approaches are complementary they cannot

be easily reconciled; we canot say, for instance, that a particular illness is X percent biologically caused, Y percent intended (Littlewood 1991). All human action takes place within a biological world, whilst conversely our knowledge of the natural world can itself only be obtained through our cultural procedures (Littlewood 1993).

This double approach is particularly difficult for psychiatrists, trained over many years as biomedical scientists who wish to approach cultural variation in a more rigorous way than by simply being empathetic with their patients' values. (Our obviously available non-biological alternative, psychotherapy, is regarded by many historians and social anthropologists as too tied to certain normative Western assumptions of individualism, self-scrutiny and self-achievement). Psychiatrists are continually vulnerable to criticisms from our medical colleagues that any non-biological approach is unscientific, untestable and lacking rigour; and thus, perhaps not surprisingly, we tend to reify what are just pragmatically useful categories of disease as if they were distinct entities 'out there' in nature.

If there is a single principle for this symposium it is that the qualitative study of psychiatric illness is neither vague nor sentimental, but that it can follow well-recognised procedures and provide comparative data which then can be independently compared and criticised by others. A perhaps necessary warning; qualitative approaches are not an easy alternative to 'hard' statistical data, and if one finds it hard to understand statistics one cannot necessarily expect to be thus an able student of more open-ended aproaches. Perhaps the opposite. The qualitative approach is not an opportunity for sloppy logic, poor judgement and one's personal prejudices; whilst our own perspective is an inescapable part of any qualitative study it is not one we can take as automatically valid for others. And this demands a certain degree of *reflexivity* - that is, seeing our own theoretical approaches not as an objective and universal truth but as themselves situated at a particular historical and cultural moment in time (Gaines 1992).

A common concern of doctors, particularly in non-Western countries, is that this sort of focus on a local culture becomes a justification of that culture in all its vagaries, opening the way to

communalism and casteism so that we end up with a very restricted idea of an 'Indian psychiatry' or even a 'Bengali psychiatry' or whatever, which cannot be related to any other system. And a major attraction of Western medicine - biomedicine as we now prefer to call it - is that by apparently avoiding cultural particularities and by focusing on the biological alone, what we learn seems somehow 'cleaner', more modern and universal, and about the world as it really is. Yet the immediate daily life and culture of the people we study are not themselves scientific and value free, whether in Europe or India.

II

Qualitative research is not just another name for social or cultural research in general (on terminology and a useful guide see WHO 1994). The collection of figures on population, household income, gender and class distribution, and so on is a generally necessary preliminary to qualitative studies. And attitudes, health-related beliefs and practices can be usefully assessed through questionnaires and population sampling. The argument for qualitative research is that these questionnaire procedures, whilst often valuable, generally restrict the research focus too much, taking complex practices and their context as simple measures which miss out the rich context of human action, and which ignore the problem of how our sampling procedures may themselves influence the data we obtain. In a currently rather overused word, qualitative research is holistic, seeking to obtain a thick description of human knowledge, experience and action in all their vagaries, and indeed prejudices.

Over the last twenty years, increasing numbers of psychiatrists and psychologists have tried to incorporate qualitative thinking into their academic work (Social workers have, I think, always used it but have had no body of theoretical knowledge to articulate it). A common feature of these newer approaches has been the influence of social anthropology - in particular - and also ideas coming from sociology, feminism, literary criticism, philosophy and the new historical studies of psychiatry. In America they have become known as 'the new cross-cultural psychiatry', in Britain simply as 'cultural psychiatry' or 'anthropology and psychiatry' (Kleinman 1987; Littlewood 1990). I will trace briefly some of the thinking behind this new approach,

looking in particular at two problems which always come up: the unit of analysis and the problem of human agency (that is, volition).

Let me start with the well-known instance of the distinction between disease and illness as used by social scientists. Both are everyday words in English yet we can use them in a more specialised sense. Disease is what we have been trained to study - the underlying pathological processes which we recognise as located in the natural world. Illness is the particular individual's response to and experience of disease, a compound of their distinctive personality, culture and social situation. Yet there are a number of problems with such a neat distinction. And the very idea of what constitutes a biologically defined disease fluctuates with particular cultural shifts: Is homosexuality still a disease? Is the sickle cell trait a disease if it protects against malaria?

If, as I have said, any illness episode can be understood both naturalistically and personalistically, how do we fit the two together? The usual solution is to take the naturalistic as primary, to use the disease model in which we place weight on biology. In psychiatry we use the distinction between form and content to divide up our data into the naturalistic and the personalistic. This is a common assumption in our approach to schizophrenia, but what is form, what content? In the first modern cross-cultural study, Kraepelin argued that the form was represented by the universal symptoms (those found in all societies), whilst Bleuler later suggested that the core of the illness was represented by its characteristic symptoms (those which differentiated it from other illnesses) (Littlewood 1990). In the absence of any clear invariate biological change consistently associated with certain symptoms, cross-cultural studies have often ended in various circular debates about the universality of the pattern. The emphasis on the 'core symptoms' of schizophrenia in the World Health Organisation's International Pilot Study of Schizophrenia has been argued to give a biased picture which ignores the wide differences of symptoms in the 'margins' (Kleinman 1987). Nor can we assume in advance of actual demonstration that these core symptoms are necessarily 'closer' to biology than the others. Symptoms may be less the pathology than the brain's (or mind's) adaptation to the disease.

Similar problems occur when we look across time rather than across culture. Traditional annual rituals in South Africa in which, at harvest time, women symbolically took on certain male roles for a limited period, disappeared under apartheid; the same women then started to develop 'neurotic' patterns of distress (Littlewood 1991). Do we than say that the rituals had somehow been prophylactic (as doctors would argue), or do we propose that both ritual and illness had some fundamentlly common features or even functions (as anthropologists might argue)? In other words, do we take a psychiatric (individualistic) focus or an anthropological (cultural) one? And recently in the West, the reverse has started happening as the feminist health movement has reframed certain female 'pathologies' such as pre-menstrual tension, agoraphobia, eating disorders and parasuicide into acts of 'resistance' against a male-dominated society (Littlewood 1995).

The second problem relates to individual agency or volition. This, too, is a clinical as well as a theoretical issue. We can identify a series of 'vulnerability factors' in parasuicide — such as age, gender, social position, immediate circumstances — but does not the individual, as our non-medical friends continue to ask us, still have to 'decide' to kill himself or herself? And similarly with eating disorders: don't we have to accept that the person somehow 'chooses'? And how far do we take this idea of volition? Is either a state of malevolent spirit possession or a depressive illness a choice? My guess is that most of us would say 'yes, sort of' in the case of possession, but 'no' for depression. Why? And if we allow for social context, what of the agency of the doctor or investigator? For patterns like parasuicide or eating disorders occur in a particular social context in which we ourselves as professionals are active participants, validating the pattern as a genuine disease beyond the individual's volition or providing a sympathetic response which must, in one way or another, influence their future actions. And thus, in this confusion over cause-and-effect versus free agency, psychiatrists increasingly have recourse to such pragmatic ideas as 'risk factors' which simply avoid any assumptions about causality or agency.

III

Such patterns take their meaning (and any adaptive function for the individual which we might identify) from local assumptions

about 'stress', 'pressure', 'intention', 'vulnerability to spirits' and the like. Psychiatric illnesses can only happen within a local context where it is shaped and responded to in various ways depending on popular ideas. It is only recently that cultural psychiatry has turned to look at lay attitudes, beliefs and recourse to treatment. And I want to consider these in the rest of my paper.

First we must avoid two likely errors:

i) Lay knowledge about illness is unscientific and therefore quite valueless.

ii) Lay knowledge about illness is just a rather simple attempt at a scientific classification (a nosology).

It will be evident that I do not accept the first point for I have argued that psychiatric illness is always dependent on local context and the responses of others. And these are closely related to their own understanding of the problem: such as whether the person has a disease or spirit, and whether the person is to be considered responsible.

The second problem, which is one of taxonomy, is a little more difficult. Surely if we are to be sympathetic to local beliefs we should take these as attempts to be empirically valid, just like our own systems of pschopathology? Yet, the scientific schemata we elaborate are theoretical and we do not use the full classification in immediate medical practice. And in just the same way, local classifications are intended for action. They are pragmatic and 'shallow' as we put it. That is, they are not attempting to give a complete picture of the world, all the time, in the same way. Let me take a simple example, that of a bird. Both a penguin and a *chidya* (sparrow) are birds, and we can find them along a simplified branching tree :

Living organism < Animal < Chordate < Craniate < Bird < Penguin, *Chidya*.

But do we generally think like this when we look at an actual bird? Putting it another way, which is most truly a bird, a penguin or a *chidya*? Well, draw 'a bird'. Now which of the two

does it most resemble? The *chidya*. At a popular or 'immediate' level, the *chidya* represents 'a bird' more exactly: it is what we Indians and Europeans might term a prototype of a bird, whilst a penguin seems to be some sort of half fish for it neither looks like nor behaves as 'a proper bird'. And, as I have noted, a practical categorisation is 'shallow' - about the differences between different sorts of birds, or perhaps comparing birds with bats, not with comparing birds with other chordates in general. For that is of interest to professional zoologists alone.

Similarly, in the case of 'mental illness': if we attempt to sketch out a folk classification after talking to our informants it will be pragmatic and 'shallow', concerned with immediate problems and facilitating their resolution.

IV

Let me develop this a little further (see Marsella and White 1982 for an excellent introduction). Let us start with the simplest sort of study, such as a questionnaire in which we decide to ask a sample of our local population what do they know about 'mental illness'? Can we then map local categories against our professional ones? What are the logical principles through which the local categories seem to be established? (Our professional ones are really rather mixed: aetiological, such as drug-induced reactions; functional, such as the dementias; or prognostic, such as the distinction between schizophrenia and other psychoses).

Ethnoscience: Are local ideas something like scientific ideas? Do they correspond each to the other? The most obvious way to proceed is to compile two lists. In one column the *etic* as it is called, that is the analytical and universal scientific term, our professional category of psychopathology. In the other the *emic* or local term. Do they fit together neatly in pairs? Well, there might seem some approximation between, say, 'schizophrenia' (etic) and *'pagol'* (emic); but they are not identical for, in Bengal for example, *pagol* may carry certain assumptions of positive value or even creativity as does the English emic 'madness' (Bhattacharyya 1986). Sometimes they do not match: in Trinidad in the West Indies, 'doltishness' includes both what we might term 'mental handicap' and 'dementia' as a single category (Littlewood 1993). In some cases, an etic category, say alcoholism,

may not have a local analogue at all (Littlewood 1993). Or else a highly specific local pattern of abnormal behaviour or experience may be subsumed into a far more general medical category (possession by Kali as simply hysteria). And thus, we might have to display our data as a series of overlapping conceptual circles or Venn diagrams.

Propositional Analysis: If local classifications are patchy and incomplete, we can attempt to fill in the gaps by asking questions about information which is not immediately volunteered. If *pagol* is described as a 'hot' illness, whilst *'dhat* syndrome' is argued to be caused in a particular instance by semen loss, we can then rate each illness on both propositions by asking: Is *pagol* caused by semen loss? Is *dhat* syndrome a hot illness? And thus we an construct a large matrix on which all illnesses and their attributes (whether volunteered or ascertained by direct questioning) are mapped. But, as with the ethnoscience approach, we may wonder whether the classification we derive really corresponds to the way people think or whether it is an artefact of our experimental method. Why should local knowledge of illness be structured in such lists or grids? And this problem becomes very evident if we attempt to use multidimensional analysis on our data to derive underlying 'clusters' of illnesses with common characteristics. It is technically easy but what is the nature of what we obtain? The way people really think?

Processual analysis: Even if these two procedures involve some preliminary open-ended questioning, they approach local categories as if they are fixed and shared in an abstract 'culture'. How people actually recognise and respond to psychiatric illness may however be rather different. It may be contextual and inconsistent. Psychiatric diagnoses in the clinic are often conflicting and challenged; it would be odd if folk categorisation was any more standardised. In Trinidad I found that on general questioning, my village informants gave identical and highly stigmatising views of 'madness' as a sort of violence, but when I asked them about particular instances, such as those involving their own relatives, or when I observed what actually happened when somebody in the village where I lived became mentally ill, they bent over backwards to say 'this is not really madness' and elaborated all sorts of ideas about 'pressure', 'stress', and 'half-way madness' which they preferred to use until a person

obviously remained psychotic even after medical treatment. So chronicity now entered the actual diagnosis but they had not previously mentioned the time course of the illness as significant. Nor did they all agree in particular cases. Categorisations are dependent on social events such as recourse to treatment, and we might prefer to examine local illness as a sort of 'decision tree' of all possible choices and responses. Somebody seems mentally ill; what has to happen in the course of treatment before you decide what sort of illness it is? Say we start with exorcism; if it is successful the person had probably been the victim of a spirit, if not they are probably mad. So you then take them to a doctor. They don't get better with medical treatment, so then .... And so on. In other words the local diagnosis is arrived at in stages through particular procedures which we need to identify. Illness categories are not abstracted entities for most people, held in their head, but rather plans for action.

Narrative accounts: So our attempts to produce tables and classifications for our informants have to be supplemented by something closer to a background story. Moving even further in a qualitative or open-ended direction, we can ask people simply to describe in their own terms episodes of illness which have happened to themselves or others, and we might have a number of fairly flexible prompt questions: "What happened then?", "Was that an illness?", "What did people think at that point?". Allowing our informants to put things their own way minimises our constructing their local knowledge according to our own assumptions, but – and this is a problem of the qualitative approach – it makes comparison across individuals and across societies difficult. We might argue that in a way our information has become more `valid', more true to experience, yet paradoxically less `reliable' for at different times our informants may give us rather different accounts of the same experiences.

Fieldwork: We are now getting closer to something resembling traditional anthropological fieldwork, in which, through participant observation, the researcher becomes for an extensive period a sort of honorary member of a local community, seeing similar events from different angles over time, and allowing their own responses to be part of the context in which they obtain their data (whilst trying to allow for the fact of their presence). There is no 'neutral' data: in our personalistic world, everything is

contextual, everything is biased. I regard intensive fieldwork as the ideal, but it is not easy for mental health professionals to find the time (and accept the temporary drop in income) to carry it out. I do not want to deprecate more limited and quantitative approaches but we must note their very real limitations and not claim that, say questionnaire data or even a focus group give us the complete picture.

V

I'm not proposing anything so very radical. The goals of qualitative research in psychiatry are not new: are psychiatric patterns universal? How do they occur in local cultural contexts? I imagine however that a short introduction like this will hardly reassure psychiatrists: what of bias? Surely the more we move towards open-ended description, the more the account we give resembles journalism or even fiction. Aren't we entering an area where anything goes, where we abandon any attempt at objectivity? Not surprisingly these are questions continually debated by social anthropologists themselves, and recently it has become more acceptable to think of qualitative descriptions as indeed something resembling a novel. And perhaps not such a bad thing either.

Ethnography: the qualitative account we write up about a particular community – can never be neutral. It is in recognising this that we bounce our data and experiences against the major models in our discipline such as :

- evolutionist : there are common stages of social and intellectual development which all societies pass through.
- Marxist : a particular evolutionist model which emphasises technology, power and social conflict.
- functionalist : all social institutions fit together to produce a functioning society, and the 'meaning' of any social institution is in its interaction with other institutions.
- ethnoscience: (see above) people everywhere construct a description and explanation of their world.

- feminist : societies are not seamless wholes but rather comprise constituent groups such as the two sexes whose ideas may be at variance with those the dominant sectors (the men) present to the outsider.

- interpretive and phenomenological: this is the attempt to simply give a rich and detailed account of others' subjective experiences in their own terms.

We cannot really provide a non-theoretical account. If we attempt to be atheoretical, it is likely that we are simply using, unexamined, the theories of the past.

But I must stop offering potted summaries and procedures which go against the very grain of qualitative research. If you wish to try it, you have a lot of reading to do: first, all the empirical knowledge available to you about the local community, everything from rainfall and agricultural patterns to the existing ethnographies on the community (if there are any); then the literature on the model you wish to apply, and some thoughts on its limitations. Every study however has its focus, and you will of course wish to look at particular aspects of the local community. What categories of mental illness do they have? What is their knowledge of the physical body? What ideas are there on volition and responsibility? How do these relate to actual events? What is it to be an individual in a particular society (Marsella *et al.* 1985)? How do people make sense of the suffering in their lives? Are certain people invalidated (stigmatised) because of their illness or actions? What sorts of systematised knowledge of illness do people have? Do local patterns of healing resemble other community rituals? (And indeed with the rituals of our biomedical treatment?) What cultural symbols and values do they manifest? What idioms can we use to interpret particular episodes of illness - are they a performance of normative roles, or are they a representation of social conflict, or something we can understand as a resistance, as theatre or social catharsis?

VI

I have not tried to neatly define 'qualitative research' but four of its major characteristics should now be evident :

i) It starts from the local standpoint of those people whose experiences and illnesses we try to understand.

ii) It is 'holistic' in integrating the current phenomena of interest with wider social and cultural issues - the material conditions of life, the knowledge systems of individuals, their relations of kinship, marriage, power and their rituals and symbolisations.

iii) It is flexible and open-ended. While we make initial choices as to our area of study, we might find that something quite different is actually more significant for our original aims. And we might simultaneously use different methods to understand our material.

iv) It is reflexive. We inevitably participate in the data we obtain.

And I think that of all professionals working in medically-related areas, it is those working in mental health - social workers, psychologists, psychiatrists - who will find these characteristics most congenial. For we already have to negotiate the ambiguities of providing objective accounts of our patients and their families, whilst at the same time doing justice to their own distinctive view of the world and their experiences. Naturalistic and personalistic : both are inevitable and necessary. And it is in the ambiguities of trying to articulate them both that lies the excitement of cultural psychiatry.

Discussion Session

MODERATOR: R L KAPUR

- **MODERATOR**: Prof. Littlewood has demonstrated how people from different cultures view illnesses differently; not only cultures in the traditional sense, but even people living in the same place, but coming from different backgrounds. The point he made very clearly is that you cannot be atheoretical. We need to have a theory to manage the information we are dealing with. To this I will just add that we cannot choose to be atheoretical even when we analyse biological processes like perception. We see what we see because of the theory that is there.

- *Is the new cross-cultural psychiatry somewhat similar to the comparative psychiatry that Prof. H.B.M. Murphy had written about? The second question is, I am glad you mentioned that we need to have a theoretical construct; we cannot be atheoretical. But how do we apply the knowledge we get through our theoretical frameworks in terms of future interventions? For example, in China, early dementia is viewed as childishness. Do I, as a psychiatrist change them or do I change myself?*

  Comparative psychiatry was a term coined in the beginning of the century. There are a number of words for the area: trans-cultural psychiatry, cross-cultural psychiatry, cultural psychiatry, and so on; they are not exactly the same. The McGill group who started the journal called 'Trans-cultural Psychiatry' are a lot more psychoanalytical and less anthropological than, for example, Kleinman and me. They place a great emphasis on individual development. But there certainly are great differences even today between Kleinman and me: Kleinman places a lot of emphasis on the individual and individual suffering, I place more emphasis on social processes. This is probably because American anthropology is called cultural anthropology, while in Europe it is social anthropology. I find myself much closer to French theorists and farther away from American. But there is a certain continuity. Murphy's book as the best single compendium of comparative psychiatry up to that date is probably more medical than we have been. He has a chapter on schizophrenia, one on depression, and so on. We might,

these days, start off with a chapter on idioms of distress such as what are a culture's particular ways of expressing distress; one on the self; and so on. Therefore if we understand something as a biological disease, the more it makes sense to see how treatments differ across societies. But it is nonsense to talk about hysteria, possession states, children's refusal to go to school, and so on, cross culturally, because these are so culturally embedded.

I will have to bounce your second question back to you and say that you obviously do not like what the Chinese do. You do not think it ought to be called childishness, so you must favor another idiom, which may be a biomedical one; for example, Alzheimer's disease, or something like that. I have got to ask you what you feel it should be before I can answer your question.

- *I want to help people by intervening. I cannot just explain away an illness model that is in my mind as childishness or a part of senility.*

  We get into this problem of making value judgments and the way to get out of it is to find out what people themselves want. Is psychotherapy the answer? To some extent, the amount of psychotherapy, particularly the "human potential" sort in America, is actually causing the problem it seeks to relieve. Large numbers of Americans are going into therapy to discover their "true" self. I would argue that the more you look for yourself, the more elusive it is.

- *My question is related to the fact that I need to define a group before I start studying them. Suppose I am studying events that are associated with post-partum psychiatric disorders and I define my group as, say 10 or 15 women who come with this particular problem. Then, how do I go about conducting my research?*

  If you want to do it really well, you will live in that community and explore things like women's roles, understandings of women's bodies, ideas on sexuality, marital relations, menstruation, menopause, significance of childbirth, any rituals that went with it, people's own understandings of what the foetus (ethno-embryology) is composed of, whether

it is alive, is there selective abortion of female foetuses, and other such things. It is no good saying I am already a member of that community; none of you are really a member of that community any more. You have forgotten the tacit meanings of everyday life of the communities from which you came and that is inevitable. If you have a body of relevant data then you have a prior knowledge based on which you can target questions and understand experiences better.

But the chance of you being able to do that is negligible because it will involve giving up your job, and I am not saying that you should all go off and become anthropologists. One way is to have an anthropologist on hand who has actually looked at those things, and with whom you could talk to and check your questions. It is also possible to get enough information just by talking to patients at the clinic. A good approach is a narrative approach, where you not only look at people's experiences at the time of the event and afterwards, but their entire history. You will have to explore meanings of words with them and with other people because they may use various words, local or others, to talk about experience. You have to push out from a formal psychiatric treatment and interview towards the area of ethnographic fieldwork.

If you cannot do extensive fieldwork, one thing you can do is use focus groups. You can go into the community, assemble a group of people you think is relevant, and direct the conversation into a particular issue. In your study of post-partum psychiatric disorders, I would suggest you take a group of women who have not experienced post-partum depression, then a group of men, and maybe even a mixed group and see what they think about it. Not doing it will not make what you do worthless, but doing it will give you a more complete picture.

In working with mental health we have to cope with ambiguity. We never get definite and final answers, because people have free will. Qualitative anthropological methods are the same: we do not have certainty. Another observer might come up with similar or different ideas. If there are

differences, that too is not that big a problem. We can take each other's biographies and see what the cause of that might be.

- *You say that we should start with a theory. What kind of theory should it be*
  Supposing I want to study the dhat syndrome which is very prevalent in our culture - should I have an allopathic theory or should I start with an indigenous theory? In case I have a theory of my own, then there is a bias. How do I overcome that?

- *You tell us to take the local classification of diseases from patients and other people. But each person's conceptualisation may differ. So how do we manage this, and what do we do with all this information?*

- *What is the primary aim of qualitative research methods? It has been said that it is not to be nice to patients. I was of the impression that by doing qualitative studies we are understanding the cultural meanings of an illness, and the basic aim of that is culturally sensitive health care. So ultimately, it is being nice to patients, in their own terms.*

- *When one goes about studying culture, who is the best person equipped to look at the culture: someone who is a part of the culture, or someone from outside?*

- *Whose need is it to do cross cultural psychiatry: the psychiatrist's need or the culture's need? It is only when a culture is in transition that it needs Westernised ways of intervention, otherwise it is happy with its own ways of treatment.*

- *Although one appreciates the need for diversity of information, I am reminded of a lecture by an Indian psychiatrist who had said that psychiatrists who are trying to understand patients should be from the same language group, locality, culture and religion. If we are not going to be able to transcend these barriers in any case, what is the point in studying culture?*

- They are all the same question. To the first one, whether to use biomedical or local point of view to study the *dhat* syndrome, you can start from both, but given that it is

primarily a local category, the first thing we do is to explore the local meanings fully. The mistake our psychiatric forebearers made was to take the local category, see that the patient was anxious, and conclude that *dhat* syndrome was anxiety. Those two things do not fit together. Before we look at whether we can understand *dhat* syndrome biomedically, we need to look at the local variants and the ways in which it is explained. We also have to look at the history of the syndrome, which has many curious similarities to certain Western syndromes; maybe this syndrome is not as local as we think it is after all. We cannot assume that cultural words are distinct entities out there. We have to bounce back between the two and get increasingly sophisticated ideas by doing so.

What is the point of all this? Well, it is up to you what you do with it. You can use it to make biomedical care more appropriate, or to offer them two rather different alternatives. These are techniques that you can use to do different things. I personally do not learn about local values so that I can subvert them and bash people with biomedicine because I think that is the best thing. For me, the illness experience of patients is theoretically interesting, apart from my wanting to help them.

The issue of being nice: Qualitative methods are not just about zapping people with what we or they think is better health care so it is not just to do with being nice. There are good biomedical reasons for qualitative research. Our biomedical categories are personalistically constituted and themselves partly folk categories. They are not entities out there in the natural world. Of course it is also important to offer something which is decent, humane and valid for our patients.

So what is the need for cross-cultural psychiatry, if a culture is happy with biomedicine? That is a slightly stereotyped view. There is no culture that is unchanging; cultures are re-formed in each generation. Each individual has a choice and they are not passive victims of their culture or of their past; they remake and invent different traditions. We are continually re-interpreting the past.

Does the psychiatrist have to be of the same culture as the patient? My general view would be that if you are doing theoretical work, it is an advantage not to be of the same society.

CONCLUDING REMARKS BY MODERATOR: The tension between the need to know and the need to heal will keep emerging again and again. Many people here are healers and they feel that they want to know only as much as they need to know (in order) to heal. They are concerned about why they need to know more than that. Will their ability to improve heal with this knowledge? This question needs to be addressed by the researchers who are going to teach us during these three days.

Contemporary Methods in Narrative Analysis

VEENA DAS

Narrative analysis provides a powerful technique of analysis in many disciplines. The orientation of the analysis, however, differs not only across disciplines but also with the methodological assumptions underlying narrative analysis. A historian may, for instance, be engaged in the task of recovering social memory; a structural anthropologist may wish to look at the logical models underlying myths; a phenomenological orientation may be directed towards the understanding of experience. It is not my intention in this paper to provide a grand synthesis of the literature (which in any case would be hardly possible within the confines of a paper), or to resolve the issues that have led to major controversies on the problems of the subject in narrative renderings. Instead, I simply want to present some simple ideas on how narrative analysis may be used as a qualitative method in addressing the problem of human subjectivity and human suffering. This may interest clinical psychiatrists both for purposes of research and clinical engagement.

The issues that arise in the analysis of narratives may be divided into the following major themes:

1. The relation between narrative and experience at the conceptual level.

2. Techniques for collecting individual narratives.

3. Methodological issues: Collective genres and individual narratives.

Narrative and Experience: Is experience structured like a narrative? The philosopher MacIntyre (1981) has argued that human life has a narrative continuity. The sociological tendency to split the person into the several roles that he or she is said to play in society pays insufficient attention to the fact that we are constantly engaged in creating narrative coherence for our lives. From this point of view, the self may be compared to a narrative and to have a self may be seen in terms of the capacity to tell a story about oneself. We lead storied lives but these stories are not completely of our own making. In MacIntyre's theory of storied lives, as in many others, the processual nature of the self is emphasised. The self, in order to be crafted, must be oriented to 'a sense of the good' or to the 'unity of virtue' - which is visible only when the shape of one's life can be constructed as a whole. For MacIntyre, the narrative concept of self-hood requires two conditions to be fulfilled. These are: (i) a description of one's experience as a flow in time from one's birth to death and (ii) a description of the person as a part of the society into which he is born or lives in – here the narrative will be embedded in the narratives of significant others.

The temporal nature of experience and the flow of narratives present a correlation that is not merely accidental. As Paul Ricouer (1984) has stated, ".... time becomes human to the extent that it is articulated through a narrative mode, and narrative attains its full meaning when it becomes a condition of temporal existence". Thus the activity of story telling humanises time in that rather than being experienced as simply an objective mode of ordering the world, time emerges as a result of the activity of the subject. It is not that events are available to us only through story telling, but that this activity gives a man or a woman a way of announcing his or her agency. Thus, one can make a distinction between the order of events and the order of telling. While we easily accept the idea that this is what the art of story telling is all about, we do not often see that it is not only the novelist but also the most ordinary of persons who exercises this freedom and thus, at least partially makes his or her own world. From an

objectivist point of view, it may be obvious that one must be born before dies. From the subject's position, however, the freedom given by the narrative mode to organise a story through different temporalities (e.g. the death of a loved one may seem to be much nearer to one's concern than the fact of one's birth) is an important guarantee of the control that one may exercise over one's inner life through the command over memory and its narration. I shall come to the methodological implications of this a little later.

MacIntyre rightly states that one person's narrative is often enmeshed in the narratives of the significant others. The task of constructing meaningful totalities out of scattered events is done sometimes in collaboration and sometimes contesting with other people who form one's significant others. The point about keeping the enmeshed character of stories in mind is not to put together the different versions in order to arrive at a 'correct' version through an application of rules of evidence, as in a court of law, but to see how individual lives relate to other lives. In the context of the experience of illness and the generation of what Kleinman calls 'illness narratives', a single person's narration of his or her symptoms can touch on a whole host of tragic family dramas, disappointments in the work place, history as a series of traumatic events, and other such phantasmic figures that may be experienced as finally located in one's crumbling and dysfunctional body (Kleinman, 1988a).

The presence of the social is not posited exclusively at the level of groups to which an individual belongs and which exist *outside* of his or her body. Through the act of narration this 'outside' is mapped on the surface and in the depth of individual bodies. A very good example of this mapping may be found in the work of Mariella Pandolfi (1990) on the illness experiences of peasant women in Italy. She shows both, (a) how a maternal and a paternal genealogy is experienced as taking form in the disorders of the body; and (b) how the external historical events of war, earthquakes, and emigration are renarrated in personal histories so that there is a continuum between catastrophe outside and bodily catastrophe. My own work on individual biography which was conducted as part of an interdisciplinary team led by Ravi Kapur, shows that the placing of one biography in relation to other related biographies of the family an the community help us to see how the failed promises of both tradition and modernity in

contemporary India fold and shape individual experiences of distress (1994).

The anthropological contribution to the understanding of experience may be seen in the move to displace the idea of a transcendental subject and substitute it by the notion of a historically constituted subject. The person who is the agent, the subject, the locus of experience, is not some kind of a spirit in history. He or she bears the marks of the era in which he or she is born; of the family, the community, the social traumas to which the community has been subjected and it is the active working with these social and cultural institutions and forms which makes it possible to recognise the signature of the social into the individual. If anthropologists have erred, it is on the other side of assuming an over-socialised concept of personality. For instance some social constructionist views hold that stories people tell about themselves are not about their psychic realities but that these reflect social conventions in every detail (Rosenwald, 1995). But are social conventions sovereign? This view would lead us to think that conventions press individual narrations of events into their own service to the extent that the function of individual narration simply becomes that of reproducing the conventions within which it is told. But if this were so then we would say that it is never individuals who are bruised and beaten but only the social body that is bruised and beaten. A more balanced view is to see social and individual development as promoted by the tensions between narrative and convention: narratives are better viewed as mediating between person and situation rather than reflecting on *either* social *or* psychic reality. The stories that people tell reflect critically on both, since they are not reducible to either. It is in recognition of this double location of individual existence and of lives as storied lives, that Arthur Kleinman and his colleagues have been trying to press for concepts such as that of *social* suffering (Deadlus, 1996) and *social* health (Desjerlais,1995). While many scholars will readily grant the importance of collective representations, experience is more commonly thought of as having an individual location for one could argue that it is not groups, categories, or texts that suffer but individuals that suffer. By naming suffering as *social* suffering and similarly health as ***social*** health, Kleinman and others are arguing that experience also has a collective dimension. It is not only big changes but also changes in the everyday environment

through demography, urban disintegration, homelessness, etc. that may shape the experience of whole collectivities and consequently of individuals located within them.

Techniques for the Collecting Narratives: I shall begin by describing three different techniques that are used in the collection of interviews - (a) the narrative technique (b) the amplificatory technique and (c) the elicitory technique. All three techniques touch upon the question of the temporality of the narrative and the relation between interviewer and interviewee. In the *narrative* technique, the emphasis is upon the sequentiality of events or a particular even that happened in a perons's life. Typically such interviews assume that the order of telling should broadly correspond with the sequence of events as they happened. The narrative is linear and oriented. The interviewer would typically intervene with such questions as, "And then what happened?"; "Did you begin to feel depressed before the accident or after it?". Underlying this approach is the model of life as a journey in which one starts in one place (e.g. birth) and then goes through fixed sequences of let us say, schooling, job, marriage, parenthood, etc. till one reaches the journey's end in one's death. The major advantage of the narrative technique is that it helps us to see the variations in the biographies of similarly placed persons in a society. While many sociologists follow the Weberian idea of ideal typical role models so that an interview may be seen as representative of a particular role - a bureaucrat, a doctor, a widow; others use the narrative technique to see the range of elective biographies possible in a given society.

In contrast to the narrative technique of interview, the *amplificatory* technique does not begin with the idea of fixed phases around which the story of a person's life may be organised. It allows the narrator to organise the story around the most significant events that he or she perceives. Instead of questions of her own, the interviewer is completely cued to the narration and may interrupt or encourage the flow by questions that would simplify amplify what has been stated. For example a close examination of the interviews recorded recently in a book by Pierre Bourdieu and his colleagues (1993) documenting the various forms of misery in large cities, in the large institutions, and in families in France, reveals that the interviewers have allowed the subjects to present their lives as they would like to order these themselves. The

simple effect of the juxtaposition of so many images in the tragic confrontation of points of view that are often antagonistic, often irreconcilable because they all arise from the working out of a social logic. Such a play of contrary images would be difficult to achieve if the authors were starting with their own models of social misery and eliciting narratives around these. I shall take one example from this text. I take some excerpts from the interview with an immigrant worker (The text from the French has been translated by me).

> Abbas: " Are we the same, the same creatures as on the first day (of our immigration in France)? Who is it that has metamorphosed us? From which date is our metamorphosis (in the strong sense, by the effect of a divine curse)? One does not come to see it, it falls on us when it is too late to resist against it..."
>
> Sayad (the interviewer): *In what consists that "curse"? Why that "curse"?*
>
> Abbas: "But for understanding that, it is necessary that I tell you everything since the first day ... [Abbas then goes on to give a detailed description of changes that he began to notice in himself – that he was talking to himself – that he was talking loudly to himself and then concludes}
>
> Sayad: *For example?*

In this exchange we see that the interviewer Sayad, picks up certain statements made by Abbas who is being interviewed and builds his questions around those very words. The nature of the questions – for example?; that is to say?; can you explain that?; – serve to amplify the narrative and give the reader a much better sense of the *meaning* of being an immigrant worker while simultaneously highlighting the features of French society that make for that experience. The amplificatory technique does not give us full information of a person's life but carves out those experiences that the subject considers to be important. Thus we can learn what it is to have an illness experience in a modern hospital; what it is to work in an institution; or what it means to cohabit with a man who does not understand the malediction of

his wife – some of the many topics pursued by Bourdieu and his colleagues. As Bourdieu argues, the juxtaposition of these interviews then provides us with a unique understanding of the working of social logic for different categories of people in French society today. It does not give us an authoritative reading of events, but it tells us what people consider to be important and how they think the social events and institutions have shaped their lives.

The *elicitory* interview is most commonly used when there is a clear hypothesis to be tested and the emphasis is upon the eliciting of information rather than experience. This often taken the question - answer format. It is useful when relations between clearly defined variables - e.g. relation between mother's literacy and school enrolment of children – are being examined. Similarly it is useful in large epidemiological studies to assess the incidence of a disorder (e.g. depression) through well defined indicators. This kind of interview is not very useful for reaching an understanding of experience but may be important for forming an idea of dimensionality of a particular problem.

Methodological Issues: Collective Genres and Individual Narratives What are the methodological issues in the analysis of narratives? From the previous discussion it would be clear that narratives may be seen as mediating between the inner life of a person and the external institutions which often make up a solid architecture of rules and resources. Unlike the model of truth and witnessing in a court of law, *there is no single version of an event that may be held to be true*. The psychoanalyst Donald Spence (1983) made a distinction between what he called narrative truth and historical truth. In making this distinction he was not exhorting analysts to move towards unearthing the historical truth from the narratives that an analysand may offer during the course of analysis, but simply pointing out that our need for narrative coherence often puts pressure on the person who is telling the story to tell a coherent and compelling story.

Thus a sensitivity to the pressures that narrative conventions and forms exert in the process of story telling is important. Just as history does not mirror the past but is a kind of fiction that helps the historian to cope with the problems of her time, so stories that individual tell are made up of fragment from their symbolic and imaginary universe and help to give expression to experience.

In this monumental analysis of myths, Levi Strauss has argued that no myth can be understood in it singularity (see the five volume study of Brazilian myths by Levi Strauss; translation published by Random House, New York). Minimally one needs to analyse a myth with all it variants. Further, a myth yields its meaning only by being placed in a whole corpus of myths that define the symbolic universe of a given group. Levi Strauss is much more interested in the logic of myths in their collective dimension and has explicitly rejected that his method can tell us anything on the meaning of myth for an individual subject. Thus his analysis highlights the importance of creativity and social logic without entering into the question of the mapping of these myths in the individual telling. This follows from the very fact that the myths that he has analysed were made available to him from the corpus of texts collected by missionaries and folklorists. But does this mean that the structural method of analysis of myths cannot be applied to individual narratives? In my understanding the structural method gives us two important clues for the analysis of narratives from the point of view of individual and collective experience.

First, corresponding to the importance of analysing a corpus of myths, is the idea that several individual narratives need to be juxtaposed to see how social logic works in individual lives. This was the method used by Bourdieu and his colleagues to show the relation between social institutions and individual experience in contemporary France. Similarly , as mentioned Arthur Kleinman (1988a) gave a whole series of narratives corresponding to what he called the 'illness experience' which were crucial for our understanding of the manner in which social suffering is produced and lived in contemporary American society.

The second method which I have used in my own work is based on the assumption that a story reveals itself over a long period of time. Since anthropological fieldwork usually establishes long term relationships as does psychoanalysis, one can find different fragments of a story scattered over time. Patience and an attention to the different forms that a story may take helps to conceptualise how narrative and grammatical forms permeate the telling of the story (Das,1990-91). Let me give an example of this process from the work of the psychoanalyst, W.R. Bion (1977). Bion gives us story of an analysand who was repeatedly late for his

appointments. The story that was offered to account for the delay was structured around one event – in terms of his wanting to eat an ice cream and not finding it; tracing the ice cream vendor who disappeared around the corner just as if he was about to buy an ice cream and thus not getting it. It was not that the story was offered in one session – sometimes there would be a six month gap between one fragment of the story and another. Then one day it struck Bion that the event no ice cream could be reconstructed as no I scream. Bion was able to relate this to other parts of the man's story, especially of the difficult divorce that he was undergoing and suggest to him that his story was about the nature of his relationship which were 'screaming' relationships so that the 'no I scream' was also the desperate hope that his analysis would help him to grow out of the older definition of his relationships. We can see here the use of metaphor and metonymy (condensation and displacement in Freud's terminology) as means of communicating what cannot be said explicitly, but it is also important to recognise that it was only the passage of time and the repetitions that occurred over it which allowed the analysand to express what he hoped from the analytical relationship. In some of my own work on women's experience of violating during the partition of India, I have described how the register of voice altered (Das,1991). In one case a woman, who had agreed to talk about the partition insisted on making a kind of formal statement as if she were delivering a speech. Her sense of wrong emerged only after many conversations and even then she represented some of the events that I knew from other sources in the family as having happened to her as if they had happened to someone else. From the two examples given above we can make the following observations. First, it is when we juxtapose different stories that we can get a sense of the social logic within which individual narratives are embedded. Second, the element of repetition in a narrative gives us a very important clue to the organisation of experience as in the case of 'no I scream'. Third, we have to pay attention to the rhetoric devices, the formality of the voice, the juxtaposition or contradiction between vice and gesture, and finally the appearance of the others in the narrative may well be a narrative device to communicate aspects of the self as I have shown.

I now turn to the internal organisation of the narrative. Since the act of story telling depends upon the rhetorical and grammatical

conventions of a society or community in order to be told, it is especially important to segment different parts of the narrative in accordance with these Catherine Kohler Reissman has described the story told by a woman who has subjected to repeated marital violence. In her account, routine and repeated interaction was communicated through the habitual narrative genre. In this genre events were not considered unique to a particular moment in time but instead stood for classes of events that were seen to happen over and over again. They told of the general course of events over time - indicated by conditional past tense (e.g. if I wanted ...; he would insist ...). The woman made a shift in the course of her narration from recapitulating the general situation of coercive sex in her marriage to a dramatic instance of rape. As the content shifted so did the form of the narrative. Sharp tense clauses in the past tense replaced the habitual conditional past. Others have similarly explored the construction of trauma and the grammatical forms that its expression takes. One of the most devastating descriptions is to be found in Lawrence Langer's (1991) study of holocaust victims in which he shows that the holocaust is not seen as something which could be located in the past, even after more than four decades. Instead, the narrative forms construct it as *present* along with the other aspects of a personal's life.

In my own accounts of survivors of the anti-Sikh riots of 1984 in Delhi, I have shown how narratives of survivors collected in the community are to be seen as the result of co-operation of different voices (Das,1990). What this points to is that the setting in which a narrative is told has an important bearing on the form that the story takes. In a community setting there is a pressure generated on the individual to put his or her voice in harmony with other voices. On the other hand, narratives offered in a clinical setting are likely to be formed around the interaction with the doctor and hence to condense, abbreviate or omit that which may be articulated when they are collected in the community. The community as a site for the activity of narration may lead to pressure on the individual to shape his or her narrative according to community norms. The clinical setting, on the other hand, may obscure the fact that the individual's narrative has a history and is enmeshed in other narratives although it may give a larger space for individual construction of events. Clearly there are no quick fix solutions to these dilemmas. What narrative analysis

offers is not a method that can provide objective explanations of events but the opportunity of being able to interpret experiences of what it is to be human in a given place and time. As social anthropologists and clinicians we might experiment more with narrative techniques of arriving at the understanding of self and identity - one compelling reason being our concern with translating experience in forms that would make it available for reflection. Narrative analysis can not offer more - it does not offer anything less though.

Discussion Session

MODERATOR: M N SRINIVAS

- *In planning research or interventions we have to design a sampling system. What sort of sampling would be more convenient for analysis of narratives?*

When one uses large scale sampling it is partly to get quantifiable sampling errors. And clearly, the model of narrative which I am talking about cannot be converted into questions of testing variations in correlations or finding sampling errors. But I think what you are really concerned about is the question of how many and how should one collect these. I gave you two examples: one was the idea that people already have a notion that these are the different slots that we want to fill, and get information to fill up those slots. The other way to do it is to think about it in the way in which ripples of water go. You take one narrative, see who are the persons who figure into that narrative, then go to those people and see what narratives they have to say. Somebody called Charles Brook has used this very effectively for cholera research. He follows places where cholera as a local epidemic made its appearances and goes and collects narratives over there, including narratives by doctors. This is because culture is not just something which societies have; there is also a culture of biomedicine. There are several methods, but these two would speak to the question of how to move from an individualised narrative to a series of narratives.

- *My concern is that the efficacy of a method also depends on the system of sampling. So what sort of sampling method would be convenient for the narrative method?*

The point is that one tends to think of sampling as one of the available methods. But sampling is actually most useful where you are dealing with very large numbers and very often the density of the narrative analysis will not allow you to do the kind of things that you do in epidemiological research. I find that social scientists use a lot of these things because of their anxiety about wanting to be quantitative. I am very sympathetic to numbers, but here, given the nature of the situation that you are investigating, you could either

think about which kind of variations you need represented in your analysis, or you could actually follow the field depending upon how many people there are, how many investigators you have, and so on.

- *My question relates to the methodology of narrative analysis. What are the ways which help us in evolving techniques of analysing narratives? Secondly, you have talked of individual as well as collective experience, especially with people who have undergone extreme stress. Are we talking about a few representative narratives which evolve and then become a representation of the collective trauma?*

- *Most societies have something we can call a narrative. I am wondering, is there a consistent thing we can call a narrative? In a narrative story, one thing happens after another, which to me seems to be a linear thing. The narrative construction of an individual self in history seems to me to be a characteristically Western, post-Protestant thing. You presumably disagree.*

- The particular way by which an ongoing narrative might be segmented (for analysis), there are several ways. The method that I use is a computative method: in an ongoing narrative, there are clear breaks which happen, and where segmenting is possible. But I want to warn you that although these look very interesting, none of these ways is foolproof; you will get very different ways and different answers when you segment a particular story differently. It is important to realise that this difference is significant.

- On the question of locating individual selves within life histories, I would see that as a critique of McIntyre, as well as the German methods, that I would have. Underlying their work was the idea of a Christian history. Collective narratives and individual narratives do tend to be historically connected. I would not think that is therefore a method that I would apply to all historical contexts. Taking the recent example of Yugoslavia, when the collective narratives have worked together to suppress individual memories, these individual memories are not going to disappear or be buried; instead they will go along with the official narratives of the self that people may be constructing. So I would want to introduce a

plurality over here and say that what you are dealing with is not a narrative, but many narratives of the self. The question is not to construct a 'master narrative' but to see which kinds of fragments appear where. But I agree with you that to get out of that Protestant notion that there is a life story which must correspond to the collective kind of story given by the entire Christology of Christ as Savior is very important.

On the last question, I agree with you that a narrative has a certain kind of sense of one thing happening after another. That is why separating the time of telling from the time of the event is very important. When using a narrative technique, you are of course constantly asking the person what happened before that, or after that, which may not correspond to his/her internal time at all. That is why I find the amplificatory method a much better method as it is more respectful of interior construction of time. But if you are trying to get somewhere, then you can use the other method, but it does have all the built in problems of linearity.

- *I have a comment on the use of narratives in the kind of work which we are doing. Most of the questioning which we are taught in the history-taking of our subjects is really meant to quickly reach a diagnosis. There is a lot of information which we are in fact taught not to look at. We deliberately ask such questions which prevent some kinds of information from coming through so that we can reach a diagnosis which allows us to prescribe particular kinds of medicines. If on the other hand, we also claim to be psychotherapists and not just drug-dispensing doctors, we do have to take other kinds of histories, allowing the person to speak out. We do need to know various different ways of experiencing an event, or a crisis in life, so that in course of psychotherapy we are not completely bound by our personal interpretation of how the crisis is felt and dealt with.*

  Taking a narrative is also as important as a therapeutic technique by itself. Very often, if I allow a person to talk out the way he/she wishes to talk, the person goes back home feeling he/she had gained a lot from the interaction, without my having said anything at all. I learnt from my teacher that if a person has suicidal intention for example, one of the things the person wants to do is in fact to talk about it. And the more we allow the person to speak about it

repeatedly, the intensity of the suicidal intention goes down. So I am saying here that on one hand the use of narratives helps us to have a much richer and wider understanding of phenomena which we can use in the therapeutic context. Secondly, the narrative interview itself is therapeutic.

One of the things in anthropological fieldwork which I find quite startling is that there are some people in a society who have voice and others who do not. In my work I gave the example of a society traumatised by violence, but the children never spoke; only the adults could speak on their behalf. When I overheard a group of children fighting among themselves about whose house the killers came to first, we organised a camp for them where they could talk and tell what had happened. The opportunity to say should not become coercive - that you are lying and so on - but simply giving an opportunity to people who usually do not have a voice is very important. There are people whose stories are already constructed by others. The typical example is the daughter-in-law in an Indian joint family: everybody else can speak about her. In such a context, the space for story telling is a very important space.

❍ *My question concerns the qualitative researcher per se. Qualitative research obviously requires a high degree of sensitivity. How universal is the ability to do qualitative research? Can everybody do it as easily?*

I do not want to say that the person who does qualitative research is more sensitive. It is a method like a lot of other methods. But you are right at one level: it is possible that underlying it there is a somewhat different notion of knowledge. We often think of knowledge as something we have to go and grasp. More than sensitivity, I think it is a rehabilitation of the notion of passivity which may be more important: can the researcher make himself/herself passive enough to receive knowledge and not just grasp it.

❍ *I would like your reaction to a particular viewpoint I hold as a clinical psychologist. Narrative is one kind of perspective in trying to understand an individual. If we assume, as we are often told, that there are multiple perspectives in understanding reality, would not our narrative method be more enriched if we also incorporate personality variables and find out where we can plot the person on our axis? It will enable us to know where this person, who is giving a particular kind of narrative, stands on our co-ordinates, and this will give us a more profitable profile of the person for inferences and generalisations if necessary.*

One of the points I have been trying to make is that there is no single monologic way by which we can do things. Even for narratives, I have emphasised the fact that I would pluralise it rather than make it into a singular thing. Let us put the tests in relationship to narratives about tests themselves. For example, in the geriatric wards that we worked in the Netherlands, with very old people suffering from dementia and so on, one of the great use of tests was to classify people in terms of which ward to put them into: the somatic ward, the psycho-geriatric ward, and so on. What troubled me was that a lot of the tests were not about the persons, but about the regime of the hospital: the caretakers wanted this information to label people in particular ways. I am not saying all tests do that, but a lot do. I have no question that you can and must combine different kinds of methods. But we must also ask how that particular method relates to the construction of the event; that is the actual test. Some very interesting experimental work done on Piaget's tests in Germany tend to show that the experimental situation also produces certain results because it is a social situation.

❍ *Following up on an earlier comment, when you said that there is therapeutic use of narratives, are these narrative experiences similar or analogous to something like free association or life history method? I work in a big hospital with sometimes about 100 patients a day. So how do I deal with that? The second question is, we all know when narratives begin. But when do they end? Do they end when we get what we want to get or when he/she exhausts what he/she has to say.*

In a general hospital social workers do a lot of work in the area of patient narratives. Also by the time you see the person, there is already a fair exchange of narratives between people who are waiting. So you need to think about institutions which are structured in different ways, in which the doctor's

knowledge is not the ultimate that is reached, but the entire system of interaction has to be taken into account.

On the question of when narratives begin, we actually do not know very clearly. McIntyre's answer on when narratives end was death. But I do not think that is a very good answer because I hope that when I die my narrative will not come to an end. There will be others in whose life that will become a narrative: my children, for example. It is given into the temporality of the narrative structure that what we say as a beginning and what we say as an end is only a fragment. It does not have any closure.

❍ *I want to go back to a more traditional language and the notion of how to shape an audience. I am very sensitive to this idea that there is a continuous tension between personal order, collective order, biography and history. It is always overlapping and ultimately we know very little about the author who is narrating. This has to be clarified at one given moment, especially in societies and groups where people are very closely interrelated. If we consider the notion of experience: if we get a collective author, then how do we demarcate what will be individual experience, the experience of a small group of people, and that of a much larger one? This creates problems of how to connect those fragments with the whole. Could you say more about this articulation of individual biographies and larger histories? Is there a personal author or not?*

I can only answer first question raised, due to limitations of time, though I agree with a lot of things you have just said. You asked who is the author at that particular moment? When you are talking to someone else you are talking to that person as located in history and society. I can imagine individuation, but I cannot imagine complete individuality. So the author is a complex author.

❍ *Dr. Littlewood had emphasised that qualitative research cannot be atheoretical. Is this undermined in narrative analysis? Because generally there are two approaches. One is what I call the garden approach: things are noted as they are presented; this approach is atheoretical. The other approach, which I call the creation approach, is theoretical, and here some amount of inner vision or experience probably creates a bias. I want to know whether narrative analysis is an atheoretical stance.*

On the question on theory, your categories are different from mine. I do not know what you mean by atheoretical, because I cannot imagine a method which does not have an implicit theory behind it. The theory behind narratives here is a theory of person and a theory of biography. Its linkages are with some kind of a phenomenological school, with the idea of experience as temporal. How it differentiates from the classical kind of phenomenology would be that it does not have the notion of a transcendental subject. It has the notion of a historicised subject, who is located in both history and society. The third theoretical thrust that one would have over here would be the theoretical premises of healing. There is a difference between curing as with a person, and healing as in a relationship. In a lot of traditional systems of healing, for example, if a woman is possessed and she goes to a temple for healing, it is an authorisation to speak about what her husband or in-laws have done to her. She will not be cured till they are also simultaneously cured. In the psychiatric field, you would move towards family therapy or bringing the family into the treatment of the individual. But this was a complete theoretical premise in many traditional societies - that you cannot heal a person till you also heal and repair the relationships within which he/she is embedded.

CONCLUDING REMARKS BY MODERATOR : I want to make two brief points. One refers to the question on sensitivity. Marcel Mauss had long ago said that the anthropologist must have the sensitivity of a novelist, and in my own writings, I have continuously emphasised this point. One of the tragedies of sociology/social anthropology as is being taught, excepting probably the Delhi School, is that we teach students statistical methods and certain amount of fieldwork techniques, but never any sensitisation techniques. Sensitisation is very necessary not only for psychiatrists and other social scientists, but for anthropologists also. I want to add that it is not only silence, but other non-verbal communication like smiles that are a part of sensitivity.

Ethnography and Psychiatry: The Place of Qualitative Methods in Mental Health Research

ARTHUR KLEINMAN

There are all sorts of thorny theoretical questions raised by the use of qualitative methods such as participant observation and open-ended, ethnographic interviews. For example: What constitutes objectivity, reliability, and validity? Can descriptive research contribute to studies of etiology? Is non-hypothesis driven research scientific? These are not issues that I seek to address here. I have written about these theoretical problems in other publications (Kleinman 1986; 1988b; 1992; 1994; 1995). Nor do I intend to review the changing landscape of ethnographic styles and methods, for which there is already an impressive literature in anthropology. What I will do is to set out my own perspective on the application of ethnography, including a variety of qualitative methods, to psychiatry. There are clearly important uses of ethnography in the study of mental illnesses, psychosomatic relations, and mental health services. There are also limits. I plan to address both.

The Object of Enquiry

Perhaps ethnography's major challenge to established research approaches in psychiatry occurs because ethnography recreates the object of enquiry. For the ethnographer, the local world of

patients, families, and the social institutions of care is the orienting reality. By focusing centrally on the local social context, the ethnographer places social reality - institutions, social relationships, political economy, meanings, power - as the framework within which illness and care are to be understood. The ethnographer also privileges the effect on local worlds of large-scale social forces such as the huge global and national political, economic, and cultural changes of our era. And for ethnographic research, norms and normality are as salient as pathology.

All this is easily said. But its implications are profound; they all-too-frequently place the ethnographer at odds with epidemiologists, clinical researchers, and biological scientists. It is this eccentric position vis-a-vis the mainstream of psychiatric research that I believe authorises the potential for ethnography's special contribution. Yet, it also is often downright discomforting for the ethnographer and his or her psychiatric colleagues, and that not infrequently is a reason why ethnography fails.

My point is that restructuring the object of enquiry is what the ethnographer must do, however painful to his relations with colleagues and to his or her own equanimity. Thus, for the ethnographer who is studying major depressive disorder, it is crucial for him or her to begin not with the disease process - its symptoms, signs, and course - but with the community, neighbourhood, family, or work setting in which persons experiencing a major affective disturbance live and work. The ethnography needs to describe the institutional structures, social processes, and cultural meanings that are directly (or indirectly) salient for the illness experience. What is overridingly at stake for family, patients and care-givers in that micro-context provides the crux of the ethnographer's description. While major patterns emerge, so will significant pluralism in experience, its meaning and social psychological consequences. Thus, the ethnographer may describe the implication for depressed patients and service-providers of a predatory, disintegrated shantytown where public violence and substance abuse cluster, or of a rapidly changing working class neighborhood where new unemployment intensifies long-standing spouse abuse and the powerlessness of women. Seen in this perspective hopelessness and lack of a sense of self-efficacy are not so much risk factors for (or psychological

consequences of) depression, but rather are social affects that characterise collective as well as individual experience in a local world. What does major depressive disorder mean in such a local world? Not all (or even most) members of even the most desperately impoverished and broken communities suffer clinical depression. But understanding the illness, course and treatment of those that do, requires a serious engagement with the different sorts of knowledge provided by clinically oriented ethnography and clinical epidemiologgy.

The ethnographer must insist that the description of the social context is important and practicable. He or she must also be prepared for a difficult struggle to give such knowledge salience in psychiatric research, training, or practice settings. The community focus also leads to a much wider-angled view of the local health care system. In that view, professional allopathic institutions are recognised as part of a plural professional sector shared with Ayurvedic and homeopathic practitioners and probably also professional religious institutions of care, adjacent to and overlapping with an even broader and more diffuse folk sector of care with itinerant nostrum peddlars, spirit-mediums, yogis, priests, imams, gurus of great variety, and many other folk religious healers (Kleinman, 1980). But perhaps more destabilising still of orthodox public health models of services is recognition that it is the family-based sector of care that is the most important service-provider in the community (Christakis, Ware and Kleinman 1994), a fact long- recognised by drug companies that advertise so heavily to laymen and women. Commercial advertising and commodity selling services take on great salience in this ethnographic map of mental health care.

The Illness Experience

For the ethnographer the experience of illness and disability is foundational. This subject is engaged via interviews and observations that realise illness narratives, stories told, negotiated, altered, and "performed" by patients, families and healers over the course of the illness.

Take, for example, the following account by his mother of the effect of the progression of her son's seriously disabilitising and

untreatable condition on her family. The account, because of the peculiar circumstances that gave rise to it - the informant's angry dismay over the impropriety of a questionnaire-based survey, also demonstrates how the lived experience of serious illness as intersubjective suffering can only be expressed and approached in a narrative that privileges the voice of the lifeworld (Kleinman 1988a: compare Mishler 1984).

Mavis Williams is a forty-nine-year-old architect and mother of three. She is a single head of household; eight years ago she and her husband of fifteen years divorced. Her oldest child, Andrew, age twenty-three, suffers from inherited muscular dystrophy. Now in a wheelchair, he is progressively losing control of his speech, arms, and upper body. The disorder first appeared when he was nine years old, but it seriously accelerated when he was twelve. It is incurable. His neurologist's prognosis is a slow decline of motor activity over three to five years, with subsequent mental deterioration and death.

I met Mrs. Williams not through clinical consultation but in the course of a field research project. I had administered several questionnaires to her to ascertain her reaction to her son's illness and to obtain her evaluation of its effect on their family.

> "Dr. Kleinman, I hope you don't mind me saying this to you, but I found the questions ridiculous. I filled in all the little boxes, but I think the questions are superficial. You really want to know what impact my son's illness has had? All right, then, you need to get at the way it has torn us apart, divided me from my husband, affected each and every one of us and our plans and dreams. When the questionnaire says, "Has the effect on your relationship with your spouse or your children been minimal, moderate, serious," or whatever it says - you know the question I mean - what does that have to do with a family turned into a cauldron? With explosions of rage, with a daily grief that sucks your eyes dry, with turning away hurt and empty? It is the totality of its effects, its all-encompassingness that you should study. And especially its deep currents of desperation and failure. There is a little voice in me which, if I knew you better, would scream at you Doctor, it has murdered this family.

There is no stability; we can't work it through. Andrew's illness doesn't end. It tortures him; it does the same to us. John, my husband, blamed me. It seems to come from my side of the family. John collapsed, literally collapsed. He couldn't handle it or do anything for any of us, even himself. He ran away and drank. He was no help, no help at all. but I can't really blame him. Who can expect to meet a test like this? It is the daily struggle to stay on top of it. I blame me for being absolutely, totally incapable of separating any part of me from Andrew's suffering. I have no free space, no private and protected place to get away and call my own. It has taken all of me. What is a mother to do? Between this horror and working to support the family I have, I really have, no—no—time! Zero time for me.

Look at Barbara and Kim (her other children). What have their lives been like? Guilt because they are normal. Anger, intense anger because Andrew has required so much of my time and energy. I have had, I'll admit, precious little left over for them. But they can't express any of this. How can you, when the person responsible is dying slowly, day by day, in front of your eyes? So they can't express it to him; they take it out on me! Like John does, like Andrew does, like I want to also —since there is no one else strong enough to take it.

OK, tell me. How do you convert this into a +3 or -3 answer, to a decimal? How do you compare it with other people's reactions? I insist it is illegitimate to make comparisons. We are not things. This is not an "interpersonal problem", a "family stress" – this is a calamity! I do not exaggerate. Before Andrew's disaster we were like everyone else: some days good, some bad. Then we had problems. But looking back, that was a kind of paradise I can hardly believe was real. Now we are burning up. I sometimes think we are all dying, not just Andy. Even my parents and brothers and sisters have been more than "affected", Dr. Kleinman. You look around you—you look! This, what you see, this tomb, our family's tomb".

The illnesss experience, then, is about meanings, affects, and social interactions. The way serious illness is lived, reacted to, and the way it comes to affect a local world (and is affected by that world) constitutes the focus for a mini- ethnography. That ethnography seeks to describe not only the psychosocial context but also the effect of cultural patterning of idioms of distress and approaches to help seeking, as well as the differences created in experience of gender, caste, class, and age cohort.

David Morris in his lucid account 'The Culture of Pain' (1992; 1995 in press) suggests how voice, genre and moral community offer analytic means for unpacking the social forms of illness. Voice is not only idiosyncratic but also expresses the differences of social categories of person. The voice of dominant patriarchal cultural traditions often occludes the voice of women and members of powerless social groups. Genre is the stylistics of illness as a cultural force. In the West, Morris argues, tragedy is the genre of expressing serious suffering. In Chinese society, Joan Kleinman and I have shown, the dominant genre is both sociocentric and individualistic, using the language of bodily complaints as a powerful metaphor of suffering in the social world (Kleinman and Kleinman 1991; 1994). Veena Das (1995), in her exploration of the consequences of communal violence on everyday life conditions, suggests that for those who are powerless, the genre itself may be fragmented, incoherent, at times offering a subaltern resistance to dominant genres, and more validly understood via the trope of irony.

The social process in a moral community means that patient voice and family genre are performed. The performative aspects of illness narratives mean that clinicians and clinical researchers need to interpret stories of sickness with an eye to the performance's intentionality. What does the sick person or family member seek to do: express a grievance, influence more powerful persons, secure resources, manipulate work relations, and so on? This aspect of illness experience also sensitises the researcher to the performance of the clinician as well. For psychosomatic conditions like chronic pain, the skepticism and sometimes downright denial that is part of psychiatrists' responses to patients with hysteria or hypochondriasis may itself be the most influential contribution to a dialogical performance that often frustrates both patient and practitioner.

The moral community of the clinician also points to the powerful influence of the culture of biomedical institutions on clinical practice and research. This needs to be a self-reflexive aspect in the construction of illness narratives, an appreciation of the social process of clinical experience. That is to say, for the ethnographer, the culture of biomedicine is at least as powerful an influence on care as the culture of patients and families.

Explanatory Models and the Limits of an Ethnographic Approach in Clinical Care

In the 1970s, I introduced the idea of explanatory models (Kleinman 1975; Kleinman, Eisenberg and Good 1978). These are the explanations that patients, families and practitioners hold concerning the illness experience. Explanatory models (EM), I emphasised, are partial, changing, and at times may not even be articulatable. Contradictions, inversions, discrepancies of all kinds, characterise difference among the explanatory models of key participants in a clinical drama. I introduced the idea in order to improve clinical practice by encouraging busy clinicians to enquire systematically, and with respect for differences, into the experience of laymen and women. This methodology was intended to involve the clinician in understanding the meaning of the illness and the treatment for the patient by getting the practitioner to ask patient and family members: What do you think is the problem? What name do you call it? What do you think is its cause? How does it affect the sick person and his body? What kind of course and outcome do you expect? What treatment do you prefer? And what do you fear most about the illness and also about the treatment?

Armed with these queries, I thought (naively it now would appear) the clinician would feel empowered to engage in a deeper and broader psychosocial interaction with patients, and would also relativise his or her own explanatory model. The intention was to model care on the basis of ethnography, and to encourage clinicians, like ethnographers, to understand the native's point of view and local practices.

Great was my consternation, then, when I discovered in the context of visiting various clinical training programs that what I

thought was an aid to increase the humanity of patient-doctor encounter had become appropriated by the biomedical methodology to become something firm and fixed as if meanings could be materialised into things, real stuff that could be weighed and measured and registered in the medical record, like the patient's temperature or weight. Thus, well-intentioned students or residents would present cases to me in the classic 2-3 minute clinical summary style and along with the patient's hemoglobin level, and quantitative analysis of serum electrolytes, would tell me the explanatory model as if it were a biomedical "variable" of the same order as the rest.

I hope it is clear from the context of this talk that nothing would have been further from my mind. But in the hurly-burly of clinical work with only a few precious moments to "see" patients, the EM technique was assimilated to the overall biomedical model. Thereby I learned a great lesson, namely, that no matter what was taught in the medical social sciences and humanities to even the most sensitive medical students, the very social structures of training and service delivery, so based in the ZERO-SUM moral economy of time and money, would bend that practical knowledge and professional process of interaction to its own purposes, so as to distort the very purpose of the practice.

Thus, I am personally painfully aware of the institutional constraints placed upon qualitative innovations like the EM methodology or the illness narrative approach to clinical care. These run against the grain, indeed they run against several grains in biomedicine. First, they challenge the essentialism and naturalism of the biomedical paradigm; they also challenge the close connection of that paradigm to the guiding bureaucratc rationality of modern societies generally, which as Max Weber showed is characterised by efficiency, instrumentality, and quantification. Thus, quantification is not just any old aspect of biomedicine, but a vital stake in its bureaucratic form. Qualitative methods, which seem, on the surface, different but complementary, are more of a threat than they first appear, which though regrettable is an important insight into why they are frequently denied legitimacy.

Thus, one limitation in qualitative methods is that the very health services they are meant to strengthen offer recalcitrant

obstacles - conceptual and pragmatic - to their use. Another limitation is that qualitative methods are not in fact appropriate for every field of research. In and of themselves qualitative methods really cannot test hypotheses or establish incontrovertible evidence of causal relations. In domains requiring either, their role must be seen as accompanying quantitative methods in order to help establish the validity and salience of findings. In the domain of treatment outcome research, again, ethnographic methods are very important to assess actual meanings and practices, but must be included together with quantitative measures to establish efficacy.

Perhaps the chief limitation of ethnographic methods is that they cannot be simply taken up and applied without adequate training in social theory and anthropology. Most questionnaires used in social surveys or epidemiological studies can be applied by medical researchers often with fairly limited training that builds upon what they already know about sampling, statistics, and interviewing. But ethnography cannot simply be taken up and applied in that same fashion. To begin with, few medical practitioners have acquired a serious background in anthropology. Yet, ethnographic participant observation and interviewing are conducted in the framework of anthropological concepts such as the social construction of reality, cultural practices, social experience, cultural idioms of distress, essentialism, medicalisation, and so on. To apply them requires learning a discourse that is so substantially different from biomedical training that the very intuitions of the one run counter to the other. For example, take clinical interviewing which is directed, interrogative, and close-ended. Ethnographic interviewing is quite different: it is open-ended, often (though not always) non-directed, and more like a conversation with a friend than the response to the imperative questions in a medical enquiry. Participant observation involves a kind of loose, personal engagement which has a resemblance to psychotherapy, but differs from it in important ways. The observer in the former, for one, is not focussed on a single person but on a network of interactions. He or she is seeking a normative, population-relevant understanding rather than uncovering the deep issues burning in a single self. Yet, perhaps I am pushing the difference for there is also some similarity to the communicative skills the psychotherapist develops and the quality of the interpersonal encounter the psychotherapist seeks to establish.

The point I am making is that even when certain skills are shared, a psychosocially oriented psychiatrist cannot simply take up ethnography and conduct a study without extensive training. For in ethnography the chief instrutment of research is the ethnographer. His or her calibration comes from intensive engagement with social theory, ethnographic literature, and readings in the anthropology of medicine. That training provides the disciplined engagement with informants, with the field site, and with the writing up of the ethnographic text that are essential to maintain the quality of ethnographic practice. A limitation of ethnography in psychiatry, therefore, is that the psychiatrist must have appropriate training and on-going collaboration with a community of ethnographers. There being less control of ethnography through formal instruments and statistics, it is all the more important that quality be controlled via training and collaboration.

I could raise other problems here. But I think I have highlighted the chief ones. If ethnography is to flourish in psychiatry it must be employed by well-trained psychiatrists who are in active communication with anthropologists. That is the kind of community that my colleagues and I have tried to develop in the Department of Social Medicine at Harvard Medical School. Relatively few psychiatrists in our medical school have taken up ethnography, but those who have, received advanced training at the M.A., Ph.D., or post-doctoral level where they have worked closely with anthropologists or become anthropologists themselves.

The place of ethnography in mental health research oriented especially to the "new-morbidity" – violence, displacement, substance abuse, the abuse of women, children and the elderly - can be much more extensive than it is at present. But this could be developed in several ways: namely, by employing anthropologists, by employing other social scientists with training in qualitative methods, and by training up a new cadre of researchers in psychiatry who have demonstrated their expertise in ethnographic field studies. I see a role for each of these types of contributors. Outcome research, clinical epidemiology, and psychosocial studies in psychiatry of a variety of kinds could benefit as well from ethnographic studies. Although there are serious obstacles and the development is late in coming, there is

reason to believe that over the next decades ethnographic research will become a larger and perhaps even routine component of mental health research. That should increase psychiatry's responsiveness to the local conditions of health and illness, to the social processes in the course of illness and help seeking, and to psychiatric care as a cultural practice.

Discussion Session

MODERATOR: M N SRINIVAS

❍ *Thank you, Dr. Kleinman, for a very inspiring presentation. I have two comments*
First, your presentation reminded me of two special issues of Daedalus a few years ago, where AIDS was discussed as a human suffering. A lot of qualitative data came in that model about AIDS. The second comment is that in the Indian context, patients describe their problems in terms of Vatta and Pitta, heat and cold. How do we translate these in terms of management?

We begin with local idioms, and the idea is to understand what the local idiom is. If you are seeing a lot of a local idiom, that is just the thing to do research on. Medicine differs from anthropology and sociology in the following way: medical science draws a fixed line at the metaphorical extention of a symptom. We do not allow biomedical language to extend beyond the body. But virtually all symptoms extend beyond the body into the social domain. Sociologists and anthropologists explore the social extension. Crucial in getting a local idiom of distress is to explore its broader metaphorical extension into the family, into the networks, into the caste and class, into the village, into the workplace. That is what working with local idioms is about. Most psychiatric and medical epidemiology needs to be complemented - these are epidemiologies of the disease. Side by side with these, we need to have cultural epidemiologies of the local idioms of distress and the local syndrome. They relate to each other in complex ways.

❍ *I would like to know whether qualitative methods would be influenced by the age and sex of the interviewer. I experienced this in the Bombay riots, where I have been asked what my religion is and Muslims have refused to talk to me after seeing my "bindi". Do these factors effect qualitative research?*

I certainly think those things effect research. The critical part of having good interviews is developing a real sense of rapport. I liked the way that Veena described how she worked in rebuilding together with people who had been burned out of their houses or had their family members

killed. And while they were rebuilding together, she got their narratives. In research, you have to think of ways to get out of the structure of the medical setting. I once made an error in research, in which I assumed that patients who came into a medical setting that I was in, who did not complain of psychological dimensions of depression but did complain of somatic dimensions, would do the same thing when they went home. In the medical setting, they talked about what they thought they were supposed to talk about - their physical complaints. When they went home they talked about other things which they did not talk about in the medical setting. So, developing rapport, finding alternative ways to interview people in different settings, and also dealing with the issue of interviewer bias are all important. In the United States, there have been several studies which show that if the interviewer is an African- American, and the interviewee is also an African-American, you get a much different story than if the interviewer is white. You generally get much more anger and much more of a sense of racism because, in a sense, there is a feeling of authorisation of permission to talk about a shared dimension of experience, while with a white interviewer, it is much more difficult to do that. So certainly, you are right - these things have to be taken into account. However, I found many examples in my own work, where establishing rapport allows an interviewer to work with a wide variety of people.

❍ *If we are fundamentally narrative beings, what is the role of narratives in therapy, especially for group therapy, family therapy and so on?*

You can use narratives in remarkably interesting ways in therapy. One is to examine the way in which the therapist implots the narrative of successful outcome. One of my former post-doctoral fellows, who happens to be both an anthropologist and an occupational therapist, has shown how occupational therapists working with quadriplegics - paralysed from the neck down - implot stories in the care that allows a sense of success, even though the success is only that the quadriplegic can move his/her neck a quarter of an inch more to push a button.

I'd like to say one other thing. We've emphasised the narratives of patients. But we ourselves, based on the definition you gave us, are engaged in narratives. Those narratives are both narratives of our trajectory and our own local world, and they are the special narratives and narrative forms of medicine. One of the powerful things that happens in qualitative research is that it sensitises us to be very critically self- reflective about how the culture of medicine, through the structuring of narratives, effects the care that we give. My own feeling, as someone who has spent a lot of his life being critical of psychoanalysis, is that we have never studied how psychoanalysis disables patients. I am convinced that there are a substantial number of patients who have undergone psychoanalysis and been disabled. The way that one can understand this would have to involve the examination of stories. I am sure most of you don't agree with me, but if we were to examine this question in research, which I think is a wonderful one to examine, I suggest that we gather stories. When I asked one of my friends what five years of psychoanalysis had done for him, he said, "Arthur, psychoanalysis reduces men to dust." Of course, we will say that was a bad analyst. But we should do studies like that. Think of outcomes. The central issue in medical anthropology is the issue of efficacy. What is efficacy, how do we assess efficacy? I think efficacy is inseperable from its narrative framing. And those narratives are always positioned so there is never any transpositional objectivity that allows you to move beyond the narrative frame. Once we engage that sense of heterogeneity of outcomes, seen differently by differently positioned actors, we realise that efficacy itself is profoundly embedded in stories. Measuring efficacy just by measuring whether the culture shows that the streptococcus is cleared or not is only part of the story.

❍ *As you mentioned in one of your slides, there is a large gap between the number of patients with epilepsy and number of patients receiving treatment. According to you, it is only qualitative research that will throw light on the reason behind this. But was it not quantitative research that showed us this problem to begin with? I think qualitative research can only serve its purpose after a number of quantitative researches have been done in an area, and not on an entirely new, raw area.*

I don't agree with you. I understand what you are saying, and many of my friends, who I have a great respect for intellectually, would say the same thing. My friends in clinical epidemiology, and internal medicine would agree with you. But my own experience leads me to believe that you do not need to have quantitative research precede qualitative research. If you look at major survey units like the NORC at the University of Chicago, or the Institute of Social Research at the University of Michigan, they never begin a survey without some ethnographically based interviewing first: participant observation, in order to know what kind of questions to ask, and how to focus the questions. My own sense would be to separate out reliability and validity. Reliability is the verification of observations. Validity is the verification of the concepts that stand behind and frame those observations. Quantitative research is high on reliability, or should be, if you do it right. But the only way it can really engage validity in the psychiatric area, since we have no gold standards in the psychiatric area for biological markers of a psychiatric disorder (the last time I looked, in this great age of neuroscience, not a single biological, patho- neumonic sign of a functional psychiatric disorder!), is through the examination of those concepts that stand behind the observations. So your quantitative study shows you that 70 percent of American Indian children and adults in the first month of bereavement for a parent hallucinate the voice of the dead person calling them to the afterworld. Many of them make a decision as to whether they are going to commit suicide or not, based on the nature of that voice. But their experience is not a psychotic experience, which it would be for an Anglo-American hearing that voice. Therefore, to say that these people are having hallucinations is invalid, since it is normative, and it has no pathological consequences of note for this group of American Indians as far as we know. So if you did a study there and said 70 percent have hallucinations, you have reliability and a very good Kappa score, looking at how reliable your interviewers were, but you have no validity. Qualitative research is there for validity. That is why you have to have both together.

❍ *I just wanted to share, from my experience, that we used qualitative methods both before and after conducting quantitative, longitudinal*

studies. First, we studied the health care seeking behaviour of slum dwellers in Dacca using qualitative methods to develop a longitudinal survey which is the quantitative part of the study. Subsequently, we found some gaps like chronic diseases which needed to be studied more. So, we studied about 200 narratives.

❍ *I have a comment on the issue of ethics and morals in the use of qualitative methods in psychiatry. Since I am not in the psychiatric field, I am only giving my narrative of what I understand the psychiatric field to be in India. One of the main reasons why doctors do not listen to the patient's stories is that the diagnosis is very fast. The doctor thinks that he/she knows best. Qualitative methods are not used in order to listen to the stories of the patient because of, for lack of a better word, arrogance on the part of the doctor vis-a-vis the patient. In addition, there is a practical problem - the money the doctor gets from the patient is much more if he/she sees 10 patients in an hour rather than just one. Since the money charged is substantial, the question of ethics in this sense becomes important and I hope that psychiatrists address this.*

I agree with just one minor exception: I would not necessarily use the term arrogant. It is easy to bash doctors. But actually, it is the most wonderful thing to see, in a place of limited resources, the skilled clinician, trying under the most desperate of circumstances, with huge numbers of patients, inadequate medication and time, to do the very best he/ she can within a short space. What you have pointed to are the structures of care around us, the political economy of care, that makes it exceptionally difficult to provide care with competence. We want the doctor to do two things: be competent, and be caring. But some of the political economic structures, at least in my society, I don't know about this society, work directly against that happening.

❍ *Narratives have been heard for a long time by anthropologists, psychiatrists, and others. What changes are brought about in the human being listening to narratives? Does he become wise, indifferent, humanised?*

That is a wonderful question and I have never seen any data on it. I think the effect should be that, for a moment in time, there is an interfusing of the experience of the patient

with the experience of the healer, and that the healer comes away with an appreciation and awe, a sense of astonishment, at the engagement of another person, at the visualisation of a life, at hearing the "other". You have been busy all day with patients, you want to break away and go home, then someone did something, and all of a sudden, you heard the tone, and you were with them! That makes us human. You shouldn't romanticise it, it doesn't make you heroes, it doesn't necessarily even make you wiser. But it makes you human, and much else in the doctor-patient relationship keeps us from being human.

❍ *My question is an extension of what was just asked. What is the temporal relationship between the narration and the event that took place? Supposing you are asking an adult about his/her childhood experiences, there are many intervening variables which may modify that experience. What is the reliability of that narration?*

This week you are going to work through a concern that many doctors have. Doctors are trained to be realists, and what anthropology does is to liberate us from that realism and to make us understand that the very event structure itself is engaged in the flow of experience and the construction of narrative. So you cannot make a fundamental separation between an event in the real world and a narrative in the meta-world; there is no separation. Three things are happening together. Take a symptom. On the level of symbolic meanings there is an order in the development and the working out of that in the course of your engagement with the person. Secondly, as you understand a symptom as an illness metaphor or as a local idiom, that symptom itself relates both to the world of symbolic structures as well as to the world of the patient and yourself - the workplace, the family, the political economy, hospital setting, and so on. The third dimension, which is extraordinarily difficult to take into account, is that it is all part of a historical epoch in which powerful social forces are coming to effect both symbol systems and structures. But that is where you should lodge yourselves - in that quest for the social, so that you hear the tone of the individual, but you search for the tone of the social, the historical, the social category, the social institutions, the embodied.

❍ *This is a question for both Dr. Littlewood and Dr. Kleinman. Both of you started making a political statement at some point and then stopped. The kind of biography such as one about an ex-capitalist becoming a faceless labourer and the psychological trauma associated with that, could equally well be viewed as poverty and not depression. If 70 percent of them responded to tricyclics then are we medicalising social distress? Are we looking for pharmacological fixes for social problems? Isn't that going to give rise to much more problems in the future?*

Arthur Kleinman: Let me say that here I think Roland and I probably disagree. I'll let him speak for himself. You may want to look at a book called *Psycho-Social Advances in Depression*, where I had a crisis of conscience which I put on paper. In 1992 I published the book with an eminent psychologist, Joseph Becker, University of Washington, who was a great student of depression. In the course of writing the book, I realised that I had serious problems with the idea of limiting depression to only a clinical category. The clinical category is one construction and if it helps to reduce the suffering even in the presence of poverty and the like, by all means treat it. But don't just see it as the only way of formulating the problem. In my country, there is something that cannot be spoken. It is not race, we can talk about race, but we cannot talk about class in America. That is because we have an ever-widening gap between the wealthiest 20 percent and the poorest 20 percent. And poverty is one of the major risk factors for almost every single disease in my society. In our national census, we do not collect information on social class. I would hope that the physician would be human enough to be able to diagnose major depressive disorders, but also have a side of him that sees suffering in the context of deep destitution.

Roland Littlewood: I am not sure I disagree so much with Prof. Kleinman. If we have any differences in this they perhaps reflect our cultural values than our own. I see him as more individualistic and myself as more collective, having got into anthropology through political advocacy and anti-racist psychiatry; but I am not sure that I see myself as any less political. It might just be the values which I sometimes feel he espouses, maybe I would want to question them a

little. Putting together what he said about testimony, advocacy, finding a voice – these seem to be the idioms, and my question would be: do you assume that these are universal? The way he put it, they sound human, international and appealing to all of us. I just wonder how much they reflect certain American therapies, particularly the idea of testimony. But I wouldn't say there is a major theoretical disagreement between us. We both recognise that we have got to make political statements when we are dealing in this area. I see differences in the sort of political statements we make.

Arthur Kleinman: Thank you for that clarification. I would say that in order to be effective in a clinical conversation, which I tried to be here, I tried to tell it in the universalist mode that we engage in as clinicians. But look, we know that our societies are radically different and that within societies there is enormous heterogeneity and difference. However, it would not be honest of me if I didn't say that I believe that there is no human nature, but there are existential human conditions which are created and constructed locally. They are being powerfully globalised by the media, but if we look at what is the existential locally, we would come up with very different things.

○ *Possibly, reformulation of the clinic problem within medical frameworks to take into account the social or political or cultural, might have an impact on the structure of medical institutions and on the amount of time we get to spend. But I wonder whether this approach would go further in raising a critical threshold of consciousness rather than pushing the responsibility of addressing bold matters like poverty onto other institutions. Do you feel this is something that may not happen at all or are there other issues that perhaps indicate that maybe this is a matter for patience.*

This is such deep issue. I think it will take a long time to develop it. I'd be interested in your reading of a chapter in a new book, *Writing at the Margins*, where I have a chapter called 'The Anthropology of Bioethics'. I would be interested in your response to it.

Gender and Mental Health Research[1]

ANWAR ISLAM

We heard a lot about various techniques of qualitative research today. It is not that all of us will become experts in applying those techniques. But the main idea is to make us conscious of the holistic aspect of human beings comprising social, psychological, physical and spiritual needs. Hence, as therapists, we have to be conscious that human beings are not just physical entities. Such a consiousness influences the treatment decisions we make, and without such a consciousness, we cannot really provide appropriate health services.

Whenever we talk of health research, it is essential to be conscious of gender issues. Gender is more than just woman/man - it is a social construction. Every society defines its concept of male and female, which in turn defines male and female roles, responsibilities, and knowledge bases. Knowledge comes out of our life experiences, and life experiences come out of status and role within the social context. Knowledge base of males and females are different because of differences in life experiences. Hence we have to be aware of male-female status both within the family and the community.

[1] This is a transcription of the lecture given by Dr. Anwar Islam.

Because of the cultural context, males and females may be perceived in terms of one being weaker than the other - not in a physical sense, but in a social sense, and ultimately in the sense of power. Cultural context also makes us percieve a female child sometimes as burden and at other times as asset. Perceptions of the definition of male and female has tremendrous consequences for the research questions that we ask, our theoretical framework, and how we collect the data.

Life experience defines knowledge and makes us perceive our world in different ways. Since, in every culture, males and females have different roles and responsibilities and their life experiences are different, so the definition of health is different. This is the first thing we need to understand when conducting research. For example, we had a research project in Uganda on reproductive health. There were some focus group discussions about how community leaders perceived women and their role during the reproductive period. The data showed that the women's definition of health and well-being was more holistic than than that of the men. Women defined their well-being more in terms of the well-being of their children, their family, and so on. Men on the other hand, related more to physical illness and had an individualistic approach. In [2]some ways, this may apply to the Indian context too.

Secondly, whenever we conduct research we must take into account that women are more vulnerable to certain diseases than men. But we see very few research proposals that focus on diseases primarily concerning women. This is because most of the research is conducted by male-dominated institutions, and the decisions on the kind of research to be undertaken are taken by committees consisting mostly of men.

The third thing to understand is that responses to illness also differ between men and women. There was some research in Peru about perceptions of mental illness. Results indicated that there were significant differences in responses to illness both between males and females as well as between indigenous people and others. Women would not talk about mental illness, they would not even define those problems as illness. But men would describe those as illness and try to seek treatment. Society too would have different responses to whether the illness pertains to

men or women. In a study that we did in a Middle-Eastern country, we found that the time period between when the illness first manifested and the time treatment was sought differed for male and female children.

Further, it is important to recognise that there is a significant difference in access to health care and utilisation of health care services between men and women. Women are usually more constrained to access health care, especially in Third World countries. In one of the hospitals in Bangladesh, only 30 or 32 percent patient days were found to be accounted for by women - the rest were all men.

Another issue to consider is the issue of decision-making in personal and family health matters. Comparing the male child and the female child in a family, in terms of how treatment decisions are made, there are significant gender differences, even if we control for differences in income and other variables.

Power relations between the genders forms another crucial issue. Sometimes researchers assume that power relations between men and women are equal in sexual matters. Research on AIDS conducted in Thailand found that women sometimes could not even ask their spouses to use condoms. Men had much more freedom in that society for extra-marital relations or multiple-partners, and their wives would become passive recipients of the HIV. We have to first understand the power differentials before we could even think of strategies of intervention and address ways in which women could protect themselves. These issues are very significant for research. Sometimes we do research with good intentions, but forget to take into account that in any society, there are both males and females. So, right from the research question to the research design to the research team and the results, we have to be aware of social and cultural factors and incorporate them into the research context. For instance, quantitative data must always be segregated by sex to account for gender differences.

In addition, it is not enough just to have a research team composed of both men and women. What roles women are allowed to play within the team - are these meaningful, decision-making roles? The team as a whole has to be gender sensitive as well.

Timing of interviews is also very important. Interviews should be conducted at a time convenient to the woman, and not when she is steeped in chores. If the research team is not sensitive to such issues, it will exclude many women from the sample.

What I have been trying to say is that in conducting research we often miss the point that women, because of their social and cultural positioning, because of their different roles and life experiences, define health and illness differently, and unless the researcher is sensitive to these differences, the research cannot be useful for to the community at large which is composed of both males and females. Further, it is not just a question of having women in your research team and in your sample size, but how sensitive you are to the social and cultural contexts of gender relationships in a society and how both you and your research questions are conscious of the power differentials in a given society. Unless that is reflected, the goal of research as empowerment through knowledge for the betterment of the community cannot be attained. Particularly in the context of developing countries, it is essential to be aware of these differentials right from the beginning and build into the reseach design ways to account for them.

Discussion Session

- *This is more of a comment than a question. I want to share my experiences when I was working with a voluntary organisation in urban West Bengal. Firstly, when you were quoting the statistics in rural Bangladesh and said that the overwhelming majority of patients were males, in my OPD clinic, between 70 to 80 percent of the patients were women. When I questioned my health workers, two out of three of whom were women, they said that this was because I came to the clinic in the afternoons. Women were more available at that time, and most of the men were at work. I accepted that explanation. But I had one woman health worker who was very perceptive and also bold enough to tell me the truth. She told me that she often found the antibiotics I prescribed in the drains near patients' homes. I was stunned and very angry, and I asked her the reason for the medicines ending up in the drain - was I doing something wrong? She said that the real reason was that the majority of the women did not come because they had an illness - they came because that was the only pretext they had to come out of their houses. They actually came to chat among themselves, though they told me that they had fever or an infection or something like that.*

- It is significant that even in Western countries, in mental hospitals, most of the patients are male. But most of the patients who go to community psychiatrists are females. There was a recent study which showed that women were prescribed medicines like Valium in large quantities because everybody would be diagnosed as having depression as per DSM4 or ICD10. There is no study done on how Valium really affected their lives. These are the things we have to be aware of when talking about research.

- *Most of the research you mentioned was done in underdeveloped countries. Once the findings were published, have policies towards these marginalised people changed? In any research design, you have the principal investigator, the theoretician-analyst, who sits in on a chair in a beautiful room and plans the entire design. Then you have the data collector who collects the data. Finally, you have the analysts who analyse that data. Would you say that the theoretician-analyst or the principal investigator should also be a data collector so that sensitivity would be better?*

It depends on the research questions and the type of research you are doing. In projects approved by IDRC in recent years, there is at least 50-50 ratio, if not more, of women as principal investigators. It is an encouraging sign that more and more women are coming forward for research, not only on women's issues, but also on general issues. Secondly, in IDRC, we try to insist on meaningful participation from women in the research team at all stages, starting from creating the research design to the interpretation of the data.

- *I would begin with a comment and end with a personal query. My comment is that I agree with you when you say that mental health is one of the most pertinent fields where it is essential to understand how culture is translated into subjectivities. But the history of psychiatry has been that of a patriarchal medical construction so far, along which lines we have been viewing women's symptoms as manifestations of individual psychopathology. But if were to shift the focus now to socio-cultural genesis of women's problems, then we would address ourselves to questions of theory reconstruction as well. Why is it so easy for us to diagnose hysteria, or borderline states or depression in women? Along this same line of argument, if we can view pathology as socio-cultural in origin, then where does the role of the mental health professional end as a healer and begin as a social activist?*

The ultimate purpose of research is to change policies, and to change the social system. If we follow the history of DSM1 to DSM4, how they evolved and how they are defined, we see that not only are they Western constructs, but they are also not easily applicable universally, without a lot of changes. Secondly, there is a lot of work being done now questioning older theoretical paradigms. It will take time to make significant changes, but in both the developing and the developed world now, there is much greater awareness that most paradigms, including development paradigms, need to be radically changed.

- *Being a gender researcher, I am trying to express the kind of frustration that one faces when doing gender-related work. You go against brick walls, because there are so many social issues which you learn about but which you are really powerless to handle. Is it that gender-related research should be done by a group which is not really doing intervention, because when you do intervention you just take these ideas, but you cannot do anything about it. This is true even of the* anganwadi *workers, through whom I do some of my work, and they have also expressed the same kind of frustration. They do both the study and the intervention part of it and feel that they are not able to achieve much. Their understanding does not help them in applying their knowledge. So how does one avoid getting frustrated in doing gender-related research? I also wonder how much the lack of gender-related studies, which has just restricted itself to epidemiological data in numbers and so on, arises out of a reluctance to look at some of these issues. Because these issues have implications in terms of power relations, and in changing the entire value system, touching them will cause chaos for some time, till a new homeostasis is reached.*

From my experience, Guatemala has a very good women's research group. They only do research, not intervention. They tell me that it is better to do research and policy papers than to provide services, because this way they can lobby and effect policy changes. So you may find some virtue in separating research from intervention and services. But it all depends on the culture and the society. Yes, it is a frustrating way of changing values, perceptions and social structure, but struggle is the only way to do that. An encouraging sign I see throughout the world is that there are more groups now than ever before who are doing research and advocating changes.

Participant Observation

M N SRINIVAS

The method of "participant observation" has been a feature of British and British-influenced social anthropology since 1922, when Bronislaw Malinowski started teaching anthropology in the London School of Economics. His field-work was carried out in the Trobriand Islands off the eastern coast of New Guinea. In America, the field-work tradition goes back further than in England, with Cushing living among the Zuni in 1879-84 and Boas studying the Eskimos in 1883-84 (Kaberry,1964). In England, A. C. Haddon of Cambridge, organised a field-expedition to the Torres Straits in 1898-99, and his team included W. H. R. Rivers, C. G. Seligman and William McDougall among others. Both Rivers and Seligman subsequently carried out field-work on their own, in South Asia and Melanesia. Seligman also worked in the Anglo-Egyptian Sudan. He encouraged Malinowski, and Evans-Pritchard followed him into the Anglo-Egyptian Sudan to study the Azande and subsequently, the Nuer and a few other tribes.

Malinowski spent in all 23 months in the Trobriands proper, and this period was split up into two, one from June 1915 to May 1916, and the other from October 1917 to October 1918. Prior to going to the Trobiands, Malinowski spent about nine months from September 1914 to March 1915, doing fieldwork mainly

among the Mailu of Toulon Island, a West Papuo - Melanesian Group and "a brief visit was also paid to Woodlark Island" (Kaberry,1964). He lived among the natives, and learnt their language, and was able to take notes directly from talks with them. He participated in all their activities, weddings, funerals, and went with them on their fishing and gift exchange expeditions. Malinowski wrote far more about the Trobriand Islanders and their institutions and life than any other contemporary anthropologist about the people he had studied. Even anthropologists who were (and are) critical of one or other aspect of Malinowski's writings, praise the richness and high quality of his data.

Malinowski's fieldwork differed from that of his predecessors in that he spent a long time among the indigenes sharing their living conditions (during the two years he spent in the Trobriands he had the company of Europeans for only six weeks). He came to know the Trobriands intimately, and no interpreter came between him and his "people".

The kind of societies which were studied by the early anthropologists were mostly tribal, and very often, data, whose existence is taken for granted in literate societies, were non-existant for them, and anthropologists had to devise ingenious methods in order to obtain reliable information. W H R Rivers, coming from medicine and the natural sciences, devised the "genealogical method" when he was a member of Haddon's expedition. He wanted to make sure that he got the kinship terms right for each category of relative, as also the precise relationships obtaining among members of a kinship group (Rivers wanted the following epitaph inscribed on his tomb: "He made ethnology a science"). The use of the genealogical method, carrying out a census of households, the details of land held by each kinship unit etc., are all now part of the bag of tools used by an anthropologist. Nowadays, he (she) also carries a tape recorder and camera.

Rivers was very careful about the recording of information. But interpretation of data is different from their observation and collection. Rivers did his field work in short trips, except for the six months he spent among the Todas of the Nilgiris. But he did not bother to learn the dialect of the Todas, and he depended for

his information on interpreters. In short, he was not a participant observer. Towards the end of his career, Rivers supported a wild form of "diffusionism", in which "explanation" consisted in tracing the origin and spread of institutions, objects and techniques, from one part of the world to another, across oceans, mountains and other barriers. Much of this was unverifiable, and Radcliffe-Brown denounced it as "conjectural history" distinguishing it from verifiable history where archival, inscriptional or other material provided evidence of the spread of customs, artefacts etc. Both Radcliffe-Brown and Malinowski rejected "diffusionism" and its resort to "conjectural history". They argued that "explanation" did not consist in tracing the origin of an institution but in finding out the part played by it in the maintenance of society or the total social system. Both of them were greatly influenced by the French sociologist, Emile Durkheim and his colleagues. From 1922 onwards, functionalism gained ascendance in British social anthropology, but by 1930 differences had developed between Radclife-Brown and Malinowski, the former hyphenating his brand of functionalism, calling it "structural-functionalism" distinguishing it from Malinowski's. Radcliffe-Brown moved away from the concept of "culture" to "social systems" and social structure while Malinowski linked function to "culture".

The establishment of rapport between the fieldworker and his people, is essential for the collection of reliable data. Evans-Pritchard used to say that data collected in the first few weeks, that is, before the establishment of rapport, should be discarded as it is usually not very reliable. The fieldworker must make himself liked and trusted by the people, for then only will they part with true information. My own experience confirms the importance of establishing rapport. After a few weeks of stay in my field-village of Rampura, I was able to get hold of a copy the "official" H.S. List (Harvest Scheme List) from the headman and I was copying it, when my neighbour and assistant, Kulle Gowda, walked in, and told me, in a loud voice, "why are you copying that silly book?" He then explained to me that he had prepared the List, and had obliged his patrons and friends by understating their yields. In those days, rationing of essential goods was prevalent in the cities, and the surplus grown by the richer peasants was bought by government officials at rates well below those prevailing in the market. This was called "procurement".

Villagers thought it unfair, and Kulle Gowda helped his friends to sell more of their grain in the open market by understating their yields. I may add here that the hereditary Accountant (Shanbhog) of the village was normally residing in Bannur, a bigger village about five miles from Rampura. He had requested Kulle Gowda to prepare the List, and the latter had helped him, and more importantly, his landowning friends.

An important element of participant observation is learning the local language. This has several dimensions : First of all, the indigenes feel pleased that a scholar from far away is taking the trouble to learn their language. It pleases them, and is soothing to their collective ego. More importantly, as Evans-Pritchard has pointed out, it places the anthropologist - in those days usually a white man or woman - in an inferior position to the local people. Evans- Pritchard thought that this was an essential part of the discipline of field-work. A student is inferior to his teachers, the anthropologist being the student learning from the (usually) illiterate people. There is a dual irony here, and a triple one where the anthropologist is white. Further, while interpreting such a sensitive area as the moral and religious ideas and values of the indigenes, an intimate knowledge of their language is essential. Crucial ethical and religious ideas are frequently multi-vocal, and the interpreter must be sensitive to meaning shifts from one context to another.

There is a vast difference between getting one's understanding of a culture directly from the people, by living with them, talking to them in their language, and participating in their activities, and from that obtained from dependence on interpreters. Interpreters cannot help refracting reality. Judged from this point of view, a great deal of work done by Western anthropologists in non-Western countries is interpreter-based. This fact does influence the quality of the data, particularly when sensitive areas of the indigenes' culture, are being interpreted. It is time that the kind of distortions that interpreter-based accounts, wherever there is resort to interpreters, becomes the subject of serious discussion among anthropologists. At the present time, however, there is an unspoken taboo on it for reasons not difficult to comprehend.

Sharing the people's living conditions, and conversing with them in their own language, are two essential prerequistes for building

rapport. But living amidst the people and interacting with them as an ordinary human being, even when they treat the visiting anthropologist as a person of high status, has its inconveniences. He has to accept, at least to some extent, local codes of behaviour and courtesy. For instance, during my stay in Rampura in 1948, I had no privacy whatever and villagers felt free to ask me any questions they liked, and also criticise my behavour. And they were not shy about pointing out my deficiencies and inadequacies. I did resent it but I had no choice but to accept it. In retrospect, however, it occurs to me that the villagers had a right to be curious about me as I was, an "odd ball" from their point of view. They wondered how anyone could spend months recording what everyone knew, and they also knew I was getting paid for what I was doing. It did not make sense. I was also a bachelor and I was past thirty years of age.

The villagers were surprised at the range and depth of my ignorance regarding agriculture and rural life. The entire village took a hand in educating me and this included some boys and girls, and even the headman's bonded labourers, who used to sleep on the varandahs opening out from my, and my cook's, rooms. As I got to know the villagers better, I learnt that they had valuable time- tested knowledge about agriculture, fertility of soils, weather patterns, flora and fauna. It is essential for all developers to know this for I am convinced that however well-intentioned they might be, their efforts are bound to fail if they are not willing to learn from the local people. One has to learn in order to be able to teach. A brief digression is called for here. As already mentioned, anthropologists traditionally studied communities for which basic information was often not available in government reports or other literature, but even when some information was available in censuses and other reports, anthropologists routinely conducted their own censuses, collected information on crop yields, household income and expenditure etc. Anthropologists are also generally sceptical of information collected in vast surveys, and wherever possible, they have tried to gather information themselves. But this does not mean that anthropologists are hostile to the use of quantitative methods when they are necessary. Qualitative and quantitative methods are complementary, and ideally should be pursued together.

A frequent question asked of the anthropologist is, "how can you generalise about a region or country, from the study of one village?" This view is predominantly a bureaucratic or political view of knowledge in which only that knowledge which can be milked to yield generalisations for eventual translation into policy prescriptions, is regarded as worthwhile, and deserving of funding. The pursuit of knowledge for its utility, either immediate or remote, has become so widespread that scholars are hesistant to admit that it is curiosity that drives them to find out more and more about the phenomenon they are interested in. It is worth recalling that at the height of World War II when England was fighting for her survival, Sir G. M. Trevelyan, declared in his Introduction to *English Social History, A Survey of Six Centuries from Chaucer to Queen Victoria*, "Disinterested intellectual curiosity is the life- blood of civilisation" (Longman Inc, New York, 1985).

But to return to the question. "How can you generalise about a country from the study of one village?" While only a very foolish person will attempt such a task, the point must be made that a good deal about the country can be learnt by the intensive study of a village. For instance, an Indian village does not exist in isolation: it is, and has always been, a part of a much wider, encompassing polity, culture, and indeed, civilisation. The influence of the latter will be seen, albeit indirectly, in the institutions, values, beliefs, rituals and material culture of the villagers. Caste as it is actually lived in, in all its multifacetedness, complexity and interdependence can be studied in the village, as also Hinduism in its local and regional specificity. Approaching the study of caste via the Manusmriti, and Hinduism through the study of the Vedas and Bhagavad Gita - the time-honoured book route of the Indologists - is no doubt valuable but it is inadequate and even distorting. It is also elitist, upper caste, normative, and it badly needs to be supplemented by studies of culture at the grassroots. There is often a disjunction between the "book-view" and the "field-view" of India, and this needs to be studied for every region, and if possible, for every period of Indian history. But unfortunately the book-view has been the dominant one, and has great prestige, and enjoys official support. The field-view on the other hand, turns up customs and practices that are part of the life of our people but which the elite look down upon, and like to ignore if not suppress. As a lawyer in Bezwada (present Vijayawada) told me during a field-trip in Andhra in September

1943, "get out, we have no customs". One last point which I feel that I should mention, at the risk of appearing immodest, is that it was from two micro-studies, one of the Coorgs (Srinivas, 1952), and the other of the village Rampura, (Srinivas,1955), that concepts such as Sanskritisation and Westernisation, dominant caste, vote bank, and the book-view versus the field-view, all emerged, and they continue to be used in the analysis of cultural and social change in different parts of India. It is well to remember here that in spite of its enormous diversity, India is one culture and this is visible in every village.

A significant development of the last forty years is the increasing salience of development - studies, a field in which economists have taken the lead. Anthropologists are latecomers to it, and it is only now that funding agencies becoming aware of the need for intensive micro-studies in order to make sure that resources and benenfits reach the most disadvantaged sections of society living in rural areas. In India, where regional and sectional diversities are enormous, and hierarchical structures are pervasive, reaching help to the poor and exploited, presents considerable difficulties. A deep knowledge of the complexities inherent in local culture and society is essential if resources have to reach the poorest. This has resulted in attempts to work out methods which try to combine a highly diluted version of participant observation and quantitative methods on a limited scale, in order to determine the spread of a practice, response or other finding. All this is done in a very short time frame. It is understandable that purists are contemptuous of such efforts, some even holding that participant observation, to be effective, must be practised in its fullness (this criticism loses much of its force where a trained anthropologist carries out development work in the region he has professional experience of). But developers are in a hurry to change people's lives, and they will try out any combination of techniques which yields results. In this connection, it may be mentioned that the World Bank has supported "beneficiary assessment" method, which represents precisely the kind of cobbling I have mentioned above, in promoting the development of sericulture in five silk producing States in India, for a period of six years. In this method, the researchers became intermediaries between government officials in-charge of developing sericulture, and the people themselves. The method has been successful in the development of sericulture in a variety of ways, and especially, in taking sericulture to the weaker sections, and women.

To return to Malinowski : He was not satisfied with the immense amount of detailed information that he had so assiduously collected about every aspect of the life and culture of the Trobrianders. "For him, the final goal of the ethnographer was 'to grasp the native's point of view, his relation to life, to realise *his* vision of the world. We have to study man, and we must study what concerns him most intimately, that is, the hold which life has on him .... Perhaps through realising human nature in a shape very different and foreign to us, we shall have some light shed on our own'" (Phyllis Kaberry, 1964). According to Malinowski, the anthropologist ought to try and grasp the world-view of the native, a most difficult enterprise for someone from a vastly different culture, and in studying the far away "other", one's understanding of one's own culture, and eventually, of oneself increases. These two propositions, enunciated by Malinowski (1922) in the Introduction to "The Argonauts of the Western Pacific", have now become part of the accepted wisdom of social anthropology if not of all the human sciences.

The understanding of another community/culture which an anthropologist obtains, is only partially the result of his intellectual efforts which includes the collection of information from the people. But what is often not realised is that the understanding also involves his emotions. Information by itself is not enough : the anthropologist's ability to empathise with the people, and their culture, is indispensable. The information he has collected by living amongst the people, sharing their experiences and learning from them, enables him to reach out in a creative effort of empathy to internalise to some extent the culture of the people and their values. It is not enough for the anthropologist to be a scientist : He should have the gift of empathy which a good novelist has.

A tacit, and occasionally, even open, approval of the culture of the indigenes usually accompanies such an understanding. Indeed, the anthropologist becomes a defender if not advocate of the indigenes and their way of life and values. Not infrequently, such understanding appears suddenly, almost as enlightenment. It also marks a distancing from the anthropologist's natal culture. It is a dual movement. The anthropologist becomes an 'aficianado' of an alien culture and at the same time, a critic of his inherited culture. In extreme cases, he might even consider himself a member of the former.

The initial stage of being confronted with another culture, or sub-culture, as when one is studying a different group in one's own culture, may be described as "first- bornness", and the heady feeling of understanding which comes after a substantial amount of field-work has been known, as "twice-bornness". The anthropologist becomes a 'dwija', to use an old metaphor. The field-work of an anthropologist is usually funded by a governmental agency, foundation, or university, and the anthropologist is under an obligation to write and publish the results of his research. Assuming that the report is written in a university department or other academic institution, the fact of having to describe and analyse the information collected in terms fellow anthropologists can understand, forces the anthropologist to distance himself from the culture he has studied. Such distancing and objectivity are further promoted by discussions in seminars, where comparison with the experiences of other anthropologists occurs inevitably. All this enables the anthropologist to translate his personal, subjective field experience into universal terms. Such translation is as difficult as it is essential.

The act of writing about one's experiences involves the imposition of sequence, order and coherence on the material. Writing makes for clarity but it also requires, as said above, distancing oneself from the field-experience. In a sense, writing is therapeutic. It not only requires distancing but promotes it. Every senior anthropologist has come across students with a writing block, and this often goes with an inability to distance oneself from the experience. In other words, it is not merely an inability to write but a failure to depersonalise the experience.

In my earlier writings I had postulated three stages in the anthropologist's field experience:the first, one of bewilderment at what was happening around him, the second, at a much later point, a sense of illumination, giving him the feeling that the behaviour of the people around him made eminent sense, and the third, distancing himself from his experience. To this I would now like to add a fourth stage, which, I fear, alters radically the character of the anthropologist's twice-bornness, his 'dwijahood'.

I think that one of the consequences of the second stage in the anthropologist's development is an endorsement of cultural

relativism, according to which the world-view of each people, arrived at after centuries of living in a particular part of the earth, is regarded as valid for them, and this is usually accompanied by a feeling of resentment towards forces which condemn that world-view and want it replaced by something considered better by politically and economically powerful, external forces.

But as the anthropologist grows older, and settles down among his own people, teaching and researching in a university or research institute, further changes occur in his outlook. He becomes more integrated into his natal culture - one might say in spite of himself - a process which distances him even further from the culture which he studied when he was younger. He may continue to be a critic of his society and some of its institutions and values, but he now does this as an "insider", and not as one marginal to it. He also sees the shortcomings of the culture he has studied, more clearly than before, though he may be very critical of the efforts of developers and do-gooders from outside to bring about change. The point I want to make is that, generally speaking, the anthropologist's natal culture reclaims him as he grows older, the early years of his socialisation triumphing over the effects of early field experience in an alien culture. In a sense, the anthropologist starts as a prisoner of his culture, seeing all other cultures through it, and after a brief period of real or imagined emancipation, succumbs to it. This is not to say that the field-experience does not leave a mark on him.

It is not unlikely that the problems I have discussed in this essay were experienced only by some anthropologists and not others. I know of a few instances where the anthropolists did not experience any liking for the people they studied. Sheer career considerations, and a puritan ethic, enabled them to collect vast quantities of data, frequently through interpreters' help, for writing up in later years, in the onward progression from lectureship to professorship. In a few cases, unfortunate experiences in the field, embittered the anthropologist to the people he (or she) studied. Such embitteredness occasionally resulted in wholesale denunciation of the culture of the people studied, or of the local exploitees of the poor. In the latter case, the need to look at the society from the "bottom up", was stressed as an antidote to accounts by those who looked at the society from the "top down". In the

process, an unhappy field experience led to taking radical, theoretical and political stances. It hardly needs to be said that that was not the only route to radicalism. A few others went to the field in order to use the knowledge they collected to change society, to mend the sorry scheme of things entire.

Discussion Session

MODERATOR: VEENA DAS

- *When an anthropologist moves into a group of people, does the culture seep in?*
  If it does, does it help or harm the observation?

  During the second stage enlightenment comes and certainly influences his observation. He internalises many of the things in that society and he looks at the world from the society's point of view. If this does not seep in, I think there is a failure in the anthropologist's imagination.

- *You said that at times the people do things which is not proper (from your point of view). Can you ask questions regarding these things while you are observing?*

  Yes. The crucial thing is rapport. If your rapport is near total, then you can ask - you are one of them. But by then, you have become so sensitive to the culture that you know when, how and to whom to ask a question. For example, I was interested in intercaste sex affairs. I took one or two of my friends for long walks, talked about agriculture, cultivation, various problems of the village, the Tahsildar's visit, and so on, and then gradually brought up the topic. Even than I felt guilty that I was doing something dirty, but such things have to be done. I also used to sit in this small shop selling bidis, cigarettes, pieces of coconut, and they thought it was great fun - this absurd man from Oxford, this high caste person, sitting in that small shop with untouchables and others. They appreciated the absurdity, but it was also a proof of my friendliness. Once you establish that, you know how far you can go with them.

- *Somewhere I read that participant-observation is described is an oxymoron. I don't know what that means. The second thing is that a doubt comes to mind about the theoretical frame that Prof. Littlewood talked about yesterday. When you go into a particular village, do you have a template approach or mapping, saying that I will get this information at the end of my study. If so, do you have any particular way of going from selective to focused?*

This is an important question. Before you go to the field, you have to be thoroughly grounded in the discipline, particularly in social anthropology. Rivers, Malinowski, Radcliffe-Brown, each came with out with their own theoretical contributions, though the initial thrust of their work was to write the ethnography of the people. Rivers came up with a form of diffusionism, Malinowski came up with functionalism, Radcliffe-Brown brought in the notion of social structure. Both Malinowski and Radcliffe-Brown were deeply influenced by Durkheim. So, when you go to collect data you have a focus in mind. But in the process, you collect a lot of additional data which may or may not become relevant for your analysis. Of the data you collect, only 5 percent may be relevant. Regarding oxymoron, I do not know who said this.

- *You suggested that as anthropologists, we are not really in a position to share completely in the world of that culture. At an earlier stage you mentioned that when we undertstand, we accept it. As anthropologists, this may be a handicap, but for a psychotherapist, this very limitation, which is inherent in being human is a plus point. If I am right, the task of anthropology is to understand. But a psychotherapist does not just stop at understanding. He also has to help the patient to help himself. So if we are going to perfectly understand his world, we are going to feel helpless. We should also, when we have understood, reject some parts of it. And the patient helps us by coming to us when he is demoralised. He is himself rejecting his personal culture and asking for help in coming out of his personal culture.*

I see the legitimacy of the point you are making. In my own mind, there has been a recent transition. Following Malinowski, I thought I understood the native's point of view. To some extent, I have done it. But now I also feel that the way one is socialised into a culture makes the person see that culture in a way that an outsider cannot. There are also anthropologists who want to intervene, and who think that it is not enough just to observe and understand. For such people what you say is very important. But intervention should come after a deep understanding of the culture and not before.

- *This is a very vital and interesting point which has been raised. Most anthroplogists know that they can get a reasonable understanding of a culture, but there is no way that they can claim to fully understand it. We also know that the same local society studied by two anthropologists can lead to two very different descriptions. This shows that there is a positional observing and a positional rendering of reality. But I would also like to add that it is not only the distinction between those who understand and those who intervene. People might misrecognise the source of their misery. For example, there was this famous study in Brazil, where women said that so many infants are dying because women are using contraceptives. The anthropologist in that case tried very hard to explain the political and economic processes leading to food scarcity to the villagers. So there is a two way relationship, as a result of which something new happens when the anthropologist enters (the village).*

- *You were an outsider to the village that you went to. Did you find any repercussions to your presence in that community?*

The villagers put me in a certain category and absorbed me into their system. I will try to explain through one or two incidents. My family owned some land in a nearby village, and I tried to hide that information. I went about dressed like a villager in a kurta and a dhoti. But they knew I was a Brahmin, and they were very annoyed I did not wear the caste mark and the sacred thread. I did not observe any rituals or any rules of pollution. The headman's family was constantly shocked by me. One day when I was shaving after my bath, the headman came and told me not to do such things as I was setting a bad example to the villagers. At the Ramnavami festival, the villagers sent gifts of raw grain, chillies, salt and vegetables to me in a tray/pan. I felt that the poor people needed it more than me, and so sent it back to be used for the feast. The headman stormed into my veranda and told me that if I wanted to be treated as a member of the village community, I should accept it. A number of incidents like this happened, and they eventually created a category for me as a distinguished Brahmin landowner from a different village. The fact that I was collecting knowledge about them also helped. The headman wanted to know why I was collecting all that information. I told him that all villages

were changing, and I wanted to record their way of life for his grandsons. This man, who had only done two years of primary school, then told me that I was doing what Fa Huien and Huien Tsang had done in ancient India. Their culture, he felt, was very deep, for me to want to record it. After that I was the sacred cow, and I was allowed to ask any questions. So I don't think I disturbed their life. It was only when somebody took me to take a photograph of a factional dispute that I got a scorcher from the headman, but this was long after I finished my fieldwork - I was on a brief visit. The headman told me that I was their friend and I should not have assisted the other side.

❍ *I want to link up Prof. Srinivas' talk with the purpose of this workshop. We are talking to mental health professionals, and very often they ask why they need to know about participant observation. To respond to that, I want to bring out some facts out of a study which I conducted, where I did participant-observation, and which changed my whole view of how to do mental health research. I am talking about my study,* ***The Great Universe of Kota.*** *I had gone to the field with a very sophisticated hypothesis in my mind. There were, in the same village, two different communities: one patrilineal, one matrilineal. I had assumed that in the matrilineal communities, the women will have more power and I wanted to find out whether women who had more power will have lesser mental disturbances than the others - this was the hypothesis I went with. I want to share some experiences, where I had to act as an anthropologist, though I had no training as one. As I went into that village, I discovered that taking information without giving anything in return was difficult. I was a doctor, and as I asked for information on mental health, many people brought patients and asked me to treat them. I had not gone prepared even with the medicines for that, as I had gone only for research. The first thing I did was to collect funds - some from my own pocket, and some from friends, so that I could dispense medicines to the people I saw. At times it was even necessary to take the patient from the village to a hospital. Some colleagues questioned me as to whether I had stopped being a researcher by acting as a healer. To my mind, firstly, it was my moral duty as a doctor to do that, and secondly, I got much more information by acting as* a healer.

As I lived in the village and examined the communities in some detail, I found, firstly, that the women in the matrilineal community, whom I had assumed had power, in reality had no power. The power is with the brother of the women, property passes on through the female line, but is managed by the men. Secondly, because of the transition of the society going on at the time, they were also losing whatever little status they had in the past. So my whole hypothesis went to pieces, and I would not have found the flaws in my hypothesis if I had not done participant-observation. This is to make the point that psychiatrists, when they are doing their studies, do have to act as participant-observers to open their eyes.

❍ *This question is related to rapport. Given that there are cultural barriers and traditional barriers in rapport building, for instance for a male anthropologist to build rapport with the women in the village, what are the techniques one uses in order to build rapport in such a situation? How do you build rapport with the marginalised in the society, particularly the untouchables and the women, and how is that perceived by those in power?*

❍ To add to my difficulties, I was a bachelor when I did this fieldwork. In villages, bachelors of 31 years of age do not exist. Once the headman even asked me whether I was 'alright'. I had to be very careful, particularly with women of marriagable age, although I could talk to elderly women. But gradually, if they are convinced that you are a bona-fide investigator, you have access. But in case of a man, very limited access to young wives and so on. You have to play it by ear. If I had gone to the untouchable area maybe a fortnight earlier, it might have ended in disaster. As for those in power, after a while they say that this man is a good man, he is collecting information from everyone, he is our friend. Two or three months before my departure, a villager told me, now you have all the secrets of the village in your books - cases of encroachments, and so on. They see you as a dangerous person with information, but since they trust you, they say it is all right.

❍ *I would just like to link your talk with Prof. Kleinman's very important comment on the ethical and moral domain of medical practitioners which he addressed along with the concept of political*

economy. I think the model of an anthropologist that you have set out, including that of humility and so on really answers one part of his question on the ethical domains of doctors as a model of practicising psychiatrists, for example, as anthropologists.

❍ Thank you. I would like to make two general comments. One relating to what Veena said about positional view. The anthropologist who thinks that he has made an objective, scientific record of a village or community or group is being very naive, because when a man observes, his total psyche is involved. As Levi Strauss said, in the process of observing the other, you are observing yourself, because both of us are human beings. You observe through your personality, through the culture in which you are raised and your entire academic upbringing. I am sure if Veena had gone to Rampura, she would have seen it differently, not only because she is a very gifted person, but she is also much more gender sensitive than I am. In my Huxley lecture in 1976, I have described that there is an androcentric bias in everything that has been written, and this has to be set right by generations of women scholars coming' and doing the kind of work that men scholars have done. If half a dozen people had been working in Rampura, they would have seen it differently. But I don't stop at total subjectivity. The clash of different subjectivities is a measure of objectivity. This is very important. We need the same place, same group studied by different people so that when they present their accounts, their different subjectivities clash. The debate following this could establish a measure of objectivity. My second comment: when people ask me where were you educated, I say Mysore University, Bombay University, Oxford, and Rampura.

Visual Anthropology and Non Verbal Behaviour

DHANU NAYAK

There are a number of definitions of visual anthropology today. These depend on the position of the anthropologist, whom she is addressing, what kind of work she is doing and/or the overall context and so on. Sometimes visual anthropology is defined according to its intent (Chiozzi,1989) whereby merely the intention to make a film on a culture is sufficient reason to categorise it as anthropological. Other times the emphasis is on the method employed to make that film, for instance whether it was based in participant-observation (Koloss,1983). Sometimes it is the subject matter or the content that is given top priority (Griaule,1957) based on which only those films dealing with some aspect of human culture would be included in the category of visual anthropology. Ethnographic film has also been defined on the basis of the function it plays in society. For example, it could play a militant function if it takes a stand in defense of the culture studied (Lajoux,1976). Or it could even be defined on the basis of the use to which the film is put (Worth,1969). Various slim and technical boundaries have been drawn between documentary films, feature films, reportages, products of cinema verite, among others, and those of anthropology. These need not be considered here.

For the purposes of this paper, it is sufficient to use a broad description of visual anthropology: it is the use of still photography and moving film in the study of human behaviour, especially those kinds of human behaviour that are more effectively studied by the use of a visual medium. For example, body style and movement, group organisation, gestures, handicrafts and other skills. Thus, while visual anthropology often duplicates the work of anthropology itself, its greatest contribution is in the study of nonverbal behaviour. Ironically, it is this nonverbal component which has also been the most neglected in social science research. Due to our interest in the "speaking being", exemplified by the current interest in narratives, a significant amount of non verbal information is lost.

This paper attempts to make a case for a more involved study of nonverbal behaviour and, while drawing upon specialised work done in this area, suggests its importance for the fields of mental health research. Some basic methodological precautions are stated. Lastly, an hermeneutic approach towards understanding visual "data" is described.

II

We often need to be convinced about the legitimacy of studying nonverbal behaviour. It appears to be a large and confusing field of expression, totally arbitrary and random in its distribution. We find it difficult to believe that there is some kind of "system" in the multitude of ways we express ourselves nonverbally. Is it significant that many Indians, especially from the South, shake their heads from side to side to say, `Yes'? Is it merely chance that all human cultures raise their eyebrows momentarily while acknowledging or greeting another person? That a shrug conveys a message? That speakers in a public meeting gesticulate with their hands in a limited number of specified ways that often emphasise what is communicated? (See Desmond Morris' *Manwatching*, 1978), for a popular overall introduction to the range of human gestures.

We do not pay conscious attention to these nonverbal signs but they act as cues, often communicating to us without the use of words. Verbal language was also considered at one time to be completely random and arbitrary with no system to it. Foreign

languages sound like "noise" to us because we don't know or understand them, while our own mother tongues appear to be equally unpatterned because we are so embedded in it that we cannot see a pattern. Now we know different. There is more information in both these extreme cases which only the trained ear of a linguist hears.

Often we see a group of people, probably talking to each other in English. If we could hear them talk it is very likely that we would be able to broadly categorise at least a few of them as being from the south, or Gujurat, or Bengal, or U.P., based on their accents. This kind of knowledge is well incorporated into our stereotypes on 'Madrasis' who say 'yell', 'yemm' and 'yenn' for 'l', 'm' and 'n' respectively, or people from U.P. who unwittingly say 'iskool' for 'school'. Such pronounciations frequently occur even among fluent speakers of English from these communities and the 'errors' of pronounciation are caused by the influence of their mother tongues on the way they speak English.

If we could not hear the people in our group talk, we could also try watching them for a number of visual clues. People from the Northeast look distinctive from those in the South. We can therefore immediately categorise them. We could also look at the ways they interact with each other: Who is assertive in a group of women and directs the flow of the conversation? Could she be a Bengali? We look at other clues to find out marital status, for instance: gold chain around the neck, bangles maybe, a red dot on the forehead. Many of these visual clues are "signs", communicating specific information in the same way as words.

This non verbal information can be manipulated, appropriated or even concealed in order to communicate certain information like we would do with words. For example, women could nowadays prefer to use Ms. as a prefix to their names to describe their marital status. But they could just as well wear or not wear a bindhi on the forehead, a mangalsutra around their neck, or maybe even a ring on the finger. Not only do these accesories communicate information, they frequently act as "markers". For instance, in the U.S. a woman wearing a ring on the third finger is still "marked" as unavailable to interested males, most of whom are deterred from making advances when they read this

sign correctly. These examples are simple ones taken from daily life. They show the way we "read" certain objects as signs communicating desired information in a condensed form.

Thus the nonverbal domain is a semiotic system in itself, communicating on the basis of signs interacting at various levels. What we try to do by using visual anthropology is decipher the basis of such kinds of visual knowledge, articulate them and build up a knowledge system based on this information. In other words, we try to find how we know what we know.

III

How significant is the nonverbal component of human behaviour and what is its role in mental health research? Nonverbal behaviour is relatively stable over time. Research suggests that "the traditions of visible behaviour are quite conservative and that extremely old human patterns are stressed in the nonverbal behaviour of living creatures" (Lomax 1973). This suggests that our body language and movement do not change as easily as, or to the extent that we think they do. This also suggests that, unlike with words, it is more difficult to deceive with the body. In this case it is not the mere wearing or removal of conventional objects of non verbal significance as we saw in the example conveying marital status; it involves more unconscious ways in which we behave nonverbally. It would thus require conscious knowledge of nonverbal communication and sustained practice over long periods of time to change, for example, body movements. Both the theatre artist and the con man know this much better than the rest of us.

Most of us know the awkwardness of a sudden lull in the conversation. Sometimes it signals the death of a topic, other times it signals that the conversation has gone out of hand. Much work has been done by Erving Goffman and others on how conversations are regulated (Goffman, 1976), how people initiate conversations, and how they take turns in talking. But not much is known about how our bodies work in such a lull. How does the conversation start anew as indicated by our physical movements? Do we move in front or the back? Or do we remain still? Do such nonlinguistic motions such as blushes, shrugs and eye movements communicate something? Work done by Desmond

Morris (1978), and Ray Birdwhistell (1970) have shown how we do indeed communicate and signal to each other through unconscious body movements.

Albert Scheflen's work shows how body positioning affects interaction (Scheflen,1964). He suggests that all English people also "move" in English and utilise this postural knowledge unconsciously for orienting themselves in group interactions. Christian Beels and Jane Ferber have used the camera in research (Beels and Ferber,1973). They took footage of a psychiatric family interview of two adults, and four children. They used a single fixed camera. and were able to examine the co ordination and synchrony of various movements across the group and discovered features of group dynamic. They compared this with the footage from a similar group therapy session taken with two cameras and a split screen image. One half of the screen showed the full body of the psychiatrist while the other half of the screen showed only the head and shoulders of the group. The difference in the methods employed showed that a lot of information had been lost in the latter because it was not possible to notice when eye contact had been made or what the patient was doing with his hands, bodies and legs. If there is a systematic way in which such nonverbal behaviour varies with verabl statements, then conscious knowledge of this kind would be of immense use for example in studying /conducting therapy sessions.

When we attempt to study marginalised sections of society, as well as victims of violence, young children and even the physically and mentally handicapped, non verbal communication becomes extremely important. For instance we may find that people who have been victims of sexual abuse as children may communicate the trauma of the experience through their bodies long before they even think or talk about it. Frequently women repress such memories and it is only when they have problems in a sexual relationship as adults that they go in for some kind of counselling or psychotherapy. Over a period of time they are able to revive that early experience prior to starting the healing process. If we had waited for them to articulate their problem, it may have been several more years, or perhaps never, before the traumatic experience was brought into consciousness. Distress is often expressed nonverbally.

The significance of such research is tremendous when we see it as cultural knowledge, with variations across cultures. Body movements may be studied, for example, in relation to other cultural processes or ways of doing things. Alan Lomax has done cross cultural work in choreometrics and related body movements with food gathering strategies (Lomax,1969;1973). As the complexity of movement varies from a one-dimensional to a four- dimensional space of body movements, food gathering strategies change from hunting and gathering, to animal husbandry, to cultivation and so on.

The significance of such research is tremendous for Indian culture. Its highly developed systems of dance for example has drawn upon culturally significant ways of expressing emotions through specific movements of eyes, legs, feet, arms, eyebrows and other body parts. These body movements and facial gestures have been stylised over hundreds of years. How they inform and relate to the ways in which we communicate nonverbally and distinctly as a culture has not yet been systematically studied. Is the expression of anger or love as indiscriminate, random or individualised as we think it is? The study of ancient dance forms may throw light on this. We also know that although some behavioural expressions such as the suckling response are genetically coded, there are others that we learn by mimicry of others as children, that is, by reflecting the expressions of adults around us we learn how to express emotions ourselves. In the current context, we could ask to what extent are expressions of violence, lust, anger or love imitating those we see today on television.

IV

I have dealt with non verbal behaviour and communication until now. But very little of the work or the examples I have given above can be systematically studied without the use of a camera for still photography, a movie or video camera. In this part of the paper I will deal with photography and its products. I will give some basic precautions one must take while attempting visual work. I will also deal with the hermeneutics involved in understanding film.

Underlying the various ways in which film has been used in anthropology, especially in its early years , was the assumption

that the medium was "transparent" (in fact the tradition of documentary films is based on this assumption). The introduction of technology in the study of human behaviour gives it the aura of being more scientific. Transparency of the medium implies that there is an external reality out there that can be captured on film, and that the camera cannot alter the "data" in any way. Semiotically speaking the indexical properties of the visual were emphasised more that its iconic and symblic properties. In a visual, iconic properties give it pictorial beauty (signification by resemblance), indexical give it a documentary character (signification be contiguity or organic connection) while the symbolic give it meaning (signification by convention).

Opposite to this belief in the truth value or objectivity of film is the the belief that it is completely subjective: that it can be made to say anything therefore it ends up saying nothing. This latter attitude shows a bias against the visual in disciplines dominated by words. Verbal language is used for poetry and fiction as well as for scientific purposes. Visual language can follow suit. Margaret Mead (1963) discussed the need to eliminate the hazards of bias in film making and advocated techniques designed to ensure the neutrality of the camera. The following suggestions act as precautions to the visual medium becoming completely subjective, as well as ensure that we get footage of a specific nature and quality to enable us to do analysis:

1. Gain competence in camera management:

   - Practise visual literacy by seeing things not in isolation but as parts of a complex web. Develop ways in which to show that context without the use of verbal language.

   - Practise exposing film before the field trip so that the technical details do not divert you. Ideally the camera becomes an extension of the self.

2. As far as possible the filming must be based on the principles of participant-observation to minimise the distortions caused by person and technology. It must ideally also be a "participant cinema" whereby the finished film is shown to the people of the culture filmed (Rouch,1975)

3. Use wide angle lens, deep focus, and minimal use of artifical lighting (Heider, 1976).

4. Film long sequences of at least three minutes each (Lomax, 1973)

5. Film whole bodies rather than shooting close-ups (Heider,1976)

6. Film whole people, studying a few people in depth rather than giving a shallow treatment of a lot of people (Heider, 1976)

7. Film whole acts, showing movement in its entirety and until it is completed (Heider, 1976)

8. Keep notes of who, what, where, when, and why of every exposure or scene (Lomax, 1973).

V

Assuming that you have collected some visual data based on the above suggestions to greater or lesser extent, it now remains to analyse the data. Although observation, recording and analysis are projected as separate acts, they are much more interconnected. I will not go into how you will analyse the data because much depends on the theoretical orientation you bring to your research, as Prof. Littlewood has emphasised in his presentation. Every method is inherently a methodology and includes theory. Without theory, visual anthropology would be reduced to a technique.

What I will now describe is the hermeneutics involved in visual media or how, as viewers, we interpret visual data. An understanding of this may guide the way we work with visual material, how we analyse it and how we present its results.

The problem with visual data as with verbal data is concerned with the polysemous nature of exposures or utterances respectively. How do we fashion univocity out of polysemy? Or how do we deal with plurality and decipher meaning? Following Paul Ricouer I suggest that it is discourse which separates actual signification from potential signification (Ricouer,1976). In other

words we must, and do, contextualise in some form or other in order to make sense. In the words of Umberto Eco, "Eating beef on Fridays and making love to X are two entirely different things; but under some situations they are both regarded as sins" (Eco, 1984; See introduction in Geertz, 1973 for a wonderful description on the role of "thick description" in interpretations and how it helps us separate the "winks" from the "twitches"). Ways in which we contextualise in order to communicate and / or understand is what I largely mean as discourse.

For films made within a research context, especially anthropological films, I suggest that interpretations depend on discourse at four levels (Nayak,1990):

1. Filmic discourse which depends on comprehension of the grammar of the film and the ability to cohere it into at least a theme or a variety of themes, at most into a narrative.

2. Extra filmic discourse which is of three kinds:

   a) Ethnographic film companion, suggested by Karl Heider, in the form of a written module detailing the general ethnographic background for the event filmed, a shot by shot description of the action, maps, charts etc., to define the relations between participants.

   b) Anthropological theory at a micro and/or macro level which is substantiated by the filmic evidence or which the film illustrates or describes. An excellent though controversial example is John Marshall's film on the San Bushman called The Hunters. The film was on the basic interpretation held by anthropologists in the mid 50's that the San were on the verge of starvation due to the nature of their subsistence techniques. Subsequent research showed that their subsistence techniques were more flexible and varied than suspected: the contributions which women make by gathering wild foods and even farming form the staple basis of the San diet. So the film by focussing on hunting shows the dramatic but undependable part of San subsistence. That Marshall's film which was based on the ethnographic theory of the time proved to be wrong is irrelevant here. That it was backed by a theory is what is

of importance. With reservations, The Hunters remains a fine film on San hunting culture.

c) Anthropological theory at the meta level whereby knowledge about the self is obtained by rationalising the other. "The moment of revelation is that of recognition in the anthropological inquiry, when things suddenly fall into place. The anthropologist loses herself and finds herself in the confirmation of rationality of the other" (Eaton and Ward,1976).

It is an intricate mix of the four above components which allow the film to be interpreted. While this is especially clear in the case of ethnographic and other esoteric films, such kinds of interpretations functioning at various levels enrich film watching of every kind.

In conclusion I would like to urge mental health professional to look at the nonverbal component of all commmunication; it has been the most neglected of all behaviour in the social sciences, and it is time that it be given a "voice".

Visual Techniques in Observation

MALAVIKA KAPUR

My interest in qualitative methods dates back to the days of my training as a clinical psychologist in the early 1960s. My research on the narratives of psychotic patients describing their delusions provided insights of the kind unavailable through quantitative research methods. One such insight was that truly empathic listening to narratives can lead to "transference" or very strong attachment between the researcher and the narrator, a condition which requires healthy resolution. This is the strength as well as the weakness of all qualitative methods including visual techniques.

A great deal of research has been carried out in child development in natural and laboratory settings using videotaped material. These have been used for training of field workers, to assess the impact of a tape depicting a particular situation of the children or their parents, testing out hypotheses based on some observations or to evaluate segmentally well-known approaches to child development such as Piagetian and Attachment theories.

There are now a number of studies using videotaped material to study children's behaviour. Some examples are: peer victimisation in boys' play groups (Schwartz & Dodge, 1993); memory of gender consistent events (Bauer,1993); maternal employment and

separation anxiety and the child's interactive behaviour (Stifter, Coulehan & Fish 1992); anger reactions in children (Fabes & Eisenberg 1993); structure and even representation in different play situations (Fivush, Kuebli, Clubb,1993); primary emotion and expression through voice, face and body (Cole, Barret & Zan Waxler, 1992); cross cultural studies of language and speech patterns in Japanese and American mothers in their interaction with children (Fernald & Morikawa 1993). Such studies have strengthened the appeal of qualitative methods by attaining desirable objectivity, reliability and validity similar to quantitative methods.

Several techniques of data management have been suggested to ensure reliability and validity of qualitative data. One of them is segmentation through which sequences are broken into small sequences of the entire tape. Two or more raters, one blind to the objectives of the study, are then expected to rate the segments. In this case, an agreement of 80% or more between the two raters is considered satisfactory. Too fine grained an analysis has been found to lead to loss of reliability. The data subsequently can be analysed using conventional statistical techniques. However, these gains in reliability often unfortunately have led to the deemphasis of core characterstic of qualitative research, that is the search for meaning.

Most Western studies have emphasised microsegmentation of video material. The possible use of the material for macro studies or 'dense representations' (or in ethnographic term 'thick' representations) have been generally overlooked (Henwood and Pidgeon,1992). With visual documentation the researcher can practise what is described by Harding (1991) as "strong objectivity" where full range of process of interpretation is presented with transparency. This method enhances the search for meaning and empathic understanding rather than universal trends. It frees the researchers to explore, and be sensitive to multiple interpretations and meanings which may be placed upon the thought and behaviour when viewed in the contexts and their full complexity. The attempt here is to *reveal* the data rather than to *obscure* it. Visual techniques, as do all qualitative methods, simultaneously liberate and discipline theoretical imagination in a way that facilitates development of dense representations (Henwood and Pidgeon,1992). At this stage, a

method of validation termed 'respondent' validation as a major tool is used as a corrective measure, where the researcher goes back to his subjects for feedback regarding the accuracy of the interpretations.

Most studies are anchored to Western conceptual frameworks with certain a priori assumptions about the individual and his context. In a culture where basic ethnographic, macro level data is unavailable, the data base is restricted and anchoring the analysis to Western conceptual frameworks may not be useful in cases where holistic documentation is called for. There is a need for a broader perspective before zeroing in on specific aspects. Visual documentation of a larger canvas offers a solution to this problem in our study of meanings of individual, family and community interactions. The following section describes an attempt made by the author to enhance quality of observations by relying on qualitative data in addition to quantitative data.

The data was obtained and analysed through conventional methods on 150 preschool children. In addition, three video clips of preschoolers, each of 5 minutes duration were obtained. The data was obtained by the author (Kapur et al,1994) through assessment and observation of preschoolers in addition to interviews with their mothers over six hours for each of the mother-child diads. The data was subjected to cannonical correlation analysis. The results indicated that the personal, social and language development of rural girls were comparable to urban children and especially boys, though their gross motor, fine motor, conceptual and readiness skills were poorer. The data failed to offer any explanations for the findings. However, the video taped material, initially recorded for the purpose of training the field workers, even in segments of 5 minutes duration revealed several simple and complex aspects of mother child interactions. Visual techniques of observation have distinct advantages in generating hypotheses for further research even at a later point of time.

Unedited video clips were used so that bias introduced by edited video documentation could be avoided. Several insights came through this method. In one video clip of an urban child and mother diad, in feeding, bathing and grooming situations, the mother was apparently very conscious of the video camera, and

this probably made her more dramatic and intrusive in her interactions. Soon enough, however, it was apparent that the subject became accustomed to the video camera. In an another clip of a rural girl child, she was seen to be unconcerned with the video camera throughout the assessment by an investigator as she spontaneously went about household chores such as bathing her younger brother, washing dishes and sweeping the floor. In a third video clip of a rural boy child having his meal, he was only momentarily distracted by the video camera. The advantage of unedited video recording is that other researchers can observe intrusiveness or bias in recording. However, with edited video documentation the researcher can chose not to report the bias. Interpretations may vary, but visual material enables researchers to test out the objectivity of observations as well as interpretations. Intrusiveness can be overcome by using the recording equipment as an extension of the observer and over a period of time till the subjects get used to it.

The video clips just discussed revealed some major differences in the preschoolers of lower middle class, rurual and urban backgrounds. These video clips may be used to illustrate the points elaborated in the strategies of video documentation. For example, the data can be analysed at two levels, i.e. descriptive and interpretive (Henwood and Pidgeon,1995).

Descriptive level : The data was used to generate low level categories to describe relevant features of the data which clearly fitted the data. The video clips, for example, were broken into the following segments, with emphasis on personal, social and language behaviour:

Self help	**Associated behaviours**
Feeds self	looking at things
Feeding Mother-helps-needed/not needed	talking to each other
Mother feeds	telling stories (mother) Persuasion needed

Walking - Can be segmented as above	enjoys meals
Combing - Can be segmented as above	eats everything
Toileting - Can be segmented as above	

Prosocial
Tidying clothes/toys/dishes
fetching and carrying
doing chores
helping children/mother/siblings

Language
Asking questions
Answering questions to mother, others, siblings and strangers
Narrating events
Telling stories

Interpretive level : The above data could also be used in three different ways :

(i) To create definitions and linkages between categories at different levels of abstractions. For example, in an urban house, the child wanted to do things herself, while her mother was over involved, intrusive and attempts to control the child most of the time. Despite this, the child had adequate self help, language and personal social skills. Similar observations could made on the other two children.

(ii) To make constant comparison between cases, instances and categories in order to fully explore the complexities of the data. One may compare the three sets of data and speculate about the first child's need to be independent being thwarted by her mother, while the boy's mother initiated certain activities while giving him a good deal of freedom, while the rural girl had to not only fend for herself but had to take the role of her mother or imitiated her. The urban

mother was intrusive and continuously reinforced the child, while the rural mothers did not do so.

(iii) To sample new cases where data is likely to explain the emergent theory. For example, urban caretaking while enhancing cognitive development was more intrusive and controlling while rural caretaking provided more freedom, enhancing socialisation and language skills. This hypothesis can be tested out on new samples.

To conclude, visual documentation is not without drawbacks. Yet being aware of them paves the way for eliminating them to the extent possible. Some of the drawbacks are:

- Ethical issues of video documentation requires prior permission to be taken for recording and protection of confidentiality.
- Intrusiveness affects spontaneous or natural behaviours of the subjects.
- Bias of the researcher may focus on what is considered essential in a subjective manner.
- Both recording and analyses are extremely time consuming.
- Procedural difficulties exist in video and audio recording.
- There are technical problems associated with gadgets of recording such malfunctioning of the equipment defective recording and processing of films etc.
- Individual differences could occur: data produced may be too sparse, elaborate, lengthy, simple or complex to segmentise.

Visual documentation, however, has added advantages over other qualitative methods such as:

- the basic data can be viewed by others who may chose to understand or use it differently, for theory building or intervention.

- there is special usefulness to study areas where unknown factors outweigh known ones which often is the case in the field of mental health.
- less accessible components of communication or interaction which are manifest as emotion or body expression rather than verbal or written expression can be studied. This is is particularly relevant in the case of children, illiterate, noncommunicative adults with a rich emotional life in addition to rural and psychotic individuals.

Visual techniques of observation have a special role in the area of mental health especially when a holistic approach is adopted. Ethnographic approaches are likely to yield rich insights embedded in the culture, to enable clinicians to plan better interventions. Holistic, empathic and context oriented approaches seem to be one of the ideal methods of work in clinical settings.

Discussion Session

MODERATOR: VEENA DAS

❍ *We all know that most of our behaviour is context specific. Do visual anthropology techniques represent different contexts?*

NAYAK: Interpretations are always context dependent. From the same body of human behaviour, depending on your perspective, you are going to make interpretations. What your question may really want to address is how are we going to seperate what is a valid interpretation from what is invalid. For that, let me give a linguistic example. Suppose we say "where are they", and somebody answers "they are in the drawer", we make an interpretation that "they" may refer to oranges, books, pencils, but we cannot make an interpretation that "they" refer to your in-laws, no matter how much you hate them. So the "text" in some broader sense upholds the interpretations we make. There may be a multiple interpretations that are possible from the same data, but you can rule out completely invalid interpretations.

What does visual anthropology give us which participant observation cannot? Secondly, it is a very powerful technique, and it worries me that you can use it to validate your pet theories. For example, Prof. Kapur showed us the child being forced to eat in the urban surrounding and the child eating very comfortably on its own in the rural surroundings. It could be quite possible that the investigator chooses the right examples to illustrate this idea that we all have. How do we prevent this sort of thing?

NAYAK: I will answer the question, what does visual anthropology give us that participant observation does not. Participant observation, historically has usually given us that kind of material that we can verbalise. Though you can study these situations without visual media, a visual medium is a much more effective use of technology for a specialised situation. There are other specific areas, like general patterns in nonverbal behaviour, which cannot be done without kinesics or choreometrics.

KAPUR: These clippings were for field worker's training. But if I were to do research on this dimension, I would take a lot more videos, and the sampling will be more rigorous. That will overcome the problem.

MODERATOR: I would like to intervene at this point and say that just as participant observation is not just one technique, visual anthropology is not simply just one technique. For example, there is a big difference between the kind of thing that would be produced with the theoretical background of a Marvin Harris, who believes that action really can be segmented in so many different ways, and other anthropologists, who would have profound differences with that kind of theoretical underpinnings. Ethnographic film making is a very varied activity and the theoretically underpinnings come in very strongly

❍ *The technique is very intrusive; if I was an adult, I might even sue. The question is about ethics: if somebody is doing this without letting the other party know, are they violating human rights? And if the other party knows, then does that have an impact on how they present themselves. How do you cope with these two situations?*

NAYAK: The first methodological precaution is that we have to base it in participant observation. So the camera becomes as intrusive as the pen, pencil, paper, and the anthropologist himself. Secondly, it is the degree of comfort with which you use the camera. So the question therefore becomes a much more general question that is not specific to the use of visual media per se. Having said this I would think that the question of ethics is the same as in the study of all human behaviour. Although some people like to target visual media especially, I think that such problems have arisen even more subtly in the case of anthropological writings which have created and therefore projected one kind of view of "the other" and ethics is involved here too, because this is much subtler and therefore more dangerous. I would think that the work of people like Said is questioning the 'ethics' of all kinds of Western constructions at this very level.

KAPUR: It is always done with the people's participation, and never without their knowledge. And if you do it long

enough, they forget about the camera after a while. We also find it very useful to show them the video later on. This makes them totally involved.

❍ *This is an observation and an experience which I am sharing in this context to reinforce what Dhanu said in response to the question of the intrusiveness of this media. I have used it extensively not in anthropological research but in social activism, particularly in organising women and in consciousness raising with women's groups. I would like to link what I think happens with what Prof. Srinivas said this morning that depending on how you position this medium, there is a rapport that gets built with it as well - with the artefacts, the camera, the photographer, and so on. But it is very important how you position it, where there is no element of hiding or secrecy. Of course in the social activism context, it does not exist any way. We are not pretending that we are not observing them or that we are not attempting to change the situation through our intervention. In fact, it serves as a very important tool for self-analysis; it has helped women analyse, for instance, their body language and their stances in the power dynamics of different kinds of relationships.*

❍ *Prof. Kapur was raising some interesting questions regarding the video clips. It would be interesting to examine other interpretations of those clips would be.*

MODERATOR: We can have two volunteers to tell us how they as observers would interpret the video clips.

VOLUNTEER 1: There was a lot of cajoling going on, which I think was artificial. The parent's irritation at the child not eating did not come through. Also, regarding the utensil washing that the girl was doing, the commentator said that she was trying to help in the household activity. But that is not so. I would think of it as a game she is playing by imitating her mother rather than as a conscious attempt to help.

MODERATOR: I will volunteer, since I have written on cinema a lot. What I noticed in the first instance was the enormous theatricality. If I was watching it as an anthropologist, I would be very unsure whether the address

of the mother and child is to each other or to the camera. This is something we find again and again in photographic representation, especially with movement. The point you made about playing the videos back to people was a fascinating one - when you play these videos back to people, it would be very interesting to know how family conversations go. To share with you one interesting information, there had been a lot of research now of families watching television together and reporting their interaction among each other. One of the worst instances that we have ever had in anthropology is of a family that was televised over years. In the end there were a lot of suicides in this family. We do not understand it very well, but anthropologists then began to wonder whether constantly seeing themselves might have had an impact on that.

KAPUR: If you have a visual material like this, people have a choice of chosing your interpretation or leaving it. That is the advantage of it. The cajoling was an artefact. She was anxious and she was pushing. But we also have the impression that urban mothers are more intrusive, and children have more feeding problems in urban areas. But that is not the point I want to make. The advantage of the medium is available for you to look at and then agree or not agree with me. That is why it is superior to other techniques.

❍ *What I will come across is your published work. The video text is not going to be available to me for reference. So it will always be your interpretation that I will be reading. That is essentially a limitation.*

MODERATOR: His point is that in that case it defeats the purpose with which you start, because people do not have access to it. That is an important point.

KAPUR: Yes, people do not have access to it. But one of the main advantages is that when unknown factors outweigh known factors, this seems to be a good method to use. When we have absolutely no background on what to study, we can use this to view what is there and then we can get our theory. This is a very good beginning point.

Focus Groups : A Tool for Qualitative Research

ANWAR ISLAM

Research: Role of Intuition

Research is the generation of knowledge for the purpose of improving the human condition. Science - the systematic application of logic, rational reasoning and observation - is one of the methods of generating such knowledge. Intuition, tradition and authority figures could be considered other sources of knowledge. Although the superiority of science as the source of knowledge can hardly be challenged, philosophers, prophets, saints and sufis (mystic Muslim saints) have long cherished the "way of contemplation, of meditation and inner reflection". In reality, knowledge cannot be achieved without contemplation and reflection. These " soft arts" must accompany and complement science. Let me elaborate the point:

Once upon a time, there was a saint or guru in India renowned for his wisdom. An eager student travelled a thousand miles to reach the guru and finally arrived at his "ashram" (school) in the evening. The student could hardly wait and went straight to the saint and ever so gently appealed: "O my revered guru, give me knowledge". The guru looked at him and said: "Well, let the sun rise and then travel to the village market. In the middle of the market, there is a big tree. O my student, stand under the tree till

darkness engulfs the earth. Return to the ashram at dusk and see me in the morning." The student followed his advise and stood under the tree for the whole day and at sunset came back to the school. The following morning, he went to the saint and said: " O my guru, I have done what you asked me to do. Please, give me knowledge." The guru asked him to do the same that day, and the day after that. By the third day, the student got utterly puzzled and, understandably, quite mad. Returning to the ashram the third evening, he thought to himself - "If the saint sends me back to the market, I am going to kill him."

That morning, facing the guru, he could hardly control his anger. But he mastered enough modesty to say politely, "Guru, please give me knowledge". The saint asked: "What did you see in the market?"" Well, nothing special," came the reply. "Did you not see some people buying things, some selling? Maybe there was a funeral procession, or a wedding party? Did you not see some kids going to school; maybe some boys teasing some girls?," the saint continued. "O yes, my guru, yes", the student nodded. "Well my boy, how can you say, then that you have not gained?" the saint continued. "And are you not mad? Did you not think of killing me should I sent you back to the market? That's good, my boy. It proves that you do have a passion for knowledge. A burning desire. And this burning desire is the precondition for knowledge. And are you not throughly confused not being able to make sense of my directives? Well my boy, confusion is the fountain of knowldge. You are now ready for the wisdom. I shall be happy to help you master the technique of seeing things, seeing more clearly, when your eyes are closed. Observation gives you bits of life, it is contemplation that makes bits a whole, that brings meaning to life."

Qualitative-Quantitative Dichotomy

It is misleading and, often irresponsible, to quarrel over the virtue of either qualitative or quantitative methods. Social scientists championing qualitative methods tend to emphasise that human behaviour cannot be properly understood if one is constrained by "measurement and objectivity". For them a social scientist must get closer to their subjects and try to see the social world from the prespective of their subjects in order to fully

comprehend it. These social scientists often complain that their fellow quantitative researchers "measure everything and understand nothing" (Filstead,1970).

Over the years qualitative methods, it must be emphasised, have produced some pioneering research. For example, John Howard Griffin's *Black Like Me* (1961) is an excellent research work that employed one form of participant observation technique in which the researcher attempts to become, effectively, part of the subjects under study. William Whyte's classical *Street Corner Society* (1981; [1943]) is perhaps one of the most eloquent examples of qualitative research using another type of participant observation technique in which a scientist observes the subjects as a scholar genuinely interested in understanding their behaviour. Case study or community study approach is another important qualitative method that produced excellent works. Robert and Helen Lynd's *Middletown* (1929) and *Middle-town in Transition* (1937) are perhaps two of the most illustrated community studies describing the changes taking place in a small Midwestern town in Indiana, U.S.A. during the Depression years. The impressionistic or interpretive approach is another qualitative method that has been used by social scientists very profitably. In this technique a journalistic approach is combined with analysis of available research findings and existing social science knowledge and theory. David Riesman's *The Lonely Crowd: A Study of the Changing American Character* (1950) is a classic example of successful application of the interpretive approach. More recently, Patrick Burman very aptly used the interpretive approach in describing the lives of the unemployed in his *Killing Time, Losing Ground: Experience of Unemployment* (1988). It is equally important to emphasise that hardly any of these scholars either refrained from using quantitative methods to bolster their research or ignored the importance of such methods. In reality, there can hardly be any research, particularly in social science, that employs either quantitative or qualitative methods exclusively. Moreover, development of computer software and other techniques in recent years are increasingly being applied to "quantify" and statistically analyse qualitative data. In other words, the lines separating them are becoming increasingly blurred and rather irrelevant. And, as noted before, no analysis or interpretation of data is possible without an inner understanding or knowledge (the Weberian concept of *verstehen* is very close to this notion of

inner understanding) that can come only from experience and intuition. Perhaps, we can again draw from the wisdom of Eastern saints to elaborate the point:

"What is your view about inner knowledge?" asked the mild mannered dervish (saint) Abduh of the traditionalistic theologian Abdurrashid of Adana.

"I have no patience with it".

"And what else?"

"It makes me sick!"

"And what else?"

"The idea is revolting!"

"How interesting," said Abduh, " that a logical and trained mind like yours, when asked for a view on a matter, can only describe three personal moods."

The Etic-Emic Continuum

Perhaps the role of such "soft arts of contemplation and reflection" is more pronounced in social science research where the subjects and the objects are meshed, or blended together. In social science research, the data is more likely to be contaminated by extraneous factors such as the views and beliefs of the researcher or those of the subjects. Following the typology of data developed by Krippendorf (1980), it can be said that social science data are more likely to be **etic** rather than **emic** in nature. Since research data, to a certain extent, are always influenced by the researcher, the subjects and the research environment or setting, the emic-etic duality, in the final analysis, is a question of degree. Value-free science, despite the bold pronouncements of Max Weber and a host of others, is a myth. The "science" of research is essentially made up of the art of making data as close to the **emic** form as possible. In other words, data must flow naturally from the subjects with minimal intervention from the researcher(s).

The focus groups (FG) technique is one of the most useful tools for social science researchers to yield data closer to the emic side of the continuum. In a focus group setting, the participants are, relatively speaking, freer in using their own vocabulary, concepts and expressions in describing a phenomenon. This freedom, if nurtured properly, should make the data only minimally influenced either by the researcher or the research setting. In survey research (and experimental research), especially when the response categories are pre-designed, the respondents are restricted in their choice. Since the response categories are prescribed by the researcher(s) and, unless each question if followed by space for 'comments' the subjects are constrained. One is forced to chose a particular response (for example, very satisfied, moderately satisfied, or dissatified, etc.) although he/she may have had reservations about the choice. In other words, in survey research, the respondents are at greater risk of being influenced by the researcher(s). Data produced, therefore, are closer to the etic side of the continuum rather that than of the emic. However, these emic-etic differences should not be viewed in absolute terms. "Neither emic or etic data are better or worse than the other; they simply differ. Each has its place in social science research; each complements and serves to compensate for the limitation of the other. Indeed, one way to view social science research is as a process that moves from the emic to the etic and back, in a cycle. Phenomena that are not understood well often are studied first with tools that yield more emic data. As a particular phenomenon is understood better and greater theoretical and empirical structure is built around it, tools that yield more etic types of data tend to predominate. As knowledge accumulates, it often becomes apparent that the explanatory structure surrounding a given phenomenon is incomplete. This frequently leads to the need for data that are more emic, and the process continues" (Stewart and Shamdasani, 1990). In other words, one should not take one of these two as the only path to truth. As an Eastern thinker said :

None may arrive at the Truth until he is able to think that the Path itself may be wrong. This is because those who can only believe that it must be right are not believers, but people who are incapable of thinking otherwise than they already think.

Focus Group : Nature and Use

In social science research, as noted earlier, the use of focus group technique has increased significantly in recent years. The growing emphasis of community participation in research activities is perhaps one of the major factors for such incrased use of focus group technique. Participatory action research can hardly be accomplished without some focus group discussions along with other techniques. Nevertheless, the term "focus group" is often misunderstood or misinterpreted. It is, therefore, important to delineate the essential characteristics of the "focus group" technique.

First and foremost, it a *group* interview technique, where more than one individual is involved. Secondly, it a *focused* interview in which in-depth probe is made on the focused subject or subjects. The final element is the fact that it is an *interview* in which an interviewer is present. However, the role of the interviewer is somewhat different. Here he/she acts more as a facilitator or *moderator* to keep the discussion focused, the participants involved and the entire process as unencumbered by personal biases or interpersonal conflicts as possible. In short, a focus group is a *group interview* on a *face-to-face* basis, it is highly *interactive, focused* and *moderated* by the interviewer. The early pioneers of focus group techniques like Robert K. Merton (1946; 1987), Fiske, Curtis and Kendall (with Merton 1946; 1956) emphasised the interactive nature of focus group discussions and the paramount role of the moderator in keeping the group "engaged". It is understandable that the focus group technique, like any other, cannot be used in all circumstances. A number of authors (Bellenger, Bernhardt, and Goldstrucker 1976; Higgenbotham and Cox 1979; Krueger, 1988; Morgan, 1988) have described different research issues and settings that are suitable for the application of focus group interviews. However, there seems to be a general consensus that focus groups are most suitable to investigate and examine complex behaviour and motivations. David Stewart and Prem Shamdasani (1990) listed several uses for focus group, each with its inherent risks. A brief discussion of these uses is worth repeating:

a. It is useful when there is a substantial power differential between the participants (subjects) and the decision-makers

(the target group). For example, the purpose of the research is to understand the concerns of agricultural workers so that relevant policies on agriculture, farm labour, rural land tenure system, etc. may be changed or improved. In such cases, focus group discussions are expected to empower the people and the community and make it easier to establish a channel of communication between the people and the policy-makers. Understandably, this can improve both the quantity and quality of feedback from the participants to the decision-makers. However, the risk is of empowering people and generating expectations and then, as it happens often, ignoring the feedback.

b. Focus groups can also be useful when there is a significant "cultural" gap between the professional researchers and their subjects. It may be difficult for an academic researcher to understand, for example, the social world of prisoners or prostitutes or, as some would forcefully argue, of "housewives". Often there is a linguistic and/or cultural barrier between the professionals and their subjects in such cases that prevents them from effectively understanding their subjects. In such cases, focus groups may be used as a powerful means of overcoming this cultural barrier, of confronting and better understanding the reality faced by the subjects. Focus groups can be useful in developing a more appropriate questionnaire. Political strategists, acting on behalf of a particular party or candidate, may make use of focus groups to develop effective campaign slogans or identify "real" issues agitating the public mind. Such use of focus group technique also has its risk. Quite often, professionals may find it difficult to readily accept the feedback received from their subjects.

c. Focus groups are useful, as noted earlier, in investigating complex behaviour and motivations that are shaped by culture, attitudes, knowledge and life experience. For example, in understanding factors leading to, or consequences of, divorce or domestic violence, focus groups can be effective. So may be the case in investigating sexual attitudes and practices of people. In these cases, a survey questionnaire or structured or semi- structured interviews may not elicit critical information that much more free interaction in focus

groups may generate. Risks are two fold: it is always a challenge to keep the participants focused enough on the subject(s) under investigation. Often emotions may take over and the researcher may get more information than ever bargained for. Some of the information, moreover, may not be useful.

d. Focus groups can be used when the purpose of the research is to understand the degree of consensus among a population on certain issue(s). While focus groups will allow the participants to express divergent views of the issue(s), a common ground or consensus may evolve through direct face-to-face interactions. For example, if the issue is to legalise prostitution or to clear a particular area of a city of prostitutes. What do people think about prostitution or how do they feel about the measures suggested? Focus groups could be very effective in these cases to identify the degree of consensus among people. A mailed questionnaire survey or personal interview can only seek opinions from individuals and analyse them afterwards. Such analysis may not indicate the common ground which is possible only when individuals are allowed to interact. However, for the moderator, the challenge in this case is to retain neutrality and avoid influencing the outcome.

e. Quite often, the researcher must bring together "opposing" parties to discuss a particular issue(s). How do labour and management feel about an impending legislation dealing with company closure? A voter group may want to know the "position" of different political parties on a certain issue. Although such inquiries may be conducted through separate interviews, often bringing together the divergent parties in focus group discussions may yield more useful information. Focus groups may help minimise tensions between the groups and help them understand each other better. However, focus group techniques may prove to be counterproductive when tensions or conflict between the groups are too great to encourage meaningful exchange of views with mutual respect.

The purpose behind all these "uses" of focus groups could include generating research hypotheses for further inquiry; developing a more appropriate questionnaire; better interpretation of survey results; and soliciting opinion on or generating new

ideas, programs, plan and policies. Obviously, when to use or not to use this technique is, ultimately, a judgement call. The decision must be made by the researcher(s) taking into consideration several factors; the nature of the research, the philosophical or theoretical underpinnings of the study, human and financial resources and time available for the study and interest and expertise of the research team. In making this judgement call, perhaps it is useful to be aware of circumstances in which the focus group technique should not be considered. First, focus, group is a qualitative technique to generate qualitative data. Obviously, it is not appropriate when the need is for statistical data. Participants in a focus group do not constitute a representative sammple of a population. Consequently, data generated from a focus group cannot be generalised for the entire population. Second, focus group should not be considered when confidentiality and security of data are of prime importance. Moreover, on some issues an individual may not feel comfortable in speaking in a group setting. Given these parameters, the focus group technique, like any other research tool, has its own advantages and disadvantages. John Hess (1968) listed ten advantages of focus group, five from the perspective of the participants and five from that of the researchers:

From the point of the participants, the five advantages, according to John Hess are: synergism (greater richness and diversity of information than in an one-to-one interview setting; snowballing (the possibility of an idea generating numerous responses); stimulation (greater group interaction); security (group setting providing "comfort" to individuals in sharing "radical" or "different" views with relative anonymity); and spontaneity (individuals being able to be selective in answering questions and, therefore more true to their feelings).

For researchers, the advantages are: serendipity (the possibility of developing completely new ideas, concepts or arguments); specialisation (opportunity to use highly trained, specialised interviewer(s) since the relative cost is lower compared to one-on-one interview); scientific scrutiny (questions/answers/arguments are likely to be scrutinised and reviewed by many and, ultimately, revised enhancing their consistency); structure (opportunity for more in-depth discussion and revisiting specific topics); and speed (being in a group, the interview process requires less time per person).

Obviously, there are disadvantages too. Often, in a group setting, individuals may feel a pressure to "accommodate" others views in order to be accepted by or belong to the group. In some cases, an individual may not even "open-up" in a group environment. The presence of the moderator may also have a "Hawthorne effect" and influence participants responses. Since in a focus group discussions are relatively freer and non-structured, the task of interpreting or summarising responses may prove to be rather difficult. On the other hand, since information in a focus group discussion is derived first-hand, researchers may feel over-confident in accepting them. These disadvantages, however, are not inherent in the focus group technique. A critical review indicates that they are, primarily, by-products of two ingredients: the environment of the focus group, and the character or skills of its leader or moderator. It is extremely important to understand the nature and role of "group dynamics" that essentially constitute the environment of a focus group, and the sigificance of the moderator's role in shaping the group dynamics.

Group Dynamics and Leadership

In any group setting, a number of factors - intrapersonal, interpersonal and environmental - influence individual behaviour. Intrapersonal factors include such demographical variables as age, sex and socioeconomic background (income, occupation, family background); and physical variables as size, height, weight and appearance of the individuals. One can easily foresee that the group dynamics in group composed of 15 to 25 year old youths will be fundamentally different from a group where 15-25 year olds share the room with people in their 60s or 70s. In some cases, the gender mix of the group may also influence the group dynamics. Personality of the participants may also influence their behaviour. Interpersonal factors include such variables as heterogeneity or homogeneity of the group, power-status differential within the group, and the pattern of nonverbal communication among the group members. For example, eithnic or racial composition of the group is likely to influence the pattern of communication among the participants. If a group consists of powerful individuals (tribal chiefs or landed aristocrats, for example) and of workers or day labourers, the group dynamics will surely be different than when it consists of people with

similar status or power. Environmental factors such as the room size, light and furniture in the room and seating arrangements are also important. Needless to say, researchers must pay attention to these factors in designing focus groups.

Leadership of the moderator is another important factor in focus group dicussions. Leaders can play a significant role in motivating the group members in participating more actively in discussions. From Max Weber, a host of other social scientists have tried to identify leadership styles and their relative strengths and weaknesses (Weber,1946; Bales and Slater,1955; Peter and Hull,1969; Ridgeway,1983). Daft and Steers (1986) identified four leadership styles each of which may be useful in a given focus group setting. Supportive leadership, according to Daft and Steers, is amiable, friendly concerned about the well- being of the members, and has an egalitarian approach to others. This is similar to Bales and Slater's concept of expressive leadership. Directive (Daft and Steers,1986) or instrumental leadership (Bales and Slater,1955) emphasises the completion of tasks at hand and generally sets rules and guidelines for the members to follow. Daft and Steers mentioned two other types of leadership - participative and action-oriented - which are, by and large, derivatives of the supportive and directive leadership varieties. Obviously, no one particular type of leadership is appropriate for all focus groups. It is a challenging responsibility of the moderator to adopt a leadership style that is suitable in terms of the objectives of the focus group, its composition and, not the least, time and resources available. In other words, the leadership style is a function of the demands put by the research itself. A moderator, to be effective, must have the skills and expertise to be flexible and adopt the most appropriate leadership style. Langer (1978), therefore, listed some qualities that a good qualitative researcher or moderator should possess. These qualities include: a genuine interest in listening to other's views, thoughts and feelings; expression of one's own feelings to others; spontaneity; a sense of humour; empathy; openness; insightfulness; clarity of thoughts; and flexibility. Although it is doubtful the any single individual would have all these qualities, a good moderator must strive to master them as much as possible.

Qualitative Research : Some Theoretical Issues

In the last section of this short paper, an attempt has been made only to highlight some of the underlying theoretical issues pertaining to qualitaive research in general and focus group technique in particular.

Philosopher Alfred North Whitehead once said that "the history of Western philosophy is nothing but a series of footnotes to Plato", presumably to stress the point that much of contemporary ideas and thoughts on philosophy, science and methods of scientific inquiry may be traced back to the writings of this great Greek thinker. One may argue whether or how much this observation is defensible. However, it can hardly be denied that some of the theoretical/philosophical issues pertaining to knowledge - its nature and methods of acquisition - discussed by Plato (and Aristotle) are very much relevant to the discussion on qualitative research (or research in general) - one of the means to knowledge.

For qualitative research, reliability and validity are still of significant concern. A focus group discussion conducted by two different moderators on the same topic with the same group may generate different set of conclusions. Is it a problem of reliabiliy of the questionnaire or questions used or that of their validity or a funcion of the expertise and skills of these two moderators? The character of group dynamics in these two groups may also contribute to such differential results. Clearly, more research is needed to better understand group dynamics issues, the role and qualities of the moderator and the issues of reliability and validity. The goal would be to set some standards that can be followed without much difficulty so that qualitative research may attain a certain level of reliability and validity. The quest for such standards must not lose sight of Karl Popper's dictum that the purpose of research is "not only to prove a hypothesis, but also to disprove alternate hypotheses".

The question of "value-free" research is at the heart of science. It is often direct interaction between these subjects and these researchers. Although complete freedom for values may neither be achievable nor desirable, objectivity is central to a scientific quest for knowledge. For social scientists, particularly for those engaged in qualitative research, objectivity is a doubly challenging

goal. Again, some pragmatic standards and guidelines are needed. Obviously, a multidisciplinary approach is required to effectively address these theoretical issues. Social scientists from various disciplines - sociology, anthropology, economics, political science, psychology and others - must work in partnership in search for answers to these challenging issues. Given commitment to the goal of setting standards and guidelines, social scienists may not find the task that overwhelming. However, even in adopting the means for achieving the goal, a "value- free" approach is essential. The ends and the means must have some congruity and share equally stringent ethical principles. Perhaps, it would be appropriate to close with another Eastern wisdom that may illuminate this ends-means discussion.

A clever man liked the beautiful lamp of his neighbour too much. So the goal was set to get it and, surely he had a plan. One fine evening he asked his neighbour to lend the lamp to him. His prayer was granted and he promptly returned the lamp the following day. After a few days, he asked to borrow it again. No problem. This time he bought a little lamp and put it inside the big one and came to his neighbour. "What is this", the neighbour asked, pointing to the little lamp inside. "Well, your lamp delivered a baby", replied the clever man. The neighbour was perplexed, but said nothing. After a few weeks, he came back to borrow the lamp one more time. Days and weeks went by. The neighbour got concerned and one day cornered the man. "Where is my lamp"?, was his polite question. "I am sad to say that your lamp suddenly died", the man replied. "What, how the hell a lamp can die"?, the shocked neighbour asked. "The same way that it could give birth to a baby", came the reply.

The Future of Focus Groups

There is a strong consensus that focus group techniques are going to be increasingly used in social science research (Morgan,1993; Strauss and Corbin,1990; Tesch,1990; Stewart and Shamdasani,1990). It is also likely to be increasingly used in health research, particularly in the area of mental health as qualitative approaches gain significance. In health research, the emphasis is shifting away from classical epidemiological concepts of disease and risk analysis to illness and life chances. A disease afflicts an individual which needs to be cured; an illness affects not only the individual, but

also the family and the community and the experience is grounded in culture which needs to be understood. The interface between human behaviour, culture, and life chances is taking the central stage in understanding health and well-being. The recently released Harvard Medical School report on World Mental Health (1995) further underscores the point. Increasingly the issue of health reform is coming to the forefront and the question of equity is ecoming the core policy concern (World Bank 1993; PAHO 1993). And health reform cannot be achieved without participatory research, strategic interventions and community involvement and participation. It is only through working in and with communities and people that equitable health reform can be accomplished. The process is painstaking, but there is not much of an alternative as a sufi points out:

A man found the secret teacher Khidr working as a ferryman. Khidr read his thoughts, and said to him:

"If I approach people in the street and tell them what to do, they will think I am mad, or am doing it for myself, and they will not do it. If I dress like a learned or rich man, and advise them, they will disobey or simply try to please me, instead of trying to please that which I represent. But if I mix with the people and say a word here and a word there, some will listen, and try to do the things as best as they can".

The need for qualitative research, along with quantitative ones, is, therefore, sure to increase. Undoubtedly, focus group technique will remain one of the most frequently and profitably used tools of qualitative research.

Epilogue

This paper only tries to explain, in a brief manner, the problems and potentials of focus groups as a qualitative research tool. For more extensive explanations, one must resort to references listed. The goal was to instill a sense of inquisitiveness among its readers.

Someone said to Bahauddin Naqshband: "You relate stories, but you do not tell us how to understand them. " He said:

"How would you like it if the man from whom you bought fruit consumed it before your eyes, leaving you only the skin?".

Discussion Session

MODERATOR: VEENA DAS

- *My question is about the number of the persons in the focus group. How small or how large can a focus group be?*

  There is no hard and fast rule, though we usually say that the ideal size is between 8 and 12.

- *We must use various techniques to focus on the same topic, and focus groups is one of those. But it is particularly useful for cultures like India, where each person can have a different opinion. I find it very useful to get people together so that opposite views are aired, and after a discussion, either some consensus is reached or one view overpowers the other. I was using focus group technique with a group of traditional healers and their views about why epilepsy emerges.*

- *Another group where this technique is very useful is with children. In a one-to-one situation, the child often feels very inhibited. But I find that in a group situation, children come up with very sophisticated answers and views.*

- *The third point I want to make is that you do require some very special skills to be able to find out the underplay of relationship in the group. A senstivity to who is the leader and whether people are agreeing to that person just because of his/her power is needed. I have found it useful to have at least two people observing the discussion, so that they can compare notes.*

  Finally, whether you want it or not, you will end up making some kind of change in the group after the focus group is over, and one has to be prepared for that.

- *Who decides the composition of the groups and on what basis? In a place like India,there is a great deal of group affiliations between people. How are these dealt with in selecting participants?*

  To answer the first question, who decides about the participants, the entire research team should decide. We as researchers cannot be biased. The research topic will dictate which participants to choose, and the researcher must be professional enough to select the right person.

- *I have a question about the role of the moderator. Should the moderator be very directive? Should he encourage interaction between participants? Should he encourage all members of the group to speak, or is it enough that only some people speak? Should group dynamics be taken into account when the qualitative analysis is done?*

  The role of the moderator, the moderator cannot be seen to be leading the discussion in a certain way, but at the same time he/she should keep the discussion focussed. The moderator should be a supportive leader - should provide support to the group, not dictate the terms, not direct them towards things that you hear, but at the same time, keep them focussed and interested on the topic.

- *I have some experience in running focus groups in a health setting, and I am all for them. But we found that there is a lot of reluctance on the part of the patients to participate in a group setting. There was also a tendency to give socially appropriate answers. We found that one way of combatting this was to get to know the patient very well beforehand. We did in-depth interviews with the person so as to give him confidence. We also found that if the person knew the members of the group very well, the interaction was much better. We read in books, particularly from western settings, that the less you know of the person, the more communicative they are. But our experience was the opposite. We used the same group two or three times, and we could see a distinct change in the way they were communicating as they became closer and friendlier with each other.*

  Interaction depends on the context. In Philippines, there was a study to evaluate how the health workers in the village and district levels perceived the new changes introduced by the very dynamic secretary of the health department. When the discussions were conducted, the participants all knew each other. But there were absolutely no dissenting views expressed, because the secretary himself was there as one of the participants. There was such a power gap among the participants that a meaningful interaction was not possible. Just because they were known to each other does not mean that you will have a good interaction. Another important thing is the subject of the discussion. If you are discussing

human sexuality, for example, you may find that people will open up more if they are not known to each other.

- *Normally, focus groups should be done in the setting where the people actually live, and not in a hospital or other such setting. But supposing we go to the slums, for instance, there is always the leader, whom we cannot ask to move away. There is often no privacy. Also, at times when the focus group is over, certain people might want to say something significant outside the group. It becomes a seperate interview. How do we deal with such a situation, where an indepth interview follows or precedes the focus group?*

- *A lot of funding agencies have rapid appraisal methods where they put together focus groups and discuss them. From an anthropological point of view, these discussions appear to be short term alternatives to long-term serious research. Two of my students have gone to live in areas where they have found that enormous money is poured by international organisations into focus groups so as to "consult" people on these matters. But after the discussions are over, there is enormous distortion in the way in which things are presentented. So I would put it to you that while you might get what appears to be very useful information, there is a politics of international agencies over here, because they need to show that people have been consulted. I am not saying this happens always - I have seen some very good focus group studies. But I am wondering how much of the modern politics of knowledge is actually not responsible for creating this simulacra that people have been consulted and that focus group techniques are going into something which is becoming very managerial.*

That is why I had a topic called Focus Groups: Some Common Myths, which addresses some of these questions. But we did not have time to go into that. I have already said that the focus group should not be the end of a research. It is a powerful way of designing your research. It is a myth that a focus group means that you have taken the pulse of the community. You cannot generalise from a focus group. In IDRC we try to monitor these things and we do not impose any research techniques on the researchers. But we do emphasise that they have dissemination strategies - an inclusion strategy. That is, they do some sort of group discussion before they do the study so that they have some

input in terms of the questionaire, the research design, sample size, etc, from a peer group or others who are interested, and in the end, they bring the results to the same group before they are put in a final report.

- *You said that focus groups are not to be used to resolve conflict. When one is carrying on the focus group process, there is probability of conflict coming in. In such a situation, what do you advise - should one stop the focus group, or go ahead with it?*

The purpose of a focus group is not to resolve conflicts. You may end up antagonising the whole group if you try to do that.

- *What would you advocate as the best time and manner of recording the information collected from focus groups?*

I am very sensitive to individual rights and ethics. If you want to record, you have to take permission from each individual present. Audio taping is the best method. You can compare it with your notes and even get some of the participants to talk about the local meanings that emerge. If I am in a focus group, I will personally never approve taking photographs or video tapes - I consider that very intrusive.

- *I have a question regarding the selection of the target audience. Can the causative agent and the affected audience both be taken as the target audience in the same research?*

Yes, it can be done, but at the same time you have to make a judgement whether it is the best technique to follow in that situation.

I would also like to say that I do not see focus groups as the method of research. You cannot do a research based only on focus group discussions. Focus group disscussion is a tool that you may use to enlighten your information and then interpret that on the basis of other information that you collect, such as quantitative information.

Anthropologist in the Field of Mental Health: An Agenda for Qualitative Research

GILLES BIBEAU

Introduction

Any attempt to map the various theoretical and methodological regions of a field as elusive as anthropology and to portray its most significant endeavours in mental health research activities is bound to be somewhat subjective. It depends to a large extent on the writer's own idea of anthropology, his personal commitment to a particular epistemology, and above all on his experience as a researcher in mental health. There certainly exist ways of reducing biases and avoiding omissions and I have in fact benefited from corrective inputs provided by colleagues. But my text is neither a photographic account of all relevant trends in medico-psychiatric anthropology nor an overview of research topics and methods. My appreciation remains intentionally subjective and I voluntarily restrict my considerations to a series of research projects which are very familiar to me: I use them as case studies that serve to illustrate the different ways anthropology can be used in mental health research.

Before entering into the heart of the matter, the reader has to be introduced to anthropology, a discipline which has its own problems of identity as any other discipline. It is useless to try to

offer, in a nutshell, the key elements which are generally covered in introductory courses to anthropology. That sort of instantaneous shot always distorts reality. I thought it a better option to raise two of the most challenging debates in which anthropologists are presently engaged: the public image of the discipline may suffer but researchers who want to become more anthropologically-sensitive will gain from this excursion behind the curtains.

Anthropology between Proximity and Distance

Roger Keesing once warned young anthropologists that "the more we know, the harder it may be to reduce rich cultural details into holes in punch cards". In those days we were still using the first generation of computerised programs for qualitative analysis but Keesing's warning still goes on challenging junior researchers entering the field of medical anthropology in this age of sophisticated high technology. During the last two decades since Keesing's warning, the discipline of anthropology, particularly cultural anthropology, has been undergoing what might be described as a crisis or paradigm shift. Statistical correlations and inferences seem to be far from the minds and hearts of the many new ethnographers who have been influenced by interpretive and hermeneutic philosophers, by post-structuralist semiologists of different vintages, and by recent promoters of literary criticism.

My daily work as supervisor of graduate students in anthropology shows me repeatedly how deeply young anthropologists think that the practice of their profession should involve more commitment to people they "study", more authenticity in what they write about them, less self-deception and less remoteness from the lived experience. Looking at the book shelves of many anthropology graduate students one should not be surprised to discover many novels and literary works that lay side by side with old enthnographic classics. Students who recently entered the discipline have been led to explore literature and drama by prestigious authors such as Clifford Geertz who define culture as "an assemblage of texts" and who therefore transforms ethnography into "an exercise of reading". Such a position still echoes in a disguised idiom the old method of ethnography which has always combined, in a paradoxical fashion, participation and observation, proximity and distance.

Young anthropologists aspire at scientificity in their practice but do not want to eliminate the ethnographer as subject from his or her ethnography; they still fight for reliability and validity in the anthropological accounts but in placing them in a frame that does not erase the plurality of meanings or silence any one of the multiple voices. Such an in-between position creates some uncertainty but the territory covered by ethnography, a fuzzy-bounded territory that touches paradoxically on science and humanities, on objectivity and subjectivity, on factual description and interpretation.

Today's anthropologists experience discomfort (and sometimes anxiety) in their effort to balance these two contrasting views about the same unique ethnographic reality they try to understand. Many find a refuge in schools of thought that criticise objectivism, and often rally to phenomenologically-minded scholars whose ideas about being-in-world reinforce the stress placed by semiological anthropologists on subjectivity, experience and interpretation. Others prefer to solve their problem by espousing the positivist paradigm which has still a strong voice in general anthropology. These two extreme solutions lead to pitfalls and dead-ends that may disfigure the whole anthropological enterprise which has to incorporate elements that belong to both objectivist and subjectivist frameworks. The anthropologist ends up standing on the boundaries and straddling - without often grasping with a firm hand - the worlds of science and humanities.

Social and cultural anthropologists have not all situated themselves resolutely in the anti-positivist, anti-quantitative camp; many still stick to community data collected through survey census and records, figures, measurements and statistics that they store in data banks and analyse with different sorts of quantitatively-geared programs. But the majority of anthropologists have, in these recent times, honed unique qualitative interpretive skills, borrowed from hermeneutics, literary criticism and the humanities. In this era of increasing reliance on computers, the collection of information, both qualitative and quantitative, tends to be coded and transformed in data bases that the technical specialists (the high priests of the late twentieth century) use to measure correlations, to do cross tabulations, factor and cluster analyses, in order to express results into the dominant scientific idiom. But data bases are also

seen as texts by anthropologists who read them in the same way as they read documents and books. The texts ethnographers have worked with and speak of were most often, in the recent past, discourses, narratives, tales and myths which represented the living world of the people among whom the ethnographer has lived and worked. One may find some nostalgia of the village square conversations in the fact that anthropologists have regarded data bases as texts that really speak with their own voice about the people. The metaphor of culture as text ends up here with the data bases, the narrative folklore of this finishing century, which are promoted to the status of textuality.

From reading what precedes many may think that anthropology has been invaded by the textual metaphor and that most ethnographers walk in the footsteps of semioticians and postmodern hermeneuts. There is no doubt that a new intellectual ethos has infiltrated the anthropological world, and even the humanities and social sciences in general, and that no discipline has escaped the hermeneutic turn of the eighties. The Italian philosopher Gianni Vattimo has pictured the on-going shift in candid terms: "a common idiom impregnates the whole thinking in the eighties, and this idiom is that of hermeneutics while in the past decades there was an hegemony of Marxism in the fifties and sixties, and of structuralism in the seventies" (Vattimo,1987).

But this new intellectual atmosphere has not led to a total displacement of Marxist-inspired critical approaches which still continue in anthropology and above all in medical anthropology. Critical medical anthropologists are explicitly aware of the importance of the macro-sociological forces at work in the semiotic ordering of reality, of the impact of the historical context on cultural values, and of the ways the structures of power and control underlie the daily life of people. The studies of contextual, historical and practical elements associated with the genesis and distribution of health problems are widely found in contemporary medical anthropology but most researchers fail to adequately identify the mediating categories that interconnect the macro-social context with the cultural codes and with the concrete behaviour of individuals. Critical medical anthropology misses dramatically the reference to a comprehensive conceptual framework that bridges the gaps between the different levels of reality. Except during the short period of hegemony of neo-

Marxist thinking in the European social sciences anthropology have never been, neither yesterday nor today, in a position to provide an explicit comprehensive framework. The accent that researchers put on context and history serves at least as a counterweight to more static semiological approaches.

Ethnographic Authority: Inside and Outside Voices

Everybody expects from the books of ethnographers the experience of an encounter with otherness which is based on the refusal to reduce difference to sameness. Travels across intellectual provinces and visits to other cultural families, either near-by or far-abroad, are professionally more natural to anthropology than to other disciplines. By trade, the ethnographer decenters the recurrent ethnocentric discourse of Western science and replaces it by the direct experience of an encounter with foreign forms to thinking, feeling and behaving through the practical sharing of daily life in other settings which the ethnographer translates into concepts and interpretations theoretically accessible to, and acceptable for, Westerners as well as Asians, Africans or Latin Americans. Ethnography cannot be limited only to the practical experience "out-there". It equally consists in the process of making intelligible otherness by unveiling its universal validity. Such a disclosure is necessarily done through the mediation of methods, concepts and theories developed by general anthropology and the different schools of thought within various national intellectual traditions.

Interpretation and translation of otherness do not mean that the ethnographer abandons his or her community of thinking and language suspending the question of truth, superiority or universality of his or her cultural presuppositions and rendering otherness only in its own indigenous and local terms. If the other is a phenomenon for the anthropologist, the anthropologist is equally a phenomenon for the other, but neither of them has to renounce the role of subject and content him or herself with being a mere object for the partner. Anthropologists think commonly that co-belonging and dialogue can constitute the adequate frame for a valid interpretation and translation of the other, of his/her language and culture into the language and culture (and sameness) of the interpreter. Such a translational

work may be quite easy when someone works in a society which shares much with his or her own culture but the reliance on dialogue may be insufficient in the case of anthropologists who are concerned by their encounter with people of foreign cultures.

The approach based on dialogue forgets generally the tragic dimensions of the ethnographic encounter: the two cultures to which the anthropologist and the other belong respectively are not only different, they are also in many cases hostile; the ethnographer, whoever he/she might be - an American, an Indian or a Chinese - represents in most cases a society which directly or indirectly 'mines' and destroys the very societies he wants to understand. No individual empathy, even the most sensitive, can annul this hegemonic position and produce a balanced egalitarian dialogue. The decolonisation process of the sixties has dramatically brought to the forefront of European intellectual debates the problem of the violence and hegemonic position of Europe in her relations with colonial countries and nobody has more accurately described the 'colonial mentality' than the psychiatrist Frantz Fanon (1961). Debates of similar nature concerning the hegemonic position of American science, including anthropology, have long been delayed in United States where the legitimacy of the dominant relations with other countries was mainly discussed in the late sixties and early seventies in relation mainly to the Vietnam war.

Two American anthropologists have recently echoed this old debate, reactualising it in relation to the problem of the validity of contemporary ethnographic discourse. "I began", writes Clifford, "to see such questions as symptoms of a pervasive postcolonial crisis of ethnographic authority. While the crisis has been felt most strongly by formerly hegemonic Western discourses, the questions it raises are of global significance. Who has the authority to speak for a group's identity or authenticity?" (Clifford,1988). Clifford reminds anthropologists that most of them are still citizens of hegemonic states and that it is not out-dated to question the basis of their authority to go on speaking for others. He states in adequate terms the question of the appropriation by foreigners of the culture of others.

Another American anthropologist, Clifford Geertz, has restated in new terms this problem of the political context in which the

anthropological work is performed. In a recent book, he reframes the problem of the contemporary ethnographic authority in contrasting the two sides of the anthropological work and life: the "being there" of the field and the "being here" of the writing of ethnography. Geertz (1988) writes:

"The ends of colonialism altered radically the nature of the social relationship between those who ask and look and those who are asked and looked at. (...) At the same time as the moral foundations have been shaken by the decolonisation on the Being There side, its epistemological foundations have been shaken the Being Here side (...) Whether the period immediately ahead leads to a renewal of the discursive energies of anthropology or to their dissipation (...) depends on whether the field can adjust itself to a situation in which its goals, its relevance, its motives, and its procedures all are questioned (...) There are dangers in regarding the anthropological vocation as in important respects a literary one".

Specifically the entrance of "decolonialised" intellectuals into the international academic life and more generally the New World geopolitics create a radically new context for any discussion of authoritative writer and more generally about the contents and style of what anthropologists write concerning other cultures.

Local experts and voices of the inside speak more and more loudly in the name of the cultures to which they belong. The work of the Palestinian-born E.Said (1978) on the critique of the Western discourse about the "Orient" has greatly disturbed a lot of quietly-installed social scientists. Mudimbe, a Zairian working in the United States, has recently produced a similar deep critique of "Africanist discourse"; Mudimbe (1988) stresses the fact that: "Western interpreters as well as African analysts have been using categories and conceptual systems which depend on a Western epistemological order". But like Said, Mudimbe cannot escape the trap: both criticise Orientalism and Africanism in using the categories developed by Western intellectuals, Mudimbe relying strongly on Foucault and French thinkers, and Said more on a combination of interpretive and critical Anglo-Saxon writers. These two brilliant scholars stand in-between two intellectual worlds that they master perfectly but in order for their critique of Western discourses to be understood, they have no other choice

than challenging Africanists and Orientalists on their own grounds in speaking their language and using their concepts.

To what extent might they have criticised the dominant Western academic knowledge by using the endogenous systems of thought and historical traditions of their own country? They have both referred to the prevalent Western intellectual configurations not because they are highly Westernised, or because they refuse to Africanise and Orientalise their critique in the terms of their own native culture, or because the non-Western Weltanschauungen are not translatable in concepts and theories understandable by Westerners. Mudimbe says clearly why he has used the Western epistemological framework for criticising Africanism: "My own claim is that thus far the ways in which African Weltanschauungen have been evaluated and the means used to explain them related to theories and methods whose constraints, rules, and systems of operation suppose a non-African epistemological locus" (Mudimbe,1988). In other words, the intellectual locus of control lies outside Africa, says Mudimbe.

Two conclusions emerge from the preceding short discussion of ethnographic authority in its relations to others. First, it has became evident that all authors are forced to express their ideas within the Western paradigms themselves, even when they are non-Western scholars who criticise the Western intellectual productions. We also noted, however, that non-Western professionals of social sciences and humanities have radicalised the critique of the "discourse about otherness" that exists and the internal differentiation within this discourse, a differentiation which largely follows historical and national divisions. This kind of critique will probably help sensitise intellectuals to the diversity of the national cultural configuration within academia and to the historical grounding of their own national intellectual traditions.

Psychiatric Epidemiology and Anthropology: Conflict and Cooperation

Contemporary psychiatric epidemiology appears to be increasingly marked by the problem of the standardisation of instruments used in diagnostic evaluation and in population research dealing with the mental health problems. This growing

standardisation of psychiatric classifications and diagnostic procedures now seems to be the norm in the clinical practice of psychiatry and is influencing what happens in the domain of psychiatric epidemiology and more globally, in mental health research. In fact, following clinicians for whom the question of the reliability of diagnostic techniques has become a central concern, (most likely because of its potential link with the prescription of more specific neuroleptics) psychiatric epidemiologists seek to incorporate more discriminating diagnostic criteria into their research techniques. These criteria theoretically allow them to identify, from a few brief questions and with relative (un)certainty, people who suffer from one or another problem of mental health. They also serve to measure the distribution of different categories of problems in subgroups of the general population (female/male, young adults/aged people, people with higher/lower education).

Westermeyer (1989) recently pictured the present state of psychiatric epidemiology: "Epidemiological studies", he wrote, "are increasingly dependent on rating scales, either self-rated or interviewer-rated (by a trained technician or by a psychiatrist, psychologist, psychiatric nurse, or psychiatric social worker). This research procedure fails to mimic the standard diagnostic procedures in psychiatric practice". While constructing their scales to measure symptoms and diagnostic grids, psychiatric epidemiologists have in fact taken as models the same methods as those employed by clinicians for establishing individual diagnosis. They nevertheless realise that short questionnaires rapidly administered can never lead to a diagnosis as precise as one carried out by a psychiatrist who meets several times with one patient. It is largely the mimicking of clinical practice that has reshaped the recent trend in epidemiological psychiatry and more fundamentally it is the question of reliability and validity that inspired the new research procedures.

With the help of epidemiological research we now know more about the mental health problems of different populations, and which types of problems are predominant. We can imagine that the standardised questionnaires and interview grids designed for a particular cultural group (usually a dominant Western group), do not always function effectively to adequately portray the pathological problems of that group. But limitations and

inaccuracies are increased when these same instruments are used to measure levels of mental health in societies where we find other visions of the world, other conceptions of the self, and an overall different way of identifying, explaining and reacting to psychiatric troubles. The grounding of these instruments in Occidental culture and their common use in psychiatric epidemiological research has led anthropologists to create, test and use instruments that are supposedly cross-culturally sensitive and valid (Kleinman,1987; Bebbington,1980; Flaherty *et al.*1988).

During the past years, the methods and instruments used in comparative international studies on schizophrenia have spurred anthropologists to reflect on this subject. In the 1970's an international study on schizophrenia was carried out in eight countries: in India, Nigeria and Columbia for those countries considered to be developing countries, and in Denmark, the United States, Great Britain, the Soviet Union, and Czechoslovakia for countries considered to be developed. In this study, the same standardised questionnaires and diagnostic procedures were used in the eight countries, whatever their culture. Although these instruments were largely non-sensitive to intercultural variation, one major result of this comparative study has intrigued particularly both psychiatrists and anthropologists. A greater proportion of patients considered schizophrenic in developing countries (India, Nigeria and Colombia) presented a better prognosis than those in the five developed countries, and these same patients were less chronic and awwere more often found to be at a relatively better level of functioning. This astonishing observation was eventually explained as being the result of an inadequate evaluation of the local forms through which people expressed their pathological states, and by the socio-cultural inability of the instruments and methods to grasp these local forms. The results of this pioneer study also showed that the prognosis of recuperation appeared to be much more favourable for schizophrenics in developing countries. Anthropologists felt more and more curious to know the source of these particular phenomena. No ethnographic data, it seemed, could provide answers as the authors of the study have noted, "a large part of the variance in the course and outcome of schizophrenia may be due to factors that have not yet been identified" (Sartorius *et al.* 1978). In fact, because the researchers had not systematically studied social and cultural variables in the course of schizophrenia,

they could offer only vague hypotheses related to socio-cultural factors in order to explain the variation between the results of different countries.

The research team of Dr. Sartorius carried out a complementary study entitled "The Determinants of Outcome of Severe Mental Disorders Research Project" in which the degree of the cultural sensitivity of the methods used was improved. This team also sought to test specific hypotheses concerning socio-cultural factors that were potentially implicated in the differential evolutionary patterns of schizophrenia. In this study, however, the researchers once again limited the scope of their research to factors such as life-events and expressed emotions which are known to influence the course of schizophrenia in Occidental societies.

The instruments used to measure these factors were created in each country with the collaboration of local researchers who evaluated the cross-cultural validity of the list of signs and symptoms considered to be indicative of a mental health problem. Nevertheless, even with the collaboration of local researchers the directors of the project had to admit that in the three developing countries where research was carried out, "the instruments never achieved the same comfortable, common sense quality reported in the developing countries" (Sartorius *et al.* 1986).

After a careful examination of the epidemiological instruments used in international studies on schizophrenia, Corin (in press) notes the urgent need for anthropological studies in this domain:

"This body of related research on culture and schizophrenia leads to two main conclusions. First, the utilisation of standardised and reliable methods of data collection has reinforced the interest of comparative studies on schizophrenia; they have shown in evidencing the possibility to document in various societies the presence of closely comparable forms of disorders and they have confirmed the existence of cross-cultural variation in course, that cannot be explained by classical socio-demographic and clinical predictive variables. Second, these studies also demonstrate the interest and the limits associated with classical epidemiology dressings; they help to identify the existence of significant variation which also concerns the significance to be attributed to the variables themselves; they also indicate the necessity for

more global and culturally sensitive studies in order to understand how the very experience of the illness is socially and culturally shaped at the level of patients, of families and of communities".

The Need for a Double Strategy of Research

Specialists in "comparative psychiatry" have recently become more aware of the necessity of carrying out in-depth ethnographic research in order to enrich their epidemiological studies. Such research should enable them to better understand and to identify with greater certainty social and cultural elements that might explain, for example, why a problem like schizophrenia may evolve in different ways from one society to the next. For their part, anthropologists have documented the intercultural variation in the area of the semiological, nosological and etiological systems used by different groups in their management of mental health problems.

Today many epidemiologists are aware of the benefits of incorporating at least some anthropological research into their investigations. The use of basic and general ethnography is, however, in the eyes of many anthropologists, insufficient if one wants to create an in-depth, cross-cultural and comparative epidemiology. According to psychiatrist-anthropologists such as Kleinman (1986), Kirmayer (1989), Fabrega (1989) and Uchoa (1993), it is only with an in-depth knowledge of local ethnography that one can understand the ways in which particular cultures shape the systems of meanings from which people construct their experience of an illness, describe their symptoms and react to their suffering.

Regarding the content of these ethnographies, anthropologists as well as psychiatrists and epidemiologists trained in anthropology are divided into two main groups. There are those who, in line with the anthropological-psychiatric research team formed by Dr.Alexander H. Leighton (1959; 1963) in the 1950's, believe that the study of social organisation (family, parental and interpersonal relations) and the impact of its structure on the evolution of mental health problems should be the central concern. Other researchers with more or less culturalist orientations believe that it is more important or at least just as important to examine

systems of signification, values and representations in the sense that these factors provide the essential framework from which people identify, explain and manage their mental health problems. Ultimately it is the cross-cultural variation of these systems that interest these researchers whereas the first group is more concerned with different modes of social and family organisation and with the ways such modes are related to problems of mental health.

The traditional tension between social and cultural anthropologies and the danger of one tradition excluding the other have been discussed ad nauseam. A focus exclusively put either on the social or on the cultural dimensions inevitably leads to the creation of artificial barriers that can only be detrimental for research. On the one hand, the first group tends to look at psychiatry from a social and community-oriented point of view and to construct a social and psychiatric epidemiology in line with the one created more that forty years ago by the psychiatrist A. H. Leighton and anthropologist Dorothea C. Leighton (and later pursued with the anthropologist Jane H. Murphy). On the other hand, the second group, which since the 1970's has been associated with, among other, Kleinman and Goods in the United States and with Littlewood and Lipsedge in Great Britain, has examined psychiatry in light of cultural values and symbolic systems and has studied the impact of these factors on the construction of pathological phenomena. These two perspectives are in the midst of diverging more and more on both theoretical and methodological levels.

Possible Pitfalls of Decontextualised Culturalist Approaches

The reduction of culture to semiological and phenomenological dimensions risks leading certain anthropologists towards the exclusive study of subjective experiences of ill people and more precisely towards an analysis of their discourse or their narratives and personal feelings. In some cases these subjective experiences have been analysed out of the cultural context as if the personal experience and idiom no longer needed to be understood with reference to an ethnographic analysis and within the framework of cultural values. One gets the impression that certain phenomenological anthropologists have come to consider

"culture" as a given accessory and as an independent artifact whose only role is to give shape to subjective experience which constitutes, for these anthropologists, the only real ethnographic material that one should analyse. We know that subjective experiences and the inside world of people are always given form by representations provided by one's culture about the person, about subjectivity, about the body, the world, and about life, and that all of these representations contribute to create the meaning given to illness by an individual person. Even more problematic is the reification of illness narratives that are read increasingly by certain anthropologists as if they are autonomous texts or, at best, simply representative accounts of the inner dynamics of people. Sophisticated methods of semiological analysis are applied with good intentions to these texts but this is often done without any general cultural background or understanding of the significant elements which form the context of these narratives. This no doubt explains why certain anthropologists have leaned toward literary analysis and have become increasingly interested in style, rhetoric and fiction.

The massive influx of semiological and phenomenological approaches in cultural anthropology has nevertheless made way for a rich contribution by anthropologists to the development of cultural psychiatry. Anthropologists refuse more and more to limit themselves to the simple identification of socio-cultural factors associated with the causes and courses of psychological disorders and to the comparison of these factors across different cultures. The new focus is put on the careful analysis of individual cases, on the personal narrative of those people and on the exploration of their subjective experiences. With these methods phenomenological and interpretive anthropology have actually shed new light on cultural psychiatry.

Authentic phenomenology seeks to understand the subjective experiences of people within the context of their socio-cultural background, whether a person is a Bambara villager from Mali, a Buddhist Tamil from the south of Madras or a Woody Allen-style New Yorker. The new phenomenological-semiological approach used in anthropology can contribute to cultural psychiatry in the sense that it examines the relationship between the overall culture, systems of signification, interpersonal relationships and subjective experiences of people.

Anthropologists may devote themselves to the study of the private language of ill people and to their specific experience but once again, as Good (1994) has just reminded us, one must not remove those people from the context of their social surroundings from which their personal lives are given meaning or separate their inner worlds from that which is collectively imposed on them whether they accept or reject this. Along the same line, M.Pandolfi (1991) has shown how contemporary women of the Italian Mezzogiorno continue to use a "narrative grammar" whose rules are still followed when they express emotions, the effects of these emotions and their psychopathological problems.

Whether we prefer to reexamine the psychiatric tradition from a social anthropological perspective along the lines, for example, of the work of Dr. Leighton's research team, or whether we prefer the culturalist approaches which is particularly phenomenological and interpretive in its present dominant vintage, the issue is not one of simple gathering superficial information about "the others" and then trying to adjust standardised instruments to specific cultural and social conditions. This has already been done by certain progressive epidemiologists and what is now at stake is much more fundamental because it radically challenges the established relations between anthropology and psychiatry.

Two Different Uses of Anthropology

In the following pages I will examine the differences and similarities, and the advantages and limits, of two research projects that I have chosen not because they are the best ones, but rather because they illustrate well two complementary research strategies. I will first look at the Stirling County Study which began in 1948 in a maritime county of Nova Scotia, Canada, and which continues till today under the leadership of Dr. Alex Leighton who himself initiated the study forty-five years ago. The second study which was carried out (1986-1990) in Abitibi, a peripheral region of Quebec, Canada, is not at all of the same magnitude but I have chosen it because the method used in this study is actually applied in an international study known as INECOM which I will elaborate upon in the closing section of the text. The Stirling and Abitibi projects will serve to examine, in a concrete manner, the different problems related to the

methodological techniques used by anthropologists, and in a more general way to question the contribution of anthropology to new approaches in social and cultural psychiatry.

In both projects, monographs of villages (of communities, write the authors of both groups) helped to illustrate the socio-cultural dynamics and to identify dominant forms of social organisation as well as the central configurations of normative systems existing in these two communities. In the Stirling county of the 1950's, people were still devoted to fishing, wood-cutting and agriculture and the Quebecois population of Abitibi are mainly dependent on mining, forest work and agriculture. Fifty percent of the twenty thousand inhabitants of the Stirling County were Acadian francophones and the remainder were anglophones of British descent. Linguistically mixed villages appeared to be the norm and the socioeconomic differences between the anglophones and francophones did not seem significant. A very strong majority of the one hundred thousand inhabitants of Abitibi, who are more than ninety percent francophone, live in two main cities where the socioeconomic level of the inhabitants appeared to be highly contrasted with those living in the cities The two regions where these studies were carried out, in Nova Scotia and in Quebec, are illustrative of typical Canadian situations. The researchers in Stirling County were confronted with the linguistic dualism of Canada and with the coexistence of mixed but linguistically homogeneous villages. In Abitibi one finds another typically Canadian trait, that being "northerners". In this region human settlements are established only on cultivable land and ecological conditions are such that people are forced to live off the natural resources provided by mining and agriculture.

If the main goal in Stirling County was to work with representative samples in order to measure the prevalence of cases, on the basis of nosographical grids used in psychiatry, researchers in Abitibi were more concerned with identifying the ways that popular semiology functioned as well as how the people of that region perceived common psychiatric problems. The difference in perspective led the Abitibi researchers to assess the "salience" of the more common and obvious problems whereas the team of Dr. Leighton assessed the prevalence of more specific incidents. In the Sterling case the point of view of professionals and psychiatric diagnoses is dominant while the focus is put, in Abitibi, on the

popular system of signs and meanings as well as on the way people interpret and react to the behaviour problems of their family, neighbours or friends. In Abitibi the researchers sought to identify the central features of the systems of signs, meaning, and actions of the population by way of a careful examination of specific pathological cases and by the reconstruction of those cases by key informants. This method was far from the professional analysis of clinical cases done in Stirling County.

In addition, I think it is important to compare the theoretical and methodological frameworks of the two research teams. In their work, the anthropologists of the Stirling County Study refer to and are guided by, a functionalist theory, and are specifically interested in the processes of "disfunctioning" of normative systems and social life. The six studies of communities in Abitibi were not limited to an examination of social organisation or disorganisation although these factors were considered to be of central importance. Using an interpretive perspective, the Abitibi researchers wanted to identify the cultural dynamics related to what they called the openness or closeness and the autonomy and dependence of the communities. The approach used by Abitibi researchers sought to integrate the best of the social anthropology practiced in the Stirling County monographs as well as to add historic and contextual research on the formation of the regional subcultures of Abitibi.

The contrast between the functionalist and interpretive approaches in the realisation of ethnological monographs leads us to stress an important difference between the Stirling and Abitibi Sudies. This difference has to do with the social etiology of psychiatric problems, a concern that is central to Alexander Leighton (1991):

> "The fact remains that the effect of cultural differences on mental illness rates does not stand out nearly so strongly as does the malfunctioning of social institutions. High rates of mental illness have been found to be markedly associated with poverty, social deprivation and social disintegration. (...) Unfortunately, it is proving extremely difficult to tell in which direction the causal vectors flow? Is the association that epidemiology reveals, the net effect of numerous small vicious circles in which particular kinds of mental disorders are linked to particular kinds of social malfunctions in such a way as to mutually reinforce each other".

In the Abitibi study researchers also tried to improve their understanding of how particular social and cultural configurations worked together to create what they called "pathogenic structural conditions" which varied from one community to the next and which no doubt contributed to explain why problems varied from one subculture to the next and from one community to another. The etiological perspective, however, is only outlined in the Abitibi research which is dominated more by the study of the construction of systems of signs, meaning and actions from an interpretive rather than an etiological perspective.

Finally, the ways in which data was analysed in these two projects can certainly be explained by differences in the nature of the data but these differences must also be accounted for by the particularities of the two theoretical frameworks. In both studies ethnographic data was analysed in the usual anthropological manner but otherwise the methods used were different. In the pioneer study by Leighton, whose central concern was etiological, researchers worked with two main categories of data. The first, which became increasingly important, was statistic in nature and dealt with the distribution of different categories of problems within representative samples of the population. The second was ethnographic in nature and this data was collected on a community level as it was done in Abitibi. In the Stirling study the analysis dealt particularly with the correlation between social variables and the rates of occurrence of psychiatric problems in order to highlight the respective influence of the different categories of social variables.

In the Abitibi project the focus of analysis was rather on the approximately 320 illness narratives provided by key informants who were given 14 behavioral descriptions to refer to. The narratives were first analysed, milieu by milieu and behavioral description by behavioural description with the goal of identifying major themes throughout the different groups of narratives. The analysis therefore allowed the researchers to demonstrate that in each milieu - mining, forest-work and agriculture, there seemed to be different semantic configurations, forming their own particular patterns and webs. The next step was then to

understand why these semantic webs were formed in one milieu around certain configurations while in another they were different. The analysis of the links between the content of the narratives and the ethnographic data led the researchers to discover, or better to guess, that the same logical structure connected the socio- cultural order, social dynamics, systems of signs, meaning, and behaviour and individual experiences.

CASE 1: THE STIRLING COUNTY STUDY

On the conceptual and methodological levels, the Stirling research team sought to understand what caused certain people to live in two worlds and to feel torn between conflicting normative systems without being able to situate themselves in one place. "Broken cultures" rather than cultures that are perfectly integrated have become central for this research team and it is more the study of order/disorder than the functional integration of social life that their research seeks to examine. This double interest, on the one hand for social and cultural disintegration and on the other for the study of the impact of "socio-cultural disorders" on mental health has led the anthropologists from Leighton's team to specify more and more precisely the indicators or markers from which one can determine the levels of social disorganisation and cultural disintegration in a particular community. Ten indicators have been taken into consideration among which are the existence of weak leaders, the number of broken homes, the rate of unemployment, the level of participation in social events (marriages, funerals).

One key question, situated at the junction between medical anthropology, psychiatric epidemiology and clinical psychiatry, guided the Sterling researchers in their collection of data. In what way do certain social, economic and cultural factors contribute to generate problems of mental health for certain people, influence their evolution, and eventually help their resolution? Researchers tried to answer this question by gathering three types of data: ethnographic data which was gathered in a sample of a few village communities on the basis of classical ethnographic methods (participation observation, interviews with key informants using a very precise grid, and the systematic collection of all kinds of statistical data related to the areas studied); epidemiological data

collected during several successive investigations that were centered around different categories of psychiatric problems and carried out in representative samples of the population; and finally, clinical data not only from local psychiatric and medical services but also collected directly during clinical examinations of sub-samples of the population, by the medically trained members of the team.

It was the ethnographic studies of communities that were the first to take place in 1950. The twenty thousand inhabitants of the county were distributed in 97 groups that were more or less divided by language (English and French), by economic activity (fishing villages next to villages where forestry and agriculture was dominant), and by the relative level of changes that were affecting them. Anthropologists at once focused their study on the impact that technological changes brought about in the forest industry and in the fishing industry and on how these changes affected the every day lives of the people in the small Acadian and anglophone communities. Two fishing villages, one English-speaking and the other Acadian-French speaking, and two villages of forest workers also anglophone and francophone, were chosen as being relatively representative of the linguistic and economic characteristics of the county. The selection of communities was also done from the point of view of their differential exposure to technological changes and their level of social and economic destructuration.

An anthropologically-inspired instrument called the Family Life Survey (FLS) that incorporated several categories of information previously investigated in ethnographic studies of the communities systematised this information in the form of a questionnaire applied to a sample population. A first group of questions had to do with ethnic identify, spoken language, religion, values, professional occupation, family structure, participation in local organisations as well as diverse demographic data (age, sex, education). The second group of questions was designed to obtain factual information on all episodes of physical or mental illnesses of the respondent, or any other member of their household. The objective of the FLS was to establish an estimation of the prevalence of different categories of problems in the adult population of the county independent of whether people had or had not consulted a local medical or psychiatric service.

The methods used and the results of the Stirling Study have irreversibly marked all subsequent work in psychiatric anthropology and have oriented all reflections about the relationship between contextual socio-cultural factors and the prevalence of psychiatric problems in new directions. After the 1952 study researchers were dismayed by the discovery that there was a 20% rate of people who were considered to be suffering from a clinically identifiable problem of mental health. In addition, the data collected no doubt indicated that there was a direct relationship in the community between the level of social disorder and cultural heterogeneity (confusion in the norms), and the increase in individual problems of mental health.

Leighton did not hesitate to pose a difficult question lying at the heart of his socio-cultural theory of the person, that being what the meeting point between the macrocontext and the individual process was. The emphasis was definitely on the impact of destructured environments on the psychological processes of people but also evoked, with a positive perspective, the possibilities of the "restoration of the essential psychic condition". While including an intersocial and intercultural comparison in this reflection, Leighton came to wonder if the emphasis on social variables would not eventually lead to the devaluation of the impact of cultural variables in the psychiatric field. He writes on this subject :

> "The findings do not negate the importance of culture in mental illness and especially in mental health. It could be that our instruments and the analyses conducted so far miss subtle yet important cultural influences. It does, however, suggest that even major cultural differences do not have the significance of our other point of concern, namely socioeconomic deprivation and disintegration" (Leighton, 1991).

In other words, the impact of social variables was so great that it masked the more subtle influence of cultural variables. In addition, this attenuation of the cultural can be explained by the fact that the researchers from the Stirling Study were examining the situation from an explicitly social etiological perspective and did not look at, for example, how a particular culture contributes to construct the subjective experience of an ill person, or to model

the common forms of the expression of symptoms in that culture. This was not the goal of the research and in order to answer these questions one would have to use a different method than that one used in the Stirling Study:

CASE 2 : THE ABITIBI PROJECT

A research team led by Ellen Corin and Gilles Bibeau has made use of a "contextual semantic" orientation in order to study mental health problems in a rural region of Quebec, the Abitibi, which is characterised by the presence of three subcultures organised around mining, lumbering, and agricultural activities. In applying this contextual semantic approach, an assumption was made that the popular models used in the three milieus for identifying, explaining and reacting to mental health problems formed divergent systems of signs, meaning and actions, and that these systems could not be understood independently of the social and ecological context of each of the three subcultures and of their specific cultural values (Corin, Bibeau, Martin and Laplante,1990).

In developing their model for interpreting lay semiological and explanatory systems and practices in connection with mental health problems in the subcultures of Abitibi, the authors have combined, and partly reoriented, the "semantic network analysis" of Byron Good (1977); they were also influenced by the explanatory model theory put forward by Arthur Kleinman (1986). In the Abitibi study, the authors interpreted individual narratives about mental illness in light of the social dynamics of the communities under study, their central cultural codes, and the respective concepts of "personhood" found in the communities.

Comparative analyses of the distribution of disease in populations has been a classic epdemiological method for sorting out appropriate hypotheses about socio-cultural and other contextual factors which contribute to the incidence of these problems. In exploring the variation in these correlations, anthropologists have raised substantive issues about the connections between these socio-cultural patterns and what is designated as pathology. In the case of Abitibi, for example, the revelation by

epidemiologists that a large proportion of women over the age of 45 suffer from severe psychopathological problems, that young Abitibian women have high rates of psychological distress, that manic-depressive problems dominate among men, and that marriage provides protection against ill health for men, has led the Corin-Bibeau team to explore two fundamental aspects of the Abitibian regional culture. Their research demonstrates that these epidemiological findings are part of a larger socio- cultural configuration associated with historically derived values which remain relevant at Abitibi today, namely, culture of virile masculinity, rigidity in male and female roles, and extended absences on the part of the men. By using an in-depth ethnographic analysis, which focuses on family structure and notions of personal identity, it is possible to obtain a fuller understanding of the relationship between these values and the specific pathologies found in the mining, lumbering, and agricultural communities of Abitibi.

In addition to contextualising the pathological patterns in local cultures, the investigators also analysed interpretations and behavior of Abitibians in response to specific cases of mental illness. This involved obtaining statements in everyday language about identification of pathological problems including beliefs about their origin and the form and course that they take. In order to carry out this research, three complementary and interactive sets of information were collected. First, key informants in each subculture had described to them, in everyday language, a series of fourteen descriptions of behaviour related to specific mental disturbances. On the basis of these descriptions, they then produced narratives in which they described the significant signs by means of which they identified these problems, the meanings assigned to them, and the actions which should be performed in order to solve them.

A parallel ethnographic study was conducted in several villages and towns where the principal occupations are mining, forestry, and agriculture. These three subcultures were compared with respect to their dominant forms of social organisation, the degree of autonomy and control over their own recent history, and their openness or otherwise to the external environment beyond the village or town. Analysis of the ethnographic data led to the identification of patterns and vocal points around which large

portions of the social and cultural life of each subculture are apparently organised; these organising focal points were then in turn interpreted with reference to the socioeconomic forces and natural environments that have shaped each cultural milieu. As a final analytical step, the lay semiological and explanatory systems of reference and actions to do with mental illness were articulated with the ethnographic data.

For example, Berthelot, a village with an economy based on a single industry (the production of timber for logs, using a co-operatively owned sawing-mill), has recently experienced deep internal conflict due to a decision to sell the mill to a large regionally run enterprise. The foundations of communal life collapsed with the sale of the collectively owned mill: brothers argued with brothers, local leaders were discredited, all collective celebrations (festivals and so on), and the symbols of community life disappeared. The structural conflict that divided families and neighbors has progressively forced villagers to withdraw into a private and domestic space. As a defense mechanism against the collapse of traditional forms of sociability, a new normative order has been constructed around family life, with significant value attached to cleanliness, foresight, personal initiative, family support, and respect for the boundaries of private life. The erosion of collective symbols, combined with a withdrawal into the domestic sphere, has led to the dichotomisation of the social world into "good" families and "bad" families. Not surprisingly, the term "bad families" was attributed by their neighbours to families with the most mental problems (see Diagram 1).

Maricourt, a larger and more heterogeneous community, which also produces wood, is experiencing dramatic changes in its social life and cultural values, but in a very different way from Berthelot. In this case the success of the lumber industry has meant a rapid transformation of what was a large village into a small industrial town. Among its people today, there exists extensive confusion regarding the normative order: conservatively oriented people still rely on a village-centered vision of social life, while the more progressive inhabitants have created new symbols of social exchange. Maricourt is now split into two normative orders which has created ambiguity about core values, and a hesitation about the direction of future projects. In this context of rapid expansion, many people who came from marginal

parishes have problems in adjusting to the incongruity of the "pharaonic" project designed by the progressive segment of the community (See Diagram 2).

DIAGRAM I

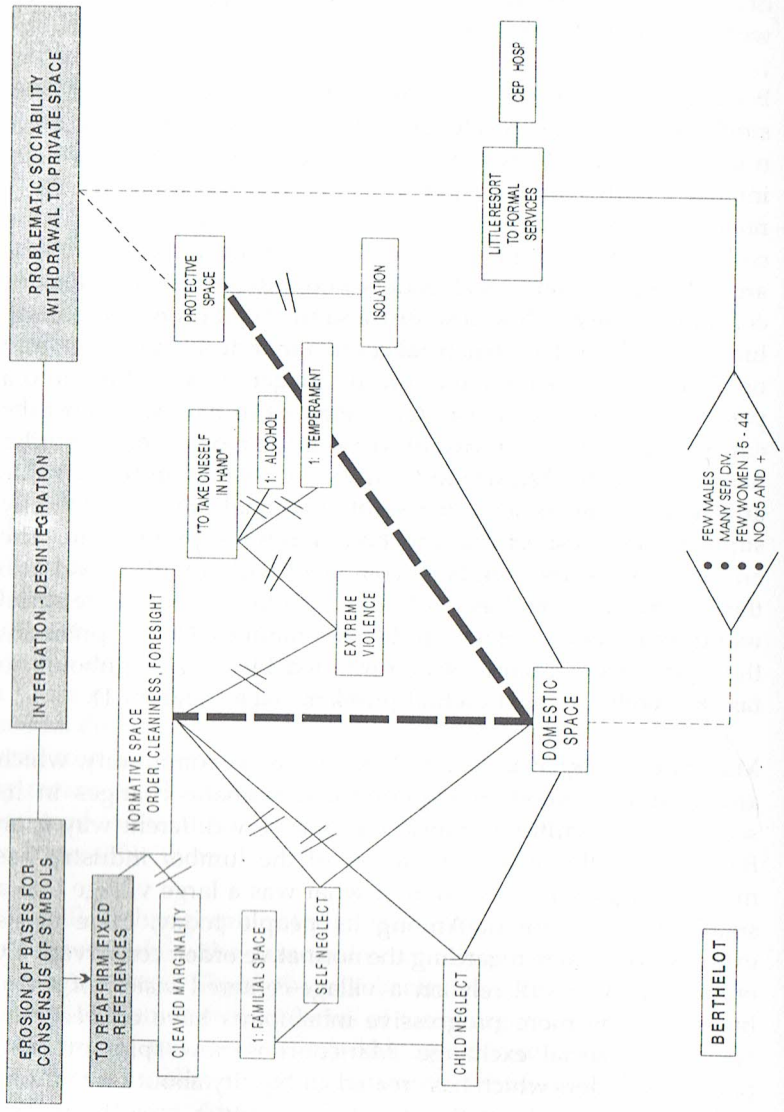
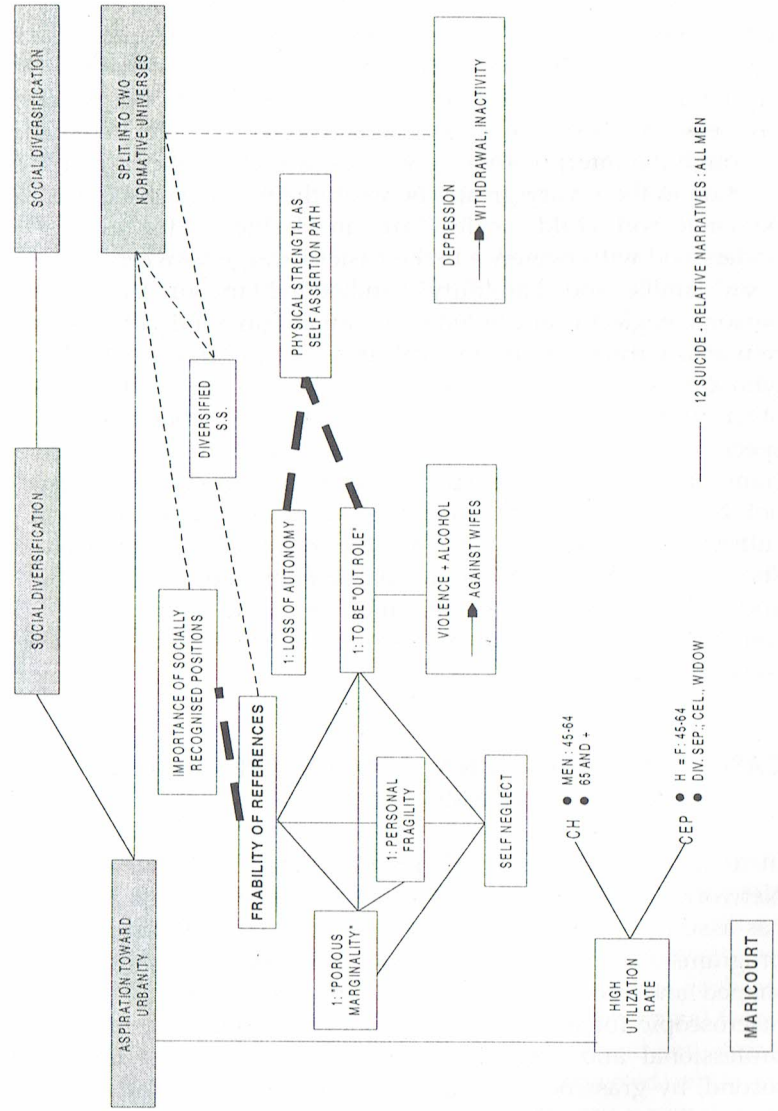


DIAGRAM II



The residents of both Berthelot and Maricourt are living a process of social and cultural restructuration which has led to the establishment of new norms, and to a deep cleavage among people as they go about their daily life. However, the way people in these two villages identify problems, assign meaning and react to them becomes understandable only when they are interpreted against the respective socio-cultural backgrounds. Narratives by key-informants indicate in both communities that the burgeoning problems of "violence" and "personal neglect" are resented, although the interpretations given about such problems and the reaction to them vary greatly between the two villages. Cases of personal and child neglect are, in Berthelot, for example, understood with reference to the basic cleavage between "good/clean" families and "bad/dirty" families. In Maricourt, by contrast, personal neglect is attributed either to marginal individuals who refuse to participate in the local dream of progress, or to those who are perceived to have a "fragile" personality. Violence and other problems are attributed with meaning, therefore, from specific social and cultural attributes found in the respective communities. Traditional epidemiological methods alone would not have uncovered the major contribution made by "local" cultures and social organisation to the construction of knowledge about mental health; the contextual semantic approach of Corin and Bibeau provides complementary data from which a contextualised interpretation of epidemiological findings can be constructed.

CASE 3: AN INTERNATIONAL COMPARATIVE RESEARCH IN ETHNOEPIDEMIOLOGY

In its action-geared research program, INECOM (International Network in Ethnoepidemiology and Community Mental Health) has assumed that existing formal and non-formal services and programs that function in one given region or country are largely shaped and modeled by a double influence. First by a set of macroscopic forces (political, historical, economic, institutional, professional and corporative) intervening from the top, and second, by grassroots forces and survival strategies of multiple community- grounded organisations and groups of all sorts. These two sets of forces interact within the same social space creating in their daily functioning a series of contradictions,

superpositions and reinforcements that have to be carefully assessed before formulating any proposal for change in the health services delivery of an area as complex and sensitive as the mental health domain.

INECOM has developed an approach for examining mental health issues in a way which is oriented in terms of both context and community facilitating the assessment of the complex interactive set of forces operating from the top and from the bottom as referred to above. Health policies and the organisation of health systems may differ from one region or country to another as well as the dynamism of community-based organisations and their capabilities in handling mental health problems which in turn may vary according to the characteristics of the local cultures and the daily conditions of life. Activities undertaken by the local teams associated with INECOM are actually performed at two complementary levels of inquiry that serve as foundations for designing community-grounded and culturally-sensitive services and for renewing policies and structures in the organisation of services.

At the community level, the objective is to gain a pragmatic knowledge of the conditions prevailing in the daily life of rural and urban populations of the ways people handle various types of mental health problems in very poor neighborhoods in which the vast majority of people struggle to survive, and in different sorts of urban suburbs which are often made of heterogeneous migrating populations with new family patterns, new values and disastrous economic conditions. In these milieus people still often propitiate spirits and gods when they are afflicted by mental health problems and in the absence of available professional services, families still largely count on their own resources or on natural community-organisations for bringing support to the mentally-ill family members. The existing community resources may be scarce in one given community or may be ill- adapted to certain categories of problems such as violence, severe delusions with passivity and withdrawal from social and economic activities or with alcoholism and anti-social behaviors. In some cases patients may be neglected while in others the families and community supports are optimal and contribute at least partially to the solution of mental health problems. Such problems are often interpreted through a religious

idiom that serves to assign meaning to the deviant behaviours but in most cases people combine very pragmatic interpretations with culturally-framed explanatory models (in terms of family heredity, actions of bad spirits, bad blood, effects of magic actions, etc).

The research activities in the communities have been organised around three key points : 1) The ways local populations identify the actual presence of a problem in one given person; the ways specific symptoms and signs are taken as markers which indicate that a behavior is "problematic"; the names used to refer to different categories of problems; the boundaries of the mental health domain. 2) The explanatory models used by the community for assigning meaning to the pathological behaviors; the identification of the key cultural categories in which the "systems of meaning" are grounded. 3) The reactions of people in front of persons suffering from social, behavioral or severe psychiatric problems; the ways family members and community organisations handle mental health problems on a daily basis. INECOM refers to these three complementary perspectives on mental health as "systems of signs meaning and actions" and it is on the basis of actual cases that these three systems are reconstructed with key informants. The focus has been on three aspects:

> Community studies based on the reconstruction of active andretrospective cases of persons suffering from mental health problems have permitted the collection of discourses and practices of the local populations regarding mental health; the signs and symptoms used for identifying the actual presence of a problem, its severity, its possible chronicity, the different explanatory models used for assigning some meaning to the mental health problems; and the strategies developed by the family, neighbourhood and community levels for managing mental illness and behaviour-related problems.

> Reconstruction with key-informants of the modes followed by persons presenting a problem; Who are the persons forming the therapy-managing group? Who takes the decision to consult and on what basis?

> Based on the preceding community studies, intensive study of a convenience sample of non-formal settings involved in the management of mental health problems is implemented in each study area describing various co-existing healing traditions: spiritist, Pentecostalist churches, Candomble cult-houses etc.

Specific approaches have been developed at the institutional and organisational levels. Most developing countries have not witnessed the pendulum swing from hospital-based practice to the current trend of deinstitutionalisation and community care that the Western world have experienced. In many developing countries formal services and programs in the area of mental health are not easily accessible or are characterised by weaknesses that severely limit their efficacy in meeting the needs of the population. On the one hand developing countries are plagued by regressive policies and problems of structural inadequacy. At the level of resources there is a lack of trained personnel and a maldistribution of the available health professionals as well as a significant deterioration of existing infrastructures not likely to improve in the near future in face of severe and lasting budget shortages. At the level of models of practice, the system relies almost exclusively on conventional schemes which are disconnected from the ways local communities identify and deal with mental problems. At the broader level of the population there is a chronic underutilisation of health services including those related to mental illness, which is largely due to inappropriateness of the services and strong reliance on the biomedical model.

More generally lacunas in relevant knowledge exist in most countries the filling of which could develop bridges between communities and mental health workers. In particular, mental health services do not sufficiently take advantage of indigenous knowledge and community resources and often do not consider the potential for involvement of both formal and non-formal resources for the development of mental health interventions. Above all there is a lack of reliable information as to how communities manage and cope with mental health problems. Such information could be used for innovative interventions that would foster the cooperation between community resources and mental health teams.

Community-based mental health activities integrated into the primary care network of a unified health system have not always been prioritised. Indeed the system of mental health care in most countries remains largely inefficient and ill-managed, being heavily based on a custodial type of hospitalisation and other conventional therapeutic strategies. Successive efforts towards changing the reality of mental health care in many countries have failed because most attempts at organising community-based systems have faced strong resistance from either the local population or the health institutions. However there have been many institutional, professional and academic efforts directed towards the development of local health systems that are based on the extensive participation of communities and that would allow substantial changes in the prevailing conditions of mental health care in these countries.

The main premise in this second level of inquiry is that in a pluralistic health care system, the illness experience, the management strategies, the institutions and the community resources are all systematically interrelated and socially constructed. The understanding of these interrelations and social responses are critical to the development of a sustainable, community-based and culturally sensitive mental health care system involving community groups, institutions, agencies and practitioners, health planners and policy-makers, in the design and provision of mental health care. Local situations have been assessed via ethnographic study of formal services and via the analysis of the actual functioning of mental health policies.

Field research is carried from two interrelated viewpoints : a "macro" one, encompassing the network of health care resources (i.e. agents and agencies) used by the mentally ill, including policies and planning processes; and a "micro" one, centered around the interface between patients and practitioners/healers working within institutional settings. The nature and quality of exchanges at the interface are largely responsible for compliance with treatment and/or the subsequent use of other healing strategies influencing illness evolution and treatment outcome. The emphasis of this "micro" level is to describe the prevailing models of practice used in the various selected institutional settings.

Complementary surveys include an assessment of current policies (national, regional and local) and planning strategies for mental health; a description of the local health services organisation (public and private sectors); and a focused institutional ethnography in selected institutional settings: organisation; levels of care; prevailing models of practice; professional profiles (i.e. background, therapeutic orientation and attitudes towards mental illness); healing systems and community management strategies in actual cases of mental illness.

Research results should serve to develop mental health services which are community-based, culturally-sensitive, manageable and economically viable. Therefore in addition to the assessment of the prevalent situation within the various institutionalised forms of mental health care, some study of current health promotion programs and rehabilitative practices is conducted in the different settings. Seminars with policy markers, community associations and mental health providers (in formal and non-formal services) have been planned as part of INECOM Phase 2 and should serve to help planners formulate adequate policies at the local, regional and national levels, and to involve public institutions, mental health professionals and community resources in partnership and cooperative endeavors.

Knowledge gained through research activities should lead to the design of strategies for planning management and evaluation of services, programs and interventions. Controlled experiments with careful assessments of health outcomes will be made at the sites which are ready for exploring new avenues in mental health delivery systems. The experiments to be undertaken in limited geographical areas will either concern specific mental health disabilities or will target particular categories of patients. In designing and implementing the experimental programs to be tested, planners and managers will build on three principles : 1) The articulation between the dynamism of community-grounded organisations and existing formal services: these different types of services will be more and more inextricably linked with an optimal utilisation of all community resources; 2) The articulation between care programs and psychosocial rehabilitation services: eventually a new type of paraprofessional will have to be trained as it is the case in India, where a program of mental health rehabilitation workers is presently being experimented; 3) The

cooperation between mental health providers of formal and non-formal systems in order to facilitate the circulation of diseased persons in the existing network of resources: experiments of partnership may be eventually launched.

Conclusion

Historians of science have demonstrated that scientific institutions and the ways scientists practice their craft tend to lose, in most countries their ecological and sociological characteristics and to acquire some degree of universalism imposed by international debates and dominant paradigms. This is not to say that the practice of science lies beyond all local idiosyncrasies: national research policies with their own priorities; the specificity of the national, intellectual style; the prevalence of certain schools of thought and so on. In that line social scientists have insisted on the fact that scientists cannot escape in their daily practice the local conditions and the national sociopolitical debates that surround them.

The case studies I have used in this paper deal mainly with Canadian examples and with the stages on which I have been performing. Other anthropologists might have illustrated their discourse differently. It belongs to the Indian researchers to root the debates in their native soil and to move the whole enterprise to their local scenes.

Discussion Session

MODERATOR : VIVAN KHAMIS

❍ *I would like to know about the importance of establishing the validity of qualitative data: is it really possible to establish true validity? For example, we were in the process of forming an instrument to measure 'burden of care' of chronic psychotic families. We had a hundred free-floating interviews. At that time many of the families had said "I wish the patient was dead" without any provocation or leading questions. Based on that we included that as a question in our instrument which said, "Do you sometimes wish that the patient was dead?" But when the question was directly asked to them, they reacted furiously and were very upset. So which is now valid? There are many instances like that where people tell us various things when they talk, but when it comes in a structured format, directly targeted at them, they react in an entirely different way. Yesterday someone had raised the question about the validity of life scripts: it is very retrospective, so how valid is it? So my question is, do we really have to worry so much about the validity of qualitative data?*

We have several levels of validity. The example you gave says a lot about either the content itself, or the form of the question. You have taken rhetorical statements as such and have used them as a question without rephrasing them. The question of validity has to be raised.

❍ *I liked the example suggested very much. It seems that there could have been a number of things that could be going on. One is that it is a different context; even though the physical setting may be the same, the first context is a very private context. The person is confiding something in you. In the second, they are standing up to be counted.*

❍ *We have to realise, especially in the context of qualitative research, that research is both an art and a science. While facing realities in research, you have to make compromises in terms of methods, the amount of qualitative information to use, how to interpret that and so on. It is your judgment in the end to decide how much you can do and what conclusions you can arrive at.*

- *The point I want to make reflects the discomfort I have regarding the problem of validity. The self-report of individuals have been emphasised in the various techniques talked about in the last two days. There is a body of research in experimental social psychology which says that many a time, persons are not aware of the causes of their behaviour. But when you ask them they say a lot of things and build up a narrative. What they say may not coincide with the actual reason. I would like your comments on this.*

  When we enter into the question of etiology, we assume that there is one ultimate professional model and the discourse provided by the patient or people around the patient is inadequate. I am not convinced that the narrative of the patient is less accurate than that of the professional. It is more a question of interaction between those different versions.

- *Suppose in indepth interviews you find a person is lying, what do you do with that person's data? Do you exclude it? In such cases what happens to the validity of qualitative studies?*

  There was a time in my life when I had thought there was coherence between an ideological system, a behavioural system and the practices of people. Slowly I have discovered that we find multiple discourses. People in different age groups, different situations, have different realities. How would you decide that one person is giving you the correct information and another is lying? Anthropologists have tried to solve that problem by selecting a number of key informants. If you systematically stay with the people for prolonged periods of time and learn their language, you will be able to distinguish between persons.

Studying Patterns of Distress, Perceived Causes and Help Seeking Pathways of White British Depressed Subjects in London

SUSHRUT JADHAV

I shall begin with a broad overview of my current research followed by a detailed discussion on the first stage, i.e., the process of developing a clinical ethnographic questionnaire to measure folk beliefs on depression in White British Culture.

Background

Psychiatric illnesses differ from other medical categories in that the social context and the patients' own belief shape their pathology to a great extent. However, Western psychiatric assessments have been criticised for disregarding cultural dimensions of illness, including the local and personal meanings which shape psychological distress in any patient group (Littlewood,1990; Hunt et al 1989; Kleinman,1978; Korsch,1968).

As a result, increasing number of patients in Britain (and the Western industrialised world) are turning away from professional biomedical care for psychological illness towards self-help and complementary therapies (British Medical Association Report 1986; Kleinman,1978). In recent years, the emerging discipline of clinical anthropology has developed models to explain the interplay of biological (disease related) and social (illness related)

data (Littlewood,1990; Kleinman,1990;1980). Medical anthropology, which emphasised the hermeneutic meaning of illness, has however focused either on the developing world or exclusively on ethnic minorities in Western cultures (Littlewood,1990). This assumes that the local "white majority" population is "culture-free", having illnesses which are a simple expression of the underlying pathology. My current research is based on the premise that patient constructions in any particular society are no less culturally constructed than in another (Littlewood,1990; Jadhav and Littlewood,1994).

At present, there are no studies in Britain that have systematically examined patients' understanding of depression, attitudes towards treatment and medications and, more significantly, the relationship of such personal explanations to clinical outcome parameters (Littlewood,1990; Higginbotham and Streiner,1991). Although there have been attempts to study lay beliefs among patients with schizophrenia, diabetes, hypertension and AIDS (Korsch,1968; Furnham and Bower,1992; Fitzpatrick et al 1984), these have not been examined for their clinical significance (compliance and help seeking), nor for their stability over time. Despite a large body of anthropologically oriented research on lay models of illness, such findings have seldom been operationalised for clinical practice (Littlewood 1990; Kleinman 1990; Holy and Stuchlik,1980; Jadhav,1995). Some major problems in such research include lack of an appropriate instrument to assess concepts and to determine the logical structure of lay conceptualisations; the instability of patients' beliefs over time, insufficient attention to social factors such as ethnicity, gender, class, occupation, economic background and patterns of expressing distress (Littlewood,1990; Weiss et al 1992; Foulks et al 1986; Holy and Stuchlik,1980). Fundamental methodological problems have, until recently, also made it difficult to effectively integrate qualitative prose information and quantitative data (Weiss et al 1992; Higginbotham and Streiner,1991; Holy and Stuchlik,1980).

Researchers in cultural psychiatry, have now developed a clinical ethnographic instrument, the explanatory model interview catalogue known by the acronym EMIC (Weiss et al 1992; Weiss et al 1988; Jadhav 1986). EMIC is a precoded semi-structured interview schedule which systematically characterises patients' explanatory models and help-seeking behaviour in a way that

facilitates analyses of the relationship between subjective experience (of patients) and objective outcomes of professional interest to clinicians. The acronym indicates the focus of the method: it aims to clarify local experience on its own terms. Its development has occurred in the context of recent clinical ethnographic research methods that seek to integrate anthropological and epidemiological aims (Littlewood 1990; Weiss, et al 1992; Kleinman 1990; Jadhav 1986; Jadhav 1995). The EMIC is rapidly becoming an established instrument for studying explanatory models and their practical clinical implications. It is currently in use to systematically examine beliefs and help seeking in a variety of culturally contrasting settings for major psychiatric disorders including schizophrenia, and for disorders of 'fatigue and weakness' (somatisation); as well as medial diseases such as AIDS, diarrhoeal disease, tuberculosis and leprosy. In a recently completed study, the EMIC was able to predict attendance at a leprosy clinic with respect to specified concepts and clarify details of associations linking stigma, guilt and depression among such patients (Weiss et al 1992).

We have now been involved in developing and field testing the EMIC to produce an adapted version for the White British population among psychiatric patients attending clinics at the Middlesex and University College Hospitals in London. The data from this ongoing British study will also be compatible for cross-cultural comparisons with EMIC based research in India, USA, Taiwan and Thailand.

By comparing EMIC data with outcome parameters (clinical record, Hamilton Anxiety and Depression Rating Scale, Structured Clinical Interview for DSM-II-R and Global Assessment of Functioning Scale), we propose to identify perceptions, beliefs and practices that predict compliance and successful clinical outcome and those that do not.

The major hypotheses to be tested in our study include the following :

(a) Concepts of depression that are congruent with biomedical understanding with respect to perceived cause(s) will correlate significantly with better compliance and improved clinical outcome;

(b) Concepts that are not congruent with biomedical understanding with respect to perceived cause(s) will correlate significantly with poor compliance and poor outcome;

(c) Patient satisfaction with treatment will correlate significantly with better compliance, lesser frequency of recourse to complementary health care and an improved clinical outcome; whilst those dissatisfied with treatment at the psychiatric clinic would be characterised by poorer attendance, non-adherence to prescribed treatment and resort to non-medical health care.

Specific Objectives

a) To develop an ethnographic instrument that would capture cultural information relating to patterns of distress, ideas of causation and help seeking pathways amongst White British depressed subjects in London.

b) Identify the relationships between conceptions held by depressed patients about their illness and their compliance with prescribed treatment.

c) Examine the relationship between popular perceptions, beliefs and practices with clinical outcome.

d) Identify beliefs and health related behaviour associated with poor compliance; to formulate more effective clinical strategies.

e) Contribute to the development of methods for cross-cultural research and comparative studies of depression and compliance.

I shall now focus the remaining lecture on steps in developing such an instrument which is the first objective. This was done over *four stages :*

Stage I

Adapting the EMIC: Cross cultural psychiatrists have extensively debated problems in developing instruments for research across cultures. The majority of research in cross cultural settings have been criticised for adapting instruments developed in Western settings and applied to generate data and test hypotheses in non Western settings. Kleinman and various others have designated this as a category fallacy (Kleinman, 1990; Littlewood,1990).

The EMIC was developed in a non Western setting (in India) and its application in a Western setting for eliciting folk ideas from a White British sample in London, posed similar problems. In order to overcome the problem of "category fallacy", we had to generate questions based on local white folk concepts rather than asking questions that were originally developed for an Indian sample. However, we felt it would be interesting to retain questions asked of Indian subjects to explore the possibility of a cultural universality of some concepts which are otherwise considered `culture bound'. We were keen to empirically validate the theoretical proposition that folk concepts in non Western cultures may not be `culture-bound': they may well be popular in Western settings if they were to be deliberately elicited.

Therefore, certain items were retained in the British version of EMIC. These include: the influence of religious background on depression, the presence of humoral ideas as causal mechanism in Western depression (hot and cold, wind bile and phlegm, and semen loss and retention) and concepts of mind, body and soul.

Gathering local information: The first step involved asking open ended questions to obtain baseline information. Someone once said "Anthropology is a method of making the familiar unfamiliar". This is easier said than done. In fact, doing field work or studying another society has the advantage of examining a culture where everything `appears' to be strange - from child rearing practices, parenting and conflict resolution to ways of eating, drinking and sleeping. The questions one needs to ask are therefore similar to "a child asking of his or her parents". There are no short cuts to this. Why do people cry differently in Britain? Why do they apologise so much? Why are they so `obsessed' with their body shape and image? Why do they eat

what they do? How do they express intimacy of feelings and thoughts/ What are their codes of interpersonal relationship? What are their (the local culture's) anatomical and physiological explanations for depression (cultural anatomy and cultural physiology)? Do they consider it a disease or sickness in local terms? If so, what terminologies do they use? How do these matters relate to wider social and political forces?

Of course such questions are never ending, and a focus on key issues and operationalising key concepts is necessary. In studying depression, this means asking how emotions in any culture are conceived, experienced, expressed, resolved etc. Operationalising folk beliefs for our research was based on Kleinman's Explanatory Model Theory cited earlier. Field work involves suspending one's biases and judgments, and asking what may appear to an informed person as rather stupid or obvious. Often a good substitute for mere asking is participating in lived experiences, for example, living with a family, attending key cultural events including births, marriages, deaths and funerals at one end along with experiences with formal psychiatric services at the other end. In the jargon of anthropology, this is "participant observation". There isn't an agreed boundary for defining the sphere of participant observation, although in recent times a lot has been written about ethical codes around such matters.

Sources: It is important not to be too concerned at the outset about the 'final' questions, structure and other technical matters. These will emerge as one begins asking the questions outlined earlier. In developing the EMIC, apart from working and participating in the British cultural life, I found the following sources useful:

Radio: There are frequent programmes that are known as counseling hot lines. People ring in to a counselor, state there problem and seek advice. This is a live broadcast, and allows an opportunity to understand popular patterns of distress, local ideas of causation and ways of seeking help.

Television: Similar to the radio programmes, these shows include: psychiatrists interviewing a person with psychological problems, soap operas which often centre around key emotional events and comedy shows which articulate popular brands of humour within

the culture. Here, it is useful to know how certain emotions are expressed and the researcher is able to calibrate feelings and emotions by visual examination of the data.

Newspapers: Most British newspapers (broadsheet and tabloid) feature health columns. The one I found most useful was the so called Agony Aunt column, similar to the question and answer slot in popular Indian magazines and newspapers. Again, the nature of letters written, the manner in which they are framed and the experts advice give vital clues. Ideas around stress, nerves, pressure (semantic networking) are to be found very often in such columns. The term 'semantic network' is a method of studying the cluster of meanings associated with a particular notion of sickness (Kleinman 1990); The source data for such using such a method most certainly includes the media and popular literature on depression and emotions. If a researcher is looking for local knowledge, then a local tabloid is far superior to the national broadsheet newspaper. Newspapers often feature advertisements on public health and other popular health campaigns that are similarly useful (refer to Appendix I).

Support Groups and Other Voluntary Organisations: A wide range of such groups exist in Britain. These include Mind, A.A., Overdose Anonymous. Bereavement and Phobia Societies. Staffed and run by ex-patients, they are key places where important concerns about depression and other emotional problems are discussed. Attending a few meetings at such venues provide rich experiences and narratives that could translate into questions for an ethnographic instrument.

Academic Tests and Research Journals: Looking for chapters on depression that discuss everything from etiology to treatment, and examining research journals on mental health provide vital clues about how and why certain ideas are privileged and the manner in which such ideas diffuse outwards into the culture and from the culture into professional tests. (See Kay,1994 for how the idea of geomagnetic storms relate to depression and how such ideas link with popular western ecological movements). To clarify this matter, try to think of how a Western anthropologist finds Ayurvedic texts in India to reflect popular ideas around the theory and practice of humoral medicine. Academic tests, in this context, mirror key cultural concerns articulate through 'scientific'

research and related enterprises (refer to Appendix II for a cultural and historical analysis of the term depression.)

Lay panel: In our work on developing the EMIC for British subjects, we set up a lay panel of 'native informants'. These included taxi drivers, teachers, parents, bartenders, nurses and hospital receptionists and porters. This was an attempt to structure and define a boundary around key informants: who would often help me understand the Why and How of popular theories about depression. This is not a prescription but a suggestion of simplifying a process that could otherwise be complex, daunting and never ending. There are no criteria for selection of informants other than willingness on their part to help you. Of course, we must remember the classic problems around informants: They might tell you that you want to hear, or conversely, not tell you something crucial. Some may not be reliable, in that they might change their stories often or offer information in return for material or other rewards. One could argue that 'changing' stories or providing information in exchange for some gain could be part of the culture, and not something that contaminates 'data'. There is no simple answer to this except for a researcher being aware of the circular nature of this problem.

Other Sources: Tape recorded transcripts, flyers and brochures and visits to local folk practitioners (in London this would include a range of Complementary healers). Careful and detailed notes of visits to such places and filing away material is crucial. During this phase, I visited several complementary healers, scanned notice boards at local council offices, neighbourhood help centres and social work departments (refer to Appendix III).

Stage 2

Structuring the instrument: By this stage, I had already formulated questions that were crucial to the study. The next stage involved organising the EMIC which was already structured in the following manner:Socio cultural data, Patterns of Distress, Perceived Causes, Help Seeking Pathways and Mind-Body-Soul relationship. It is not enough to ask the 'right' question but also crucial to *situate* it within the appropriate section of the instrument. Thus, items such as "Neighbourhood Interactions" need to be

placed following queries on "Housing and Social Supports". Similarly, appropriate introductory remarks shift the process of interview in a direction that then allows subsequent questions to be meaningful and engaging. Other technical matters include wording (personal or impersonal); language (avoiding jargon and skillful useful of local slang); length (brief but designed to elicit maximum information) and layout (boxes for codes, dates and ID number, space for recording prose data particularly for open ended questions). Subsidiary probes are often useful when the subject is unable to understand the main question. Researchers need to be decided which questions are to require either closed and fixed or open and free responses. Starting with an open and unstructured style is useful as there are few assumptions about peoples' answers and this often provide unanticipated answers.

We did a content analysis of the material gathered to extract key concepts embedded in the prose responses to open ended questions, and these were then tested out repeatedly to exclude some and retain others. This was done with Professor Mitchell Weiss, the senior author of the EMIC, and Professor Roland Littlewood who is directing this study.

Stage 3

Precoding the responses: This is relevant if you wish to analyse data quantitatively. Anthropologists would not be particularly concerned about precodings as their analysis is based upon reading and picking up themes embedded within their field notes. In the EMIC research, as outlined earlier, we plan to analyse data both quantitatively and qualitatively. This was done after reading all responses to each question on our pilot versions and generating categories that were then coded with a numeric. Thus the initial open ended questions were **transformed** into closed and fixed response ones. For example, for the question: Do you like your first name? The codes were 1=Yes, 2=No, 3=Don't Know & 4=Uncertain.

As the nature of questions and responses got more complex, such as in the 'Perceived Causes' section of the EMIC, we had to develop newer ways of coding. Most of you are familiar with the problem: What does one do with prose data after numeric coding? If one is doing a quantitative study, the prose data is

often discarded at this stage; but in qualitative research, such data is the very essence that serves to amplify the coded categories. Professor Weiss has developed an unique mathematical statistical method that would capture this information in addition to retaining the numerical codes. I shall discuss a related computer software package, HyperResearch, in tomorrow's session, to highlight some important developments in this area.

Stage 4

Field testing: This is analogous to testing a car on the road before a decision to mass produce and market is reached. For ethnographic research instruments, this translates into assessing the face and content validity of questions, checking out precodes, adding or deleting questions, and in general tidying up the instrument.

With the EMIC, this was done repeatedly, and involved independent ratings by colleagues to examine the reliability of questions and codings. We field tested the EMIC about ten times and generated 15 versions before freezing the instrument. Each version was given a specific number and used as draft versions for subsequent EMIC versions until we were satisfied that we had reached a stage at which no further developments were necessary. The process of finalising the version is known as 'freezing' or 'closure'. After freezing the instrument, data collection can begin. There is a great temptation at this stage to add new items or new codes. This must be resisted at all costs as it will affect subsequent data collection and analyses of results.

I have detailed steps in developing a clinical ethnographic instrument to assess folk beliefs on depression in White British culture. A crucial aspect of such a procedure entails a thorough and detailed examination of local information from a range of sources outlines earlier. This requires a considerable amount of time and energy to be invested in, with returns of rich and meaningful data for subsequent analysis. For a more detailed discussion of limitations and problems in rushing through a hastily compiled set of questions, refer to Jadhav and Littlewood,1994; Heggenhougen and Draper,1990; Jadhav,1995). It may surprise some of you that the process of developing the questionnaire, took over 12 months.

I would conclude by emphasising this: an ethnographic instrument developed without prior extensive anthropological field work and utilisation of key cultural knowledge is of little use for a study that seeks to analyse 'cultural' data generated from such instruments. Cultural psychiatric research has unfortunately been, until recently, largely impoverished by the absence of such data; and a major reason for this relates to the hasty manner and culture 'blindness' of researchers and their research instruments.

Discussion Session

MODERATOR : R RAGHURAM

- *Did you prepare such detailed questionnaires while in Bangalore? Secondly, was the degree of alienation and the sense of being an outsider more in London or in Bangalore?*

Yes, but in 1983, Bangalore was equally foreign to me. Then I equated myself with the culture: I learned Kannada, tried to integrate myself in the community, took part in plays and other such activities, and most important, got a feel of the place. It took about three years to do this. But in London, it took much less time. I knew English, I was socialised into the medical theories while I India. I was acquainted with some of the cultural categories through movies and books. One difficulty I had was my own ethnicity: whether I would be viewed as an outsider coming to study them. But it has never been an issue so far.

- *What are dividing lines between grief and depression? If you have a lot of data on grief or loneliness, how do you make that shift? I am not talking in emic/etic terms because I never liked them, for a very complicated set of reasons. Secondly, within the white, British category you have, are their any distinctive ways of talking about grief which distinguish men and women?*

The data that I have been gathering has been from a sample who have a clinical diagnosis of depression. Grief, depression, loneliness are subsumed under it. I have not looked at each category by itself but that is something I would like to do when I analyse the data.

- *How long did each interview take? When do you decide the end-point of an interview? Secondly, why did you choose minor depression rather that endogenous depression? Is it because you feel that the etiology of minor depression is more cultural, while in endogenous depression it is really biological?*

The time I take for an interview depends on the time I take to finish the questionnaire. The questionnaire is structured and it has a finishing point. It takes me about two hours to go through it, with a break in between. The break is when I have finished an experience-distant, diagnostic interview. I

stop over there and then move in to ask them their personal experience and explanations. When I re-interview them a few months later, I run through the same questionnaire, but abbreviated to the point that I pick up themes from the last interview to see whether those explanations have changed.

The reason I chose neurotic depression is because that is one diagnostic entity where the explanation we have about the illness is the illness itself. There is very little evidence to support that it is biological. It therefore addresses a variety of hypotheses from different frameworks: sociological, political, cultural, psychological, that are all competing theories. There is a lot of pluralism within it. That is why I am very keen to see if there is a cultural base to it, and I think there is.

- *Does anybody really doubt the fact that neurotic depression differs from culture to culture?*

I think they do. If you look at the way diagnostic categories are used, you will find that there is a certain doubt. The 'Old School' believes that the form is the same everywhere, but the content changes. I would take the stance that the form itself is different. We do not know what the form is unless we go about looking for the other experiences and issues that people who are depressed have. We may even have to give it another word instead of depression. Unless we do that we cannot just say that this is depression and capture it with questions which are themselves limited. I am saying that culture constructs depression actively, rather than just filling up the content.

- *Would it make sense to you to look at the gender difference in depression along relational lines? Most feminist theories of depression are now looking at depression in women as a self in relation to gender (differences between men and women as experienced during times of depression). As you also pointed out, women are more able to talk about depression than men.*

I have not really looked at it. I have a focused ethnography and so I have to limit myself in terms of what to study. The main issue I am looking at is, what are the ideas that are there, what are the internal articulations people have, keeping in mind that the depressive vocabulary is culturally unique.

Observations from the Project: A Psychosocial Study of Alienation Among Indian Youth

BISWAJIT SEN

I

Before I come to the specific objective of the paper, let me give an outline of the project itself. Briefly we wanted to understand the psychosocial processes which, on the one hand led youth to commit violence, sometimes of the most perverse kind. On the other hand, as we assumed, the same processes led other youth to engage themselves in the most constructive of activities though remaining firmly anti-establishment. To that end we devised both a quantitative and a qualitative part to the project.

The quantitative part, which we shall not discuss here, involved the administration of three questionnaires to 576 youth in different parts of India. The qualitative part involved detailed interviews of around 75 individuals. They broadly fell into three groups: the first included those who have either actively indulged or indirectly participated in movements of political violence; the second consisted of those who are involved in voluntary but peaceful protest movements of a constructive nature and the third comprised youth who have done neither.

The length of the interviews varied a great deal depending on how much the individual had to say. On average, an interview was of two hours duration and none was less than one and a half hours. The longest interview was about four hours and had to be divided in into three sessions spread over three days. On the one hand, the interview had an agenda inasmuch as we were interested in events, activities and attitudes, and inasmuch as we were inquiring into psychosocial processes. On the other, the interviewee was allowed to set the agenda to a large extent, particularly during the initial part where the person's socio-political beliefs and actions were being addressed. The interview became a little more directed when his or her psychological processes were being discussed. But there too, unlike a doctor-patient interview, the interviewee was never pressed for information. He or she was free to impart as much or as little information on any particular topic that was being discussed. Most of the time our interviewees were exceedingly cooperative in providing information, even of a highly personal nature. Confidentiality was, of course, assured.

There were a few occasions, however, when the interviewee frankly declined to talk about certain areas of his or her life. On one occasion, permission for recording the session was refused. But these were the exceptions. Thus we have succeeded in obtaining interviews from terrorists in Punjab, ULFA extremists from Assam, ex-Naxalites in Calcutta, and people actively involved in the PWG movement in Andhra Pradesh. We also have interviews from people who are successfully raising their voices - by entirely peaceful protest - against the forces of oppression in Bihar, West Bengal, Karnataka and Punjab.

We are still in the process of analysing the interviews therefore I will be unable to share the results of our analysis. What I propose to do will be to share just one aspect of the analysis and that will be the different contexts in which even the simplest of statements needs to be thought of to impart meaning to the statement. And even when I think that I have exhausted the situation of all possible meanings, perhaps a second person from a different speciality will be able to see new meaning by pointing different contexts. What I am essentially trying to say is that words take on different shades of meaning when you place them in different contexts.

What are these contexts? For example, the interview situation and the personality and conversational style of the interviewer would supply the first two contexts. The cultural background of the interviewee and the current sociopolitical events at home and abroad would supply two other very different contexts. The question itself, the way it is being asked, the mood of the interviewee, his personality, and his childhood experiences supply more contexts. You may need all or most of these contexts to bring meaning to even simple statements. It makes a difference, for example, whether there is a third person or others present during the interview, and this person's relationship with both the former two.

Before I go further, I must make a clarification. I will first provide an illustration and then go on to show how meaning can be derived by exploring some of the contexts I have mentioned above. At the end of my exercise, the question will remain, how do I know that the meanings which I have derived from the transcript of an interview which I have conducted are valid? The most convincing validation would be to take my interpretations back to the subject and ask him to what extent would he be inclined to agree with me. Some researchers have carried out such an exercise. However, for this project this method of validation is not feasible except for only a few interviewees. The next best method would be to check with my peers whether they can perceive my logic in deriving a particular meaning. This, in fact, is being done.

II

Now let me come to my first example. A younger colleague of mine, who is also a psychiatrist, and I were interviewing an ex-Naxalite in Calcutta. He was around 45 years old and a very close friend of this colleague of mine. The interview was being conducted in his own home, in a room where privacy was assured. Tea was brought in at regular intervals, and the interviewee had intimated in advance that being a regular smoker, I should not mind if he lighted up from time to time. To this I had readily agreed. In other words, in my opinion, all three of us were very comfortable.

Now I come to the point where I was brought up short when reading the transcript of the interview. My feeling was that the question asked was quite specific: How did you come to be associated with the Naxalite movement? The answer in Bengali, according to the transcript, was: *Ekta hawa* which means literally a wind or a breeze. Following that phrase he went on to describe some his Naxalite activities.

I was puzzled on listening to the tape, and realised that as the statement stood, no meaning could be made of the two words. Yet, he may have indicated something by the words which could be psychologically extremely important. So I went back to the original tape and listened to that portion of the conversation a number of times. Then I listened carefully through the entire interview, reading simultaneously from the transcript and occasionally pausing to make remarks. You see, when you listen with focused attention to the entire interview, it no longer remains a two-dimensional transcript. You are transported back in time as it were to the interview situation itself. For example, you can remember the eyes, the non-verbal communication imparted through conscious and less conscious gestures of the interviewee. You can also remember your own reactions to the remarks made by the interviewee. The following is a reconstruction of the relevant section of the interview in order to make some interpretations from what he said:

When I put this question to him, there was a pause on the tape. I distinctly remember that he appeared to go into a kind of reverie at this time. Though I am not too sure, I think he was actually visualising his early days in the movement and trying to think of a proper response to my question. I must add here that he was an award-winning film-maker and he had said at the outset that while he was good with visual material and with music he was not sure how good he would be with words. He had further cautioned that he was extremely likely to go off at a tangent and should be brought back to the point if he started rambling. The words, *Ekta hawa*, were said after a long pause with his eyes still focused in the past. This was followed by another pause after which he began to describe some of his activities.

Here some more information about the person may be useful as it forms the background to understanding his words. His family

migrated to Calcutta from the erstwhile East Bengal soon after independence. They were constrained to live frugally in a colony of refugees in an area which, in the 1950s was considered the outskirts of Calcutta. When I was introducing myself and the topic of our research, I happened to mention that Calcutta had become turbulent from the late 1960s. He read into my statement, correctly if I may add, the implication that Calcutta was relatively peaceful before this period. He promptly disagreed and said that Calcutta had seen a lot of turbulence in the 1950s itself which he remembered vividly and in some of which he also had participated indirectly, like throwing stones with other children. At this point he also added that so far as he was concerned violence was a way of life, and right from his childhood he had seen a lot of protests by the refugees about claiming land, ration cards and the like. Some of these movements had turned violent, mostly after the police made a lathi-charge or threatened to do so, he had said.

I need to add here that although I was not a participant, my years in medical college were spent almost literally in the thick of Naxalite rebellion. The Calcutta Medical College is situated on College Street which was the hotbed for many public Naxalite protests. So with the knowledge of his background and my personal experience, I choose to interpret his *Ekta Hawa* statement in the following manner: It was as if a whirlwind had descended and before I realised what I was doing, I got caught up and had eagerly joined THE CAUSE with great fervour.

It is quite true that between 1968 and 1970, Calcutta did appear to be caught up in a whirlwind of Naxalite activities. Almost overnight, people refused to venture out after nightfall and many cinema halls had been compelled to call off their "night shows" for some time.

The words I have "put in his mouth" can perhaps be justified from another angle. I strongly feel that he is the kind of person who would impulsively join a novel, political movement out of the sheer romantic appeal it would convey to him at that impressionable age of eighteen. He provided a number of examples, from his childhood till this day, where he has, purely on impulse, taken on odd jobs of various kinds just because they carried some emotional appeal to him, and has equally abruptly

left them when bored. I am given to understand that his films usually have a highly romantic and mystical flavour and the same is apparently true of the music he composes.

One more context of the meaning I have constructed may be offered. It was very clear that he deeply regretted the apparently mindless violence which Naxalites indulged in the latter part of their movement, like killing an unarmed traffic policeman. When I asked him had he killed anyone by his own hand, he flinched, and almost whispered, 'Yes!' while a deep shudder ran down the entire frame of his body. I got the impression that he would now like to distance himself from those aspects of the movement which distress him a great deal. Thus a whiff of escapism appears to emanate from the *Ekta Hawa* statement. It is as if he is trying to say: It wasn't really my fault; this whirlwind came and swallowed us up.

So he just might be conveying a denial of personal responsibility in those words. In fact there were some other events in his life which were indicative of evasion of personal responsibility.

III

I will now go on to give you a smaller but still interesting example of how, unless you are particularly sensitive to linguistic and cultural nuances, it would be easy to miss the significance of what really is being said. The interviewee, who was around my age, had introduced himself as a writer. His father had been a left-oriented political activist as well as a dramatist. So from his early childhood, he was exposed continuously to all blends of leftist politics, from the most moderate to the most extreme. He himself had been closely associated with the Naxalite movement without being formally attached to any political party. His most violent action to date was throwing a bomb on a police vehicle. Incidentally, his mother is a writer of national eminence as well as a social activist. She divorced his father when he was around twelve, a loss which he says put him through severe distress at the time.

Speaking of the "boys who commit murder" he used the word `dichotomy' in English, while speaking in Bengali for most of the time. Note carefully what he says:

> "There was a dichotomy in my mind from the 1970s, (and here he stumbled over his words), ...that the boy who commits violence.....who commits murder..... for the cause of radical socialism.....to what degree.....to what extent.....does he understand his actions.....does he really understand what he is doing?"

He then goes on to give examples but for our purposes this is enough.

The word 'dichotomy' has two possible conceptual meanings in the Oxford English Dictionary: a) division (especially sharply defined) into two classes, parts etc. and b) a sharp or paradoxical contrast. If you look at the context in which he used the word, you will see that neither of the two meanings apply here. So you are left wondering why does a well-read individual use the word in a novel way? What exactly does he mean?

Because of my own urban Bengali background, I am able to shed some light here. There are two words in which an urban educated Bengali will use to express doubt. Both are taken from Sanskrit. One is *dwidha* which means two contrasting streams of thought, the other is *dwandwa* which implies opposing forces. If I add to this my personal knowledge of the Bengali intellectuals' fondness for thoughtless and occasionally extravagant use of English, it becomes easy to say that he really meant to say *doubt* but because of the closeness in meaning of the specific words to the English word dichotomy, he has, unconsciously, used the word in this sense. One might object here to say that he may not have been sure of the precise meaning of the word. Fortunately for our analysis he himself provides evidence that he does understand the exact meaning of the word.

Later on during the interview he referred to a news report which had appeared in the English daily, *The Statesman*, concerning a bonded labourer who was almost beaten to death. He was visibly angry and said:

> "This is the crux of the matter.....a man like Kasim Mia is almost beaten to death and you (he presumably means the leftist intellectuals) will sit here in Calcutta with your lofty intellectual verbiage, as long as this dichotomy remains unresolved, no significant political change is possible."

There is little doubt that he is here using the word dichotomy in its accepted meaning.

IV

Let me end with a mildly humorous example where the meaning would have been largely missed if one only read the transcript. One needed to remember exactly how this man spoke to provide the various shades of meaning for a single word. This man, quite an extraordinary individual, was in his fifties, the managing director of a fairly large organisation. At one time he was very close to the top Naxalite leaders of Calcutta and had hidden the most prominent of them, viz., Charu Mazumdar, in his home for several years. Describing his days at his firm just before Naxalbari happened, he said that he was persuaded to accept the post of an executive committee member of the newly formed union in his company. He was a fairly senior officer by this time. With twinkling eyes, and a half-smile playing about his lips, he said:

> "And the effect of this information (that he was now a union leader) among upper echelons of my company was - (Here he paused for a second, possibly for effect and slowly said,) cons-ter-nation! "

What is so remarkable about the use of this word? His seniors would understandably be concerned on hearing that one of their colleagues had now become a union leader? The point is, as he made abundantly clear later, that he enjoyed the discomfiture of his seniors. He did not like snobbery which is a characteristic feature of the business elite anywhere (among others). His friends were largely constituted by the workers and villagers with whom he reported sharing an excellent rapport. A second point, more subtle than the first also becomes clear. The word consternation is not commonly used whether in speech or writing by the average professional. And here was this man using the word in English, naturally and humorously, during the course of a Bengali narrative. One immediately suspects the person is a learned man, an erudite individual. Later on I saw his vast collection of books of almost every kind and learnt that he spends a lot of time reading and writing and thus the hunch turned out to be true.

Discussion Session

MODERATOR : R RAGHURAM

❍ *Are two to four hours enough to really understand the concept of alienation in such a broad sense, which involves patriotism, ethnicity, thought, philosophy, ideology, and so on. Secondly, have you interviewed the family members of the extremists?*

No, two to four hours are not enough. Even ten hours may not be enough. I do not know if there is any expert here who would be able to say what is enough. We allowed the person to go on till he or she started repeating himself or herself, or stopped, or started going off on a tangent. I also stopped when I felt that the individual was tiring, because it is a very intensive thing. Most of the time we do not have the information about when the interview was about to end so in that sense it is an artificial thing. This I accept as a limitation, and it will be reported.

When we had planned the study, we discussed whether the family members should be interviewed. We agreed that it would be a very interesting study, but the way we had planned it, it would be impossible to interview the family members. How do you interview the family members of a terrorist? He will not take you to his home. Besides, some of the interviews were conducted in jail. So how would you interview the family members who were not even in the place? It was therefore not practical.

❍ *First, did the interviewee know, from your theoretical framework, what you expected from him? Second, you gave two examples of words he used:* ***ekta hawa*** *and 'dichotomy'. Could you not have asked at that very moment what he meant by those two words, instead of conjecturing what he meant by them? My third question is about nonverbal communication: when you asked him if he had killed somebody, and he flinched and shuddered visibly, you inferred that he had killed someone. Could you not have probed further? Or is it wrong in anthropology to go further?*

He said 'yes' to that question, but he also said he was against it. As for the first question, we kept our theoretical framework as simple as possible. We wanted to understand the

psychosocial processes which led a young individual to deliberately make a commitment towards political violence. We also made an assumption - it still remains only an assumption - that similar socio-political processes might lead a young person to a totally opposite direction, and make him do very constructive activities; still protesting, but non-violently. We told him that we would like his views about it, from his life.

Your second question was that instead of conjecturing now, why did I not ask him to clarify at that particular moment. I will give you specific reasons in each of these two cases. In the first case, I did not want to disturb him. He had fallen into a reverie of his own, reliving his experience and I did not want to interrupt him. Besides, and some of my own overconfidence will show here, I felt I could understand it. In the second case, the person was speaking so fast that at that point it did not strike my mind that he had said dichotomy in that particular way. It struck me only when I was reading the transcript.

❍ *I have a question relating to the ethical aspect of the materials we collect: In psychiatric interviews, materials are collected in confidentiality, but the psychiatrist also has a comittment to the law. When a person makes a confession in the confidentiality of an interview, the American view is that it is the psychiatrist's duty to report it. In my institute, we had a debate about it and the psychiatrists in the forum agreed that it should be incorporated in India also. When anthropologists come across such information, what is their ethical standing on the issue?*

As for the question on ethics, I can only give you a personal view. I explicitly reject the idea that if I hear confidentialities in the capacity of a psychiatrist or a research worker, my job is to report that to the police. I reject that idea; the decision is mine. If I decide to keep something confidential for a research reason, I am perfectly justified. I do not think a court of law can contest that.

❍ *Was this particular investigation supported by a governmental agency? Did the persons interviewed know this?*

For those who asked, I said that it was government. I did not specify the department.

❍ *What is the issue of confidentiality of the information obtained? Would the government have access to these records?*

Absolutely not. The names and the identity of the individuals will never be given to anybody else, other than the persons directly engaged in this interview, and all of us will keep it confidential. The report that will go to the government will contain the case histories, but not the names. In fact, in one or two cases, we may have difficulties in concealing the identity of certain individuals, and we have to work out how to do that.

COMMENT BY CHIEF INVESTIGATOR OF THE PROJECT (R.L.Kapur): What we are trying to present here is the use of a method which we do not normally use in mental health research, either in collecting or in interpreting information. In some way we are also learning how to make a presentation of this kind. Anthropologists have been doing it very well for a long time, but for us psychiatrists, it is a new thing to do. We went through quite a difficult process in learning how to interview. The first interview we held was in Dr. Veena Das's house, where the three of us adopted three different roles. Dr. Veena Das interviewed the same person in a completely open-ended manner, without any interruptions. I took a psychiatrist's style and got the information through directed questions. Dr. Sen took the middle style of letting the flow come on and then making occasional interventions to illustrate things. Please understand that some of the methods we are using have arisen out of this difficult process of learning how to interview.

The second point I want to make very clear is that the total context is absolutely necessary to understand how to interpret the information. Firstly, it was an eye opener for us when, after spending hours and hours in transcribing the interviews, we found that transcription alone was not enough. We had to go back to the voice and the emotions in the voice. That raised images in our mind which were much more meaningful than what we got out of transcripts. Secondly, the personality

of the interviewer and his mental state at the time of interviewing is equally important in what interpretation comes out. I have listened to the same Punjab interview several times, and each time a new meaning emerges. It is not that the previous is wrong, but a new kind of interaction has occurred between me and the information I am dealing with; and all are important. All enrich the meanings which come out of it. If you ask me what is the ultimate truth in this matter, I am unable to answer. For me, all is truth and equally necessary for understanding. Yesterday you had heard very scholarly descriptions of the narrative techniques from Dr. Veena Das and Dr. Kleinman. We are trying to show you an illustration of our fumblings with it. At some time or the other, we all have to learn this kind of a method to do our jobs better.

❍ *I want to make two points. One is about the relationship to the government. It should be absolutely clear that the research project as I understand it was not to give the government any help in tracing terrorists. There was a move from the government for a better policy for young people because there was a sense that what they were seeing as particular violence was the result of a large-scale alienation. The idea was that there should be a move towards better policies and some understanding on the part of the government on how alienated young people were feeling, first from the domain of politics itself. So quite a lot was about the anger of young people against those who were in positions of power.*

The second point is that as an anthropologist, I have done both kinds of things; I have collected affidavits and used the Supreme Court in petitioning the court against some very powerful politicians in the case of the 1984 riots, and also in Sri Lanka in the context of IPKF atrocities. In all these cases, I have been very clear that I am standing with the people, petitioning the court for certain kinds of justices, and I will stand by it. As an anthropologist, I find that these are things which I just have to do in relationship to people.

In the question that was asked whether it is one's duty to report a particular incident that has come in an interview, most anthropologists will not privilege the State. Even State laws are very differentiated. In the American context, what has been said

in a confession or in a medical interview is not something to which the State can have direct access. But I want to say that the dangers are not only in relation to the State. I wrote a paper on Punjab militancy in a French journal, which I thought was quite inaccessible to most people. However, it has actually been read and I have had feedback from organisations saying that my views are not correct and would I like to change my position. These are particular decisions one has to take about how much danger one is willing to take upon oneself.

Lastly,clearly you can have a collaboration at the level of collection of materials, but the analysis, I am sure, will go in several different directions. For example, if I was analysing the material that Dr. Sen presented, it would be interesting to know in what different ways this analysis might proceed. That plurality of interpretations must be kept in mind.

A Psychosocial Study of Creativity in Indian Science : An Illustrative Project Presentation

SUSMITA SUBRAMANYAM

Introduction

It is common to hear that Indian science is not doing as well as it should be doing. Whether such statements are justified is certainly an issue for debate. However, in literature on Indian science a paucity of finances, bureaucratic interference or inefficient infrastructure have been described as responsible for the poor state of science in India. It seemed to us that perhaps we were satisfying ourselves with far too superficial an understanding of the state of science in our country. A few years ago, a project was begun at the National Institute of Advanced Studies to examine the psychosocial and cultural factors which affect creativity in Indian Science. The study, initiated by Professor R.L. Kapur, relies considerably on qualitative methods of inquiry. An attempt will be made in this paper to summarise our research design and method for study. In addition, a sample of the qualitative data which has emerged from this work will be presented along with some of our preliminary findings.

Psychosocial Study of Creativity in Indian Science

The study aims to examine the psychosocial factors (family, education, culture, society, work environment etc.) which affect creativity in Indian Science. The project was designed in a series of stages.

Following pilot work, stage one of the study was initiated. Twenty scientists from the Indian Institute of Science, Bangalore were interviewed in-depth to elicit the following: (i) A description of creativity (What is creativity? How does the creative process operate? Who is a creative person? How does one recognise creativity? How do psychosocial factors affect creativity in general? etc.). (ii) Views on creativity in Indian Science specifically and the role of psychosocial correlates in this context. This exercise was done to formulate a set of questions which we would try to explore in the final stage of the inquiry.

In stage two, peer evaluation was used to identify a sample of creative scientists. This was done by asking a large number of scientists to nominate peers whom they considered creative. The criteria for peer evaluation were derived from data obtained in stage one. Ten scientists who were nominated the maximum number of times were identified for further study.

In stage three, this sample of ten scientists nominated as creative by their peers is being studied along with a control group. To comprise this control group we chose scientists of roughly similar rank and seniority from the same departments as those who constituted the group of scientists nominated as creative by their peers. The objective of this phase is to study these scientists in the context of: (i) Their scientific work and research; (ii) their life histories focusing on the psychosocial influences and patterns which may prevail. This is the final phase of the study and it is currently in progress. A more detailed discussion of the methodology being used in the this stage of the study follows.

Stage Three of the Study

Scientists nominated as creative by their peers along with a control group selected as described are being studied extensively

using the interview technique. A considerable amount of time has been spent in developing a specific style and agenda for interview. A semi-structured approach of interviewing was adopted. The agenda was, in part, derived from stage one of the study which has already been described. In addition, our own reading of the literature on creativity raised several questions and issues which we wanted to explore in current interviews with scientists. The agenda itself is used for three purposes: first, to provide a starting point for scientists to talk; second, to allow a framework for the interviews, so that if people get stuck or are unable for some reason to respond, one has ready prompts and third, to explore the agenda itself. Most importantly the interview, despite its agenda, has been designed to be open ended, neutral and non directive, thus allowing ample scope for interviewee flexibility.

I will briefly describe the interview agenda which is in three parts: Part one covers the area of scientific work and research. It includes questions on how one became a scientist; one's detailed experiences while doing research (How do you pick a problem? How do you set about working on it? etc.); one's habits; styles of work; colleagues; goals; index of knowing one has done well etc. Part two is a standard life history approach used in clinical psychology which we have adapted for our study. It covers practically every area of an individual's life from birth to present day. Part three of the interview consists of a set of open-ended questions which have been added in order to gather a richer description of the person (What does living mean to you? What is most important to you? What qualities do you value most? What are your happiest/saddest experiences? etc.) This part also includes questions on whether the person has had any transpersonal or peak experiences.

Over 50 percent of the interviews are complete. Interviews lasted from five to seven hours; the longest interview was about 8 hours. Interviews were conducted over a couple of sessions, the break up of sessions themselves were arranged to suit the individual convenience of scientists. The scientists were extremely accommodating and hospitable. Identities of the scientists and interview material are strictly confidential and the scientists were assured of this. All the interviews have also been audiotaped.

Preliminary Findings

Interview material to be presented will be restricted to a few themes from part one of the interviews, relating to scientific work and research. It must be remembered that it is by no means conclusive, but only a preliminary understanding of the data. Thirteen out of the total twenty scientists have been interviewed to date on their scientific work and research. Out of these, six are scientists nominated by their peers as creative and seven are those chosen by us as part of the control group.

The following data illustrates some of the experiences while doing research which seem to be shared by the scientists nominated as creative by their peers.

> Scientist 1: "While you are sharpening it (ie. the problem), things are very confused. You do not know whether you do not know or other people do not know, it is very confusing at that time. So you have to live in that confusion for some time".
>
> Scientist 2: "Suppose there is a problem I am working on, there is something I am not able to...and that feeling can be there for long...at some stage, it is an act of resolution, but that stage takes a long time to come and I have no idea how long that is".
>
> Scientist 3: " I was struggling...I always felt as if it is far away. I am not able to do it. Yet I wanted to do it. I have a feeling that anytime I have made a good contribution, there was a specific problem which caused confusion and when that confusion is removed, it gives you happiness, the degree of happiness depends on the degree of confusion you went through".
>
> Scientist 4: [Speaking about what happens before a new result was seen] "Before that you are never quite sure, before that you are mostly fishing. I do not think the fisherman really bothers what kind of fish he is catching. They can sometimes sit there the whole day and derive a lot of pleasure without having caught a single fish".

> Scientist 5: "Often the solution comes after a large amount of struggle. I struggled, tried day after day, this method, that method...there is a fair amount of tension and lot of unhappiness, these things come slow. Trying to overcome this feeling of unhappiness or dissatisfaction leads you to your own way of trying to solve...but that is something which takes a long time".

Here, five out of six nominated scientists interviewed describe research as involving confusion, uncertainty, 'fishing', struggle - one does not know when, or if at all, a solution will appear. Most of these scientists also tended to work on long range problems which they pursued for several years - ten, twelve, seventeen years. Quotes from two scientists illustrate this well:

> Scientist 1: " I am willing to wait for five years, even ten years to open out the thing slowly so that at the end of the period I can say we have figured it out".
>
> Scientist 2: " Whatever problem I have taken, I have worked on for ten years on average. I will say nothing new in the first five years".

It appears that these scientists are able to hold on to their confusion and uncertainty. One is tempted to make an comparison, though cautiously. From the interviews with the control group of scientists, at least three out of the seven interviewed so far discuss their inability to work on long term problems which disallow immediate results in the form of publications. It is still early to conclude anything from this.

Another important theme which has emerged from among four of the six nominated scientists is that following the uncertainty, confusion and struggle, the solution to a problem occurs quite suddenly in a flash:

> Scientist 1: "The resolution came in a flash. It happened sometime in the night, at home. Then I could hardly contain myself. All of sudden it all fit one night. Good heavens, how is that it did not strike me all along..."

Scientist 2: "The way it happens to me is that I see that I have been struggling along, then I suddenly see, I recognise the solution, the essential thing and that final thing is a matter of seconds or less than that, it is immediate".

Scientist 3: "In fact I still remember, I was going from here to my house. It was a Sunday and the idea occurred to me on the way...maybe this is the reason. I went home, I wrote those equations and made calculations for the simplest possible case and my calculations worked. From that Sunday onwards I was on fire, I knew I could do it".

Scientist 4: "Some flash has to be there and you see the solution laid out before you...even now I remember...I went down for a cup of tea, I realised maybe I should try such and such method, this step...I went back and in five minutes I could see it".

One particular theme stands out remarkably as yielding different reactions among the two groups of scientists. This is the issue of recognition and awards. Quoting first from the interviews with nominated scientists:

Scientist 1:" I think I have got more than I deserve on the basis of my work ... after all what is it ... I am just doing what I feel like doing and why for that reason people should give you rewards and awards and I have got them. If at all, I should only feel grateful to people".

Scientist 2: " I did not want anything, but when they come, I feel happy. When an award comes I feel very pleased, but I do not go after it...that means there is something called putting in effort to achieve something, there is another thing that something comes on its own. So many things have come to me on their own".

Scientist 3: "Many people get worked up...I am not recognised, I have not become a fellow, I have not got this award. Honestly, of course, people can say now you can say, but even in the beginning, I do not think I ever

worried about it. You go after it and you get frustrated. It should come by itself. If you deserve, you will get...self satisfaction, that itself is very important".

Scientist 4: " I think people who enjoy research are those who do just what pleases them...many people might not consider what you are doing important or relevant or even fashionable, but you still do it and the reason you do it is because you like doing it".

Scientist 5: "I am not really worried about what other people say is important and (am) quite willing to bet on my own judgment about what is important. Recognition (is important), makes me feel good, but at the same time, I would have continued to do what I wanted to do (even otherwise)".

Scientist 6: " I have no attraction for positions... they have never mattered to me".

A sample of the excerpts from interviews with the control group on the same theme of recognition and awards:

Scientist 1:" Each of us is trying to be successful in our own way. You are trying to build up a career...once you have built a career and reach a certain level, this internal drive comes - you want to publish a paper, you want to be recognised, you want to make an impact".

Scientist 2:"You see, just doing research, publish, is not enough...you should be noticed..."(went on to speak about the importance of publications, number of students guided, awards etc. as his index of success).

Scientist 3:"After you perform for a while, people look for other recognitions - becoming members or fellows in specific bodies in the country and abroad. I have been fortunate to get elected into both academies. So these are yardsticks...one is publications, second is this kind of recognition and third, if you are doing well, there are international conferences you get invited to".

Scientist 4: "Certain amount of recognition is important. I will quantify that amount: fellow of one of the recognised institutions in your profession, scientific advisory body of some of the associations, editor of one of the international journals...this is satisfaction".

The material presented so far, though preliminary and far from conclusive, raises two important questions which need to be considered carefully:

i) Are creative scientists more able to live with uncertainty over long periods while doing research, compared to other scientists? ii) Are creative scientists less pre-occupied with recognition per se and the evaluation of their work by others?

Using qualitative approaches in research requires sensitivity, patience and most of all the ability to explore with the unfamiliar and uncertain. We have used this approach in our attempts to study the psychosocial factors which affect creativity in Indian science and have enjoyed doing so. May I conclude this paper by suggesting that while qualitative research is challenging, there are rich dividends for those willing to experiment and learn from it.

Discussion Session

MODERATOR : R RAGURAM

- *What did your sample consist of? Are you talking about natural scientists, or does it also include social scientists?*

  No, it did not include social scientists. They were scientists from IISc (Indian Institute of Science, Bangalore) in the engineering and pure sciences.

- *What status did you give to the interviews that you collected in terms of their structure? Did you call them life histories, life stories, or a narrative structure?*

  I am so glad you raised this question, because that is one of my own questions too. I don't know what to call them. I suppose they are stories, I suppose they are narratives as well. But to me they tell me something about the person.

CONCLUDING REMARKS BY MODERATOR: What you heard in these sessions were very promising insights into ongoing research which have utilised some kind of qualitative methods to study behaviour in a wide range of conditions ranging from depression to creativity. The response to such presentations can broadly be two: Initially you can feel inspired to follow the leads that have been suggested and carry out a similar kind of investigation. The second category of response which we often do not share is one of disbelief. What can this method offer that I am not getting from the method which I am accustomed to? But I must remind you that good science always emanates from those who disbelieve. So, if these presentations have lead some people to disbelieve the potentials of qualitative methods, even that would inspire scientific creativity.

Impact of Violence on Family Mental Health in Palestine

VIVIAN KHAMIS

The negative impact of political violence on the psychological functioning of family members has been documented in the mental hoalth literature. Research has shown that Palestinian families who were subjected to political violence during the *Intifada* have experienced a number of stresses and strains that presumably have taxed the psychological distress of family members, their well-being as well as their on-going life pattern (Khamis, 1995a; 1995b; 1995c).

In February 1995 the International Development Research Centre (IDRC) approved a grant to enable Palestinian researchers to carry out a research project entitled "Impact of violence on Family Mental Health in Palestine". The aim of the project is to explore the relationships between persistent political hardships threaded through daily life of the Palestinian people and their consequences for mental health, well-being and family functioning.

As a principal researcher I will highlight how quantitative and qualitative methods are going to be used in carrying out this scientific inquiry to Palestinian families living under occupation who have been subjected to various forms of political oppression during the *Intifada*.

The sample of the study will consist of 900 family members who were affected in one way or another by political oppression during the *Intifada*. The sample will include families one of whose members was :

a) Killed, b) Injured, c) Imprisoned, d) Tortured and e) Families who had their houses demolished. Efforts will be made to ensure that the sample is representative in terms of geographical locations, residential patterns, gender, status (refugee or non-refugee), educational and socio-economic backgrounds, religious backgrounds, and family roles (father, mother etc.).

The measures in this study will be administered as a large battery of instruments that focus on traumata, political stressors, role strains, socio-political satisfaction, family and community resources, family coping, family ideology, and outcome measures (i.e., psychiatric disorders, well-being and PTSD).

The majority of the scales were developed specifically for the purpose of the study except for the PTSD which was adapted from the DSM III-R, and the Psychiatric Symptom Index (Ilfeld,1976). A pilot study will be carried out on 100 cases in order to obtain the reliability and validity of the instruments.

For the qualitative portion of the study, a sub-sample of 140 will be drawn from the 900 individuals and will be divided into focus groups as a supplement to the quantitative method. There will be a 18 groups that will be selected to guarantee variation in grographic location :

a) 6 groups from East Jerusalem
b) 6 groups from the West Bank
c) 6 groups from Gaza strip.

In each district there will be 3 groups of women and 3 groups of men. Among the gender groups of each district, one group will consist of refugees and 2 groups of non-refugees. These will vary according to residential patterns (i.e., urban and rural). In addition, age and education of participants will be taken into consideration in order to maintain homogeneity in background which will foster discussion. The size of the group will be moderate (Morgan,1988), and the favored number of participants for the

purpose of the study will be 8. This size will ensure the contribution of each individual participat and will facilitate managing their discussion.

To recruit family members, payments of 50 Canadian dollars will be given for each person per session. The cash incentives will be meaningful to the affected family members whose psychosocial and financial costs in terms of medical and psychosocial care, loss of productive time, chronic disability, loss of function and loss of life and property are enormous (Khamis,1995a).

Three moderators with a post graduate degree in psychology or sociology, will be involved in each focus group. They will be trained by the principal researcher to adopt roles that ease entry, facilitate receptivity of participants, elicit cooperation, trust, openness and acceptance. They need to demonstrate that they can control the topics that are discussed and the dynamics of the group discussion in such a way that the people in it are not harmed (Marshal and Rossman,1989). Moderators will be guided to get the most useful information from the participants while attempting to keep their comments as nondirective as possible in regard to sensitive issues.

The level of the moderators involvement will be high in order to cut off unproductive discussion and to probe what the research objectives are intended to elicit. In brief, the moderators in the study will interact as Palestinians to Palestinians, exploring, but doing so with an interest in the welfare of the participant families. All interview of female participants will be conducted by females.

Moderators will acquaint participants with the aims of the study, the possible uses of the information, and the manner in which participants could aid in the research. Confidentiality will be safeguarded and the right of the subjects to abstain or terminate their participation at any time will be secured. This right will help the participants feel safe and not at risk by participating in the study.

The moderators conducting focus groups will know in advance what specific aspects of an experience they wish to have the respondents cover in their discussion; thus certain types of

information are required of all participants (Morgan,1988; Denzin,1970; Seltiz, 1965). In addition, the moderators will have a guide that will be prepared by the principal researcher to enable them to organise the discussion topics in more or less the same order from group to group (Wells,1974).

The guide will help in channeling the group interaction and in making comparisons across the groups in the analysis phase of the research. However, the moderators will have considerable latitude within the framework of the guide and may redefine the order of questioning to fit the characteristics of the conversation (Denzin,1970; Wells,1974). This feature of the focused discussion will be based upon the assumption that the most effective sequence for any respondent is determined by his or her readiness and willingness to take up a topic as it comes up (Denzin,1970; Richardson, Dohrenwend & Klein,1965). Allowing respondents this freedom may result in the raising of important issues not contained in the guide. The major topics of the guide will cover:

a) The magnititude and severity of trauma;

b) The identification of community-based resources which the family may call upon, access and use to meet their demands;

c) The identification of family system resources (e.g., cohesion, organisation, communication, social support);

d) The identification of family coping mechanisms and strategies;

e) The identification of specific intervention approaches and strategies that will minimise immediate short-term and long-term effects of violence and resultant trauma.

f) The initation of a mental health policy for victims of political violence, and the protection of Palestinian human rights as should be reflected in forthcoming legislation and laws.

The focused group interview will last between one and two hours. An audio taping (Silverman,1993) will be used to create a record of the discussion. One of the moderators will take notes to determine who is speaking. This will help the transcript typist to

identify each speaker. The focused discussion will be transcribed by unisort cards.

Analysing focus group data will be carried out at two levels. The first will focus on analysing the written material via content analysis (Silverman,1993) in which the researchers identify major categories within the topics covered in each participant discussion. Two researchers will read the transcripts and will code comments judged to be relevant into broad categories developed for the preliminary guide and for the emerging categories which will be relevant to the research questions but were not anticipated by the researchers. Reliability will be calculated by dividing the number of coded comments on which the researchers agreed by the number of agreements plus disagreements. Absolute freequencies, cross-tabulations and chi-square analyses will be used to summarise content-analytic data (Silverman,1993; Marshal & Rossman, 1989). The second level of analysing focus groups data will adopt the ethnographic approach which will use direct quotations of the groups discussion. It is hoped that by combining the quantitative and qualitative methods within this research project the results will be valid and reliable enough to aid the goal of treatment and policy recommendations.

Discussion Session

MODERATOR : MALAVIKA KAPUR

- *I want to make a comment in connection with research on victims of violence. There is a lot of difficulty in carrying out this type of studies. In some places, the victims of violence do not want to be approached, to disclose their identity, or to report about their suffering to the police or to other people. In those cases the methodology becomes difficult. It is only after a long and difficult process of winning their confidence that the information gets disclosed. At times they flatly refuse to disclose that they were ever kidnapped or that they had money extorted from them.*

  MODERATOR: That is a very valid point. People undergoing trauma may be very reluctant to openly discuss their experience. They may be suspicious about the consequence of their disclosure. Did you have such difficulties with your work?

  We are just starting this study. But in previous studies I did not have this problem because I, the researcher, was Palestinian. We have information centers which collect information on all reported cases from hospitals, police and so on. But there are a lot of cases which are not reported because of confidentiality and safety.

- *One thing we found while working with families with problems was that if we take just one or two members from each family as spokesperson for that family, we often do not get intimate details like cohesion and organisation. To come to an interpretation, it is often very useful to keep all the family members together, including the children, and to see both the verbal and the non-verbal communication that goes on. Have you handled that difficulty in your projects?*

  In this research, we are going to take one member from each household. In the previous studies, I had taken people in several roles - father, mother, brother, sister, and so on. In this study too we are thinking of including those family members, if the budget allows us. Having several members is also helpful because we can test the consistency in their answers.

❍ *Why did you specifically chose the focus group method? With the kind of objectives you stated, if you had given me the choice, I would rather have gone into each family and had a group discussion with all the members sitting there.*

The method you suggest is very good and would give a lot of information. But we wanted to interview groups together so as to get certain categories which may not have been possible in the quantitative instruments. We want to see the interaction, how they explain their concerns, their coping strategies. We do not want to go in-depth to that extent. We also want a large number of people to be included in the study.

❍ *You said you will be taking 900 members. Are these 900 households? And did you calculate dropouts within this number? We carried a study in the slums in Dacca, where we had 1050 households. But at the end of the study, after one year, we had only 800 households. Of course there is probable lesser chance of a dropout because you are giving CA $ 50 to the households. Do you think this amount of money will meet their needs? Besides, would this not amount to buying data from these households? I am uneasy about this.*

900 will be the final sample. We still have to decide on the numbers we will have to get to account for dropouts. We have taken dropouts into consideration. Secondly, we are not going to pay for the quantitative data we get from the 900. We are going to pay for the 140. Conditions in the West Bank are really hard, with some people living below the poverty line. When you ask somebody to come to you to be interviewed for hours, he or she should be compensated for loss of work time. Besides, participants in focus groups are paid all the time - you will see that in all literature on the technique. This is because focus groups started in the context of business research and in the field of business, they pay a lot more than what we are paying.

COMMENT : I would like to clarify some points, as a person who was involved in reviewing the proposal. The 900 households selected will be out a list from the Center for Human Rights Information Center. There is a registry for all those affected, whether imprisoned, killed, deported, and so

on. The data will be collected by survey questionnaires on those 900 families. That will be the quantitative part of the data. The second part is the focus-group discussion to get more in-depth information on the impact of violence, and to understand coping mechanisms. The quantitative data collected from the 900 households will complement the qualitative information gathered from 140 discussants from across the region. It was the combination of the quantitative and the qualitative that made us interested in funding the study.

This is part of a study we are trying to promote in different parts of the world, on refugees, displaced persons, and victims of violence. We are trying to get a similar research going in Sri Lanka. We are also trying to get studies going in Lebanon and Bosnia. The idea is to have a cross-cultural knowledge base to look at violence, so that we can even suggest policy changes at the highest levels such as the U.N.

❍ *May I still persist with a previous comment? I also feel a little uncomfortable in making this kind of a business relationship of paying for information. But I think there is an alternative. After all we are doing all this to ultimately help and to heal the people or the community. When I did a population survey at one time and decided to pay for information rendered, I discovered that helping them to find the right resources for their problems was much more desired by them than actual payment. In fact, it also sets a very healthy relationship between the research team and the interviewees since some people do come because money is being offered. Instead, if you make it a part of the program that those who have difficulties will be helped medically, psychologically, as well as in getting resources from the state to help them resettle, my feeling is that it would be a very useful way of doing the same thing which you are doing by paying. I would like you to consider this.*

The second point I want to make is that in all the studies that are being done on the effect of wars, disasters, or violence, I only see our interest in how the people suffer because of these; which is fine, and it needs to be looked at. But we all know that under these circumstances, people reach heights of character and of nobility also. We need to find out what kind of people are those, who under these adverse circumstances in fact came up as leaders. If you can

find their characteristics you are discovering in your culture, not only the negative side of the mental heath dimension, but the positive side also. I think it is very important to discover who did well under the circumstances.

I agree with you. In another project of mine we did this. The sample consisted of 624 families, who had high psychiatric disorder. I took people who were very high in stress. I divided them into two groups: one with low GSI scores (low psychiatric symptotology), and the second group with high scores. I wanted to see what made the difference. I found out that some family characteristics, such as commitment, control, and family resources were very important. They also differed across the country. When you compared the rural areas, the urban areas and the refugees, we always thought that the refugees were the people who suffered most from stress, and who have the most psychiatric disorders. When you go to Ghaza strip and see the military occupation, you think that Ghaza might suffer more. But on the contrary, I found that Ghazans were much better than the West Bank people, and the refugee camps were much better than the rural areas. My interpretation of this finding is that in the Ghaza strip and in the refugee camps they externalise what they have inside by rioting, throwing stones, and so on. This interpretation is made probable by another analysis which took age into consideration. The younger people, usually the youth, were found to be healthier - and it is the young people who are most involved in political activities. You see in this data that the political stresses did not effect the psychological distress of family members, instead it had a positive relationship. This implies that the more pressured you are with political stresses, you avoid other stresses such as violence in the family, drug addiction, alcoholism, and so on. There are many variations concerning education, income, and so on. It is very interesting how these combinations work together, and you can single out who gets better in what conditons and what resources. For instance, those who had the coping strategy of seeking spiritual support were much better, and this could be true for the Ghazans, because they differed from the West Bankers on this variable. The majority (of Ghazans) seek religious support. That is why I am taking ideology into consideration.

- *One of the important reasons why we use a combination of methods is to overcome the limitations of one method of data collection against the other, and the fashionable word we use is triangulation of methods. Each method of data collection is based on different epistemological assumptions. In your proposed study, how will you deal with the situation where your content analysis data distorts or contradicts your correlation coefficience or your test of significance?*

  I hope not to have such results, because the instruments have been developed by me, from my own experience in the area, and have not been taken straight off a book. But if it happens, we will have to go through the whole process again.

- *My question is regarding your sample size. Why did you take 140? You could have taken 70 and spent more time with them.*

  We wanted to include more - in the original proposal, the number was 300 - because we want representation from different places, and from a range of people. But I reduced it to 140, because 300 was a very large number.

- A *lot of people are concerned about the impact of this long-term systematic violence or violent environment on children. I wondered whether in your study you would be focusing on the impact, both positive and negative, on children's mental health, and especially on their attitude to violence and the legitimising of violence in conflict.*

  Here we are not going to take children as children, because then the instruments should be different. But I have done some research on children for UNICEF and for other institutions and we are concerned about children and the effect of the political condition on their safety, well-being and mental health. We are taking all these into consideration, but in different studies.

CONCLUDING REMARKS BY MODERATOR: We have had a very interesting paper and discussion. Many of the questions raised crucial issues: What do you do with non-cooperative groups? Why use focus groups at all? Why not do indepth study instead? What is the importance of

ethnographic report? When you have both, if they contradict each other, what are you going to do with the data? And also some issues such as the ethical aspect of payment; the positive aspects of coping with stresses. These enrichen our ideas about working with focus groups in the groups of people who have been subjected to violence and suffer more. It makes us understand the multi dimensional and complex nature of human behaviour and the factors involved while doing research.

Using Semi-Structured Interviews to Develop Guidelines for Gounselling Street Children

SHEKHAR SESHADRI AND STEPHEN ALOOR[1]

Background

A training programme was conducted over a six month period with the participation of street educators from over seven organisations involved with street children in Bangalore. Part of this programme involved counselling needs of street children. Trainees opted to take this on as a special area for further research and practice. This study was conducted by one participant using semi-structured interviews to evaluate aspects of resilience in street children.

Study Objective

The relationship of a street child with a street educator (SE) is unlike any social or therapeutic relationship. Contact is irregular and brief thus placing an additional burden on the SE to be alert to psychological issues over and above his other functions and also fulfill a counselling need. Since this aspect of street work is an acknowledged experience of most SEs, it was felt that any street counselling strategy would have to cover the child's ability

[1] Stephen Aloor is Coordinator, National Research and Documentation, Bosco

to handle specific problems commonly encountered on the street. Thus, the general objective of this study was to identify factors contributing to resilient handling of problem situations in street children.

Study Design

Based on the experience of SEs, six major concerns of street children were identified as problem sources with psychological implications.

i. Physical illness
ii. Physical treat
iii. Job dismissal
iv. Relationship problems
v. Police trouble
vi. Basic needs

These constituted the agenda for semi-structured interviews with 10 street children (5 interviews were later discarded and data analysis covered the remaining 5). Each interview began with a priming statement making the child clearly aware that he was participating in a study which could benefit him and his friends. Each of the six areas listed in the agenda had an opening question (For example, Have you ever been physically sick?). In response to an affirmative answer, a series of questions were asked to describe the experience and identify resolving abilities (For example, What happened? What was it like? What did you do? or How did you handle that?).

Data Analysis

All interviews were audio-taped and later transcribed. The transcripts were then broken into narrative parts based on whether they represented the following:

i. Crisis
ii. Experience
iii. Process
iv. Resolution

An example of this process is as follows :

Have you been seriously sick in your past life?

Yes, once I had typhoid in 1981, while I was on the street — II Experience

Then what did you do?

Then I went to Fr. Binny — IV Resolution

I see

Then Sheeba took me to the hospital and I was admitted in Martha's (Hospital) — IV Resolution

What were your feelings that time?

If I had some of my own people they would have taken care of me — III Process

Did you have anyone whom you really trusted?

Yes, Fr. Binny — IV Resolution

What were your feelings about life?

I was thinking that this street life is so bad to live. — I Crisis

The resolution modes were separated and scrutinised to see if they indicated resilience. A categorisation of the resolution modes were also attempted.

Analysis in stage 2 focused on the context of each resolution statement, that is its contextual meaning. These possible meanings were discussed to cull out definitive aspects of resilience in them.

Findings

From five interviews (covering the six areas in the agenda with each child), 111 resolution statements were identified. These could be separated into 19 categories based on what the context represented. The common categories were :

i. Confident of eliciting social support
ii. Sense of self esteem
iii. Relationship to extended family
iv. Sense of personal skill in coping
v. Assertiveness
vi. Definitive action.

An example of the second stage analysis and discussion is as follows :

When the boy went through a problem of physical illness (typhoid) the thought came to him that he could go to the Father in the nearby street children center. Now let us take a look at the context of this statement. The following points may arise :

- That he has build up a relationship with the Father. Therefore he feels accepted
- A relationship with the house where Father is residing.
- A relationship with even the other boys who may be frequenting the place.
- That help could be got from such a place.

Therefore varied types of characteristics emerge from this statement. Still more could be discussed from the way he answered the question, the quickness, the loudness, the facial expression. All the above mentioned characteristics give us a message that the child has experienced mainly two things.

a. experience of relationship
b. experience of being socially supported.

Therefore, this statement tells us that what can really promote resilience is the experience of relationship.

Conclusion

Since the major aspect of resilience arising out of these interviews indicate relatedness to a social network, there is a move towards initiating action based research on Network theory and therapy. Also, the method of analysing narratives in this study has been documented in a manual for day to day work of the Street Educator.

Discussion Session

MODERATOR : MALAVIKA KAPUR

❍ *I am not too clear about narrative categories two and three. Were these categories inferred from your experience, or was it from any psychological or other literature? The difference between experience and process is not very clear to me because everything involves experience and everything is a process in a temporal context. Secondly, if you are trying to identify the psychosocial factors contributing to resilient handling, what about the psychological factors which have been consistently shown in the literature such as delay of gratification? It has been shown that children at 3 or 4 years, who have this ability to delay gratification consistently predicts mental health even in adulthood and later.*

It is true that there is a significant degree of overlap between process, experience and crisis; I made the distinction that every crisis is an experience, but all experiences are not crisis. There is a similar distinction between a process, which is an internal dialectic, as opposed to an experience, which involves interactions with people or with institutions. To give another example, when we did the counselling training for street educators, one of the people who was represented was a doctor who has recently migrated to Bangalore from Hydrabad and is working in a counselling agency. One of the sessions we had was crisis experiences of participants themselves, the objective of which was to both recognise and understand emotional feelings and needs. We invited participants one by one to recount a personal experience of stress or vulnurability with responses and questions from the others. The moderator summarised each narrative under these areas. What one person said was, "I remember the time when my husband got a transfer". So what is the crisis? It is a life-change, it is the transfer. "We had to shift residence", which is also a crisis, a life-change, "and I also had to look for another job". This was the third life-change. "I felt very insecure, but decisions had to be taken" - this was the experience. "There was no escape from that"- a sense of no escape is a process. "Anyway, we had ten months time and my husband was very supportive, so we ultimately managed": this was the resolution - a time frame of ten

months and the fact that the husband is very supportive. I am not saying that these are watertight categories, nor are these inferred. These are based on literature on narrative analysis, which do not use the same constructs. In narrative analysis, the researcher is free to develop the categories on which to analyse the narratives, depending on the research agenda. If you are interested, there is an entire Sage series on qualitative research in the NIAS library, of which one of the books, written by Catherine Reisman gives the standard narrative methods, of which resolution is one of the modes. The other mode is called coda, and she describes what a coda means.

As far as delay of gratification is concerned, I agree that it is there in the general psychological literature on child developement. But that was not our intention here, and this is only the beginning of research into the specific area of street children. We did not take some of the earlier aspects into consideration, because our aims were very specific.

❍ *I have a problem about clubbing the two groups of street children together under the term "street-children"- there are specific groups of people who live with their families on the streets and there are people who live off the streets. If I understand you right, I think you said you have taken both the categories.*

No, the children in our study are all living on the streets, and have no immediate connections with their families.

❍ *We have a project in Brazil on street children who are facing a lot of violence. They have no relationship with their families, they are harassed by the police, and they are sometimes even killed by merchants who consider them a nuisense. I would therefore like to know the age group of the street chidren in your project. I also have the same question - are you talking about street children with attachment to families or street children who do not have any attachments. There would be a real qualitative difference between the two.*

The children interviewed for this project had no attachments to the family. All these children were between ages 9 to 15. Are you saying that that is a big range?

the software programmes. Usually, it takes an average period of 2 to 3 days for a computer literate audience to sufficiently master any one of the various programmes. This workshop is therefore a glimpse at how such newly emerging technologies can assist qualitative research.

For the remaining part of this chapter, I shall be using the term CAQDAS as an accepted abbreviation for Computer Assisted Qualitative Data Analysis.

Some Assumptions (Crabtree et al 1992):

Although computers have been used for several decades in analysing numerical data, their use in analysing prose data is recent and relatively less developed. Unfortunately, this has influenced our image of what computers as research tools can accomplish. Computers can facilitate analysis of data but cannot do it by themselves. They have to be told what to do. Computers do not analyse data, but they help manage it.

With proliferation of computer programmes and computer hard ware technologies, it is becoming difficult for many programmes and computers to talk to each other as they speak different langugages. And as data grows in complexity, the more difficult it gets to translate accurately from one programme to another. So it becomes more important to know what your needs are before you invest in a particular software.

A single keystroke can help accomplish a calculation that may manually take weeks or months, but that may not be what you want. And with large volumes of data, this could easily build into huge computer files taking up valuable disc space together with heaps of printed paper that might well confuse the researcher.

And finally, if data analysis requires simpler manipulation such as searching and retrieving key words, manipulating or joining texts etc, it is a waste to go for an expensive hardware and or a sophisticated CAQDAS package.

Preparation

Fundamental to any CAQDAS is the 'raw' material which consists of field notes and other ethnographic material. There is no substitute for rich data obtained through good field work involving accurate observation and detailed note keeping. Transcription of audio and video tapes in a similar manner is about typing out or writing down everything from tape, including pauses and interruptions during the interview and adding secondary notes at this stage.

This raw material or data has to then be processed and cleaned in order to given it a structure and coherence. Spell checks from popular word processing software programmes could be used but many do not recognise foreign words or jargon of a particular speciality so you may have to consult a dictionary. This is also the time to substitute names and places of your informants to protect their anonymity. Following this, the data has to be converted into some sort of electronic format such as typing into a common word processor package that your CAQDAS can read. It is useful to leave wide margins for notes or editing a printout. Technologies such as scanning may well make it easier and substitute typing, but in my experience, this process is not entirely accurate and is currently still in a stage of infancy.

Following the preparation of your material or prose information, you are ready to begin using CAQDAS. Most CAQDAS packages have built in coding schemes and I shall demonstrate this in the HyperRESEARCH software (Appendix I).

Using CAQDAS

Before using CAQDAS, the following questions need to be asked by researchers. The answers will help you in the selection of your computer technology. For instance:

How computer literate is the researcher?
Has the researcher a link or access to computer technology?
What is the project timetable and budget?
Is the researcher working alone or on a team?
For what audience is the analysis intended?

For those who are already computer literate:
What kind of computer user am I?
Am I choosing for one project or for the next few years?
What kind of projects and databases will I be involved with?
What kind of analysis am I likely to do?

Are the data sources single or multiple?:

- Single versus multiple cases
- Fixed versus open data
- Structured versus open data
- Uniform versus diverse entries
- Size of the data base.

What kind of analysis is to be facilitated?:

- Exploratory versus confirmatory analysis
- Coding schemes that are firm at the outset and those that evolve
- Multiple versus single coding
- Iterative coding or a single pass
- Granularity of analysis
- How important the context of the data will be
- How does one wish to have the data displayed
- Is the analysis purely qualitative or does it include numbers

TYPES OF CAQDAS PACKAGES

Text Retrievers :

- Metamorph
- The Text collector
- Word Cruncher
- ZyINDEX
- SONAR Professional

Text based Managers :

- askSAM
- FolioVIEWS
- MAX

Code-and-Retrieve Packages :

- HyperQUAL
- Kwalitan
- QUALPRO
- The Ethnograph

Theory-building Software :

- AQUAD
- ATLAS-ti
- HyperRESEARCH
- NUD-IST

Introduction to HyperRESEARCH (Excerpts from User's Manual)

Most qualitative researchers still analyse their data the old-fashioned way. After gathering their data, they transcribe the source materals with a typewriter or word processor, make multiple photocopies of the text, painstakingly read through and assign codes to the material, cut the pages up into coded passages, and then manually sort the coded text. This process can easily take a great many hours, days, weeks, or even months.

The manual method often leads to a number of pitfalls. The enormous amount of work involved in any study tempts the researcher to use smaller samples than might be desirable. A smaller sample size may lead to erroneous conclusions. Without a concrete method for describing the reasoning chain from the codes to the researcher's conclusion, independent verification becomes almost impossible.

HyperRESEARCH makes the manual labour and irreproducible reasoning clains common to qualitative studies a thing of the past. HyperRESEARCH aids the qualitative researcher not only in handling and coding the large quantities of data involved in a research project, but also in analysing the data and in reaching reliable, verifiable conclusions.

HyperRESEARCH permits the easy organisation, storage, retrieval, and analysis of coded materials. HyperRESEARCH lets you:

- Code any amount of data any number of times
- Retrieve and manipulate portions of coded source material
- Test propositions about the data on any code or combination of codes using Boolean searches
- Test hypotheses about the overall meaning of your data using artificial intelligence
- Print or export the retrieved data to a word processor, spreadsheet, or statistical package for more in-depth analysis.

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Note on discussion groups on mental health service delivery in cross cultural settings:

TRANSCULTURAL-PSYCHOLOGY is a forum on delivering mental health services in cross-cultural settings. The forum is operated on the Mailbase facilities at the University of Newcastle in England. Transcultural-Psychology is a discussion for anyone trained in predominantly Western theories of mental health, mental illness, and other problems in living who now finds themselves delivering services to people of other cultures. We especially welcome comments from traditional healers and non-

Western practitioners on their encounters with practitioners trained in the Western tradition. We invite contributions from anyone involved in mental health service delivery, including (but not limited to) psychologists, psychiatrists, social workers, nurses, researchers, academics, clergy and consumers of mental health services.

A sampling of topics discussed includes:

- Cross-cultural differences in definitions of mental health, mental disorder, and healing, and how these differences impact the assessment and remediation of mental illness and problems in living.
- Culture-specific syndromes, and the methods one might use to diagnose and treat such syndromes.
- Possibilities for collaboration between "Western" trained mental health practitioners, and traditional healers.
- The application or misapplication of Western mental health techniques and ideology in cross cultural settings.
- The adequacy of various classification systems (example: ICD-10 and DSM-IV) and assessment techniques in cross-cultural settings.
- The invariant and variant features of mental disorders across cultures.
- Whether and how our increasing emphasis on the biological aspects of mental disorders has modified the nature of our interventions in cross-cultural settings.

To join this open, unmoderated forum, send an e-mail message, without subject heading, to the Internet address: mailbase@mailbase.ac.uk

The only text in the body of your message should be the command: join transcultural-psychology [your first name] [your last name]

Transcultural-Psychology is a member of InterPsych, a consortium of discussion forums, real time conferences, electronics publications, and other resources in abnormal psychology and psychiatry. To learn more about InterPsych, send the followiing command to: mailbase@mailbase.ac.uk send psychiatry InterPsych

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Discussion Session

MODERATOR : SHEKHAR SESHADRI

- *Do you need to have a hypothesis before going to your data, or do you allow your data to generate your hypothesis?*

  You can go both ways.

- *Each one of the software that you mentioned incorporates a logic in the organisation of the data? For example, NUD-IST implies that there is a tree structure. You always open your data through sub-dividing your data. Let me take the example you have used. You have a fruit, and the categories, juicy and dry. You take juicy and have another sub-division. You have to always have your data in such a way that you can subdivide a category, which is one type of logic.You may want to organise your data in other ways - in multivocal ways, which obviously has a different logic inherent in it. Will you say a few words about this type of constraint which is put on the calibration of the data?*

  Getting back to the NUD-IST, that provides a little bit more than what this provides - this is a very simple one. Let me say a few words about what NUD-IST does and maybe that will help what you are asking. NUD-IST allows you to store your data and your notes within your computer field, unlike this, where you process your data, you scan it in, and then you start building a hypothesis. With NUD-IST, you open your data, it is scanned in, while you are reading it, you can make notes on the side and there is on the menu bar something called MEMO. You click MEMO and store all of those into the MEMO bin. The other advantage of NUD-IST is that it is able to represent your data in a graphic form, in the form of a tree. Every branch is called a node and its divisions are called sub-nodes. Each node stands for a major overarching category and there are sub-categories within it.

  If the data is in a continuum and not exactly divisible into two, what will you do?

  Then it won't divide. It will just show you one particular line and tell you where the nodes are. But it will represent it as a tree with only one trunk going up. But then it will not make

much sense because you are looking for multiple categories. The whole idea of doing qualitative analysis is that you want multiple categories.

It is not necessary that everything should fall into categories, is it?

As opposed to what?

Dimensions. Logic can also be dimensional.

Right, but it is just the matter of plotting in another dimension. But you need to have some particular way of defining these dimensions. You can use these nodes, and with the curser you can move the nodes to another place, and the data will move along with the node inside the computer. But you cannot change the logic. It is going to be a binary logic.

CONCLUDING REMARKS BY MODERATOR: It is immediately obvious to me that in a workshop like this, some of the anxiety that is likely to be generated is on account of the wide diversity. On one hand you have this participant observer position - this whole business of the construction of the self, the dramaturgy, no agenda, lots of transcribing, and the other extreme is the inbuilt systems of logic within the computer technology. I do suspect that a lot of us are going to chose a middle level stretegy, unless you are an anthropological or sociological researcher in the classic style. But in any case, Dr. Kapur has made it clear that the intention of this workshop is to sensitise people to the extreme options available for those inclined to respond to them depending on their direction of interest. I think if that message has gone through, the workshop is successful.

Bibliography

Agar
1986 *Speaking of Ethnography.* Delhi: Sage Publications

Agar, M.
n.d. Microcomputers as field tools. *In* Computers and humanities, 17: 19-26.

Alasdair M.
1981 *After Virtue,* University of Notre Dame Press, Indians,

Babbie,
1986 The Practice of Social Research. Belmont, California: Wadsworth.

Bales, R. F., and P. E. Slater
1955 Role Differentiation in Small Decision-making Groups. In *Family, Socialization and Interaction Process,* edited by Talcott Parsons and R. F. Bales. New York: Free Press.

Bannister P, Burman E, Parker I, Taylor M & Tindall C:
1994 *Qualitative Methods in Psychology* - A Research Guide. Buckingham. Open University Press

Bauer, J.P.
1993 Memory for gender consistent and gender inconsistent event sequences by twenty five month old children. In *Child Development,* 69 (1): 285-297.

Bebbington, P.
1980 Causal Models and Logical Inference in Epidemiology Psychiatry. In *British Journal of Psychiatry,* 136 :317-325.

Becker, H. S. et al.
1984 Fieldwork with the computer. In *Qualitative Sociology*, 7(1,2): 16-33.
Beels, and Ferber
1973 A context analysis of family interviews. A film distributed by Audio Visual Services, Pennsylvania State University.
Bellenger, D. N., K. L. Barnhardt, and J. L. Goldstucker
1976 Qualitative research in marketing. Chicago: American Marketing Association.
Bernard, H., and M. Evans
1983 New Microcomputer T |techniques for Anthropologists. In *Human organisation*, 42: 182-185.
Bhattacharyya, D. P.
1986 Pagalami: Ethnopsychiatric Knowledge in Bengal. New York: Syracuse University Press.
Bion, W. R.
1977 *Seven Servants*, Jason Aronson, New York.
Birdwhistell, Ray L.
1955 Kinesic Analysis of Filmed Behaviour of Children. In *Transactions* (Conference on group processes), edited by B. Schaffner, pp. 141-145.
1970 Kinesics and Context. Philadelphia: University of Pennsylvania Press.
Bourdieu, Pierre *et al*,
1993 *La Misere du Mode*, Seuil, Paris.
Brent, D.
1984 Qualitative computing: approaches and issues. *In* Qualitative Sociology, 7(1,2): 34-60.
British Medical Association
1986 Report on alternative medicines.
Bryman, A. and R. Burgess, editors
1994 Analyzing qualitative data. Routledge, New York.
Burman
1988 Killing time, losing ground: experiences of unemployment. Toronto, Ontario: Wall and Thompson.
Byers, Paul
Still photography in the systemic recording and analysis of behavioural data. *In* Human organization, pp.78-84.
Casti, J.L.
1994 Complexification: explaining a paradoxical world through the science of surprise. Harper Collins.
Chiozzi, Paolo
1989 Reflections on Ethnographic Film with a General Bibliography. In *Visual Anthropology.*

Christakis, N., Ware, N., and Kleinman
1994 Illness behaviour and the health transition in the developing world. In *Health and social change in international perspectives*, edited by L. C. Chen et al. Cambridge: Harvard University Press.
Clifford, J.
1988 Predicament of Culture: Twentieth-Century Ethnography, Literature and Art. Cambridge and London: Harvard University Press.
Cole, P. M., K. C. Barrett, and Zan-Waxler
1992 Emotion displays in two year olds during mishaps. In *Child Development*, 63 (2): 314-324.
Conrad, P., and S. Reinharz, editors
1984 Computers and Qualitative Data. Human Sciences Press.
Conrad, P., and R. Shulamit
1984 Computers and Qualitative Data: An introduction. In *Qualitative Sociology*, 7(1,2): 3-15.
Constas, M.
1992 Qualitative analysis as a public event: the documentation of category development procedures. In *American Education Journal*, 29 (2): 253.
Corin E
1990 Facts and Meaning in Psychiatry : An Anthropological Approach to the Life World of Schizophrenics Culture, Medicine & Psychiatry :14:153- 88.
Corin, E.
In Press The cultural frame: Context and meaning in the construction of health. In *Society and health*, edited by Sol Levine. Oxford University Press.
Corin, E., G. Bibeau, R. Laplante and J. C. Martin
1990 Comprendre pour Soigner Autrement: Reperes pour Regionaliser les Services de Sante Mentale. Montreal: Presses de l'Universite de Montreal.
Crabtree, B. F., and W. L. Miller, editors
1992 *Doing Qualitative Research*. In *Research Methods for Primary Care*. Volume 3. Sage.
Daedalus, Special Issue
1996 Social Suffering 1996, Winter Issue
Daft, L., and M. Steers
1986 Organizations: A Micro/Macro Approach. Glenview, Illinois: Scott, Foresman.
Das, Veena
1990 "Our Work to Cry: Your Work to Listen" in *Mirrors of Violence: Communities, Riots, and Survivors in South Asia* (ed. Veena Das) Oxford University Press, Delhi, p. 344-98.

1991 "Composition of the Personal Voice: Violence and Migration", *Studies in History*, p. 65-77.
1994 "Modernity and Biography: Women's Lives in Contemporary India", *Thesis Eleven*, 1994, P.52-63.
1995 Critical events. Delhi: Oxford University Press.

Dennis, D. L.
1984 Word-crunching: an annotated bibliography on computers and qualitative data analysis. In *Qualitative Sociology* 7(1,2): 148-56.

Denzin N. K, Lincoln U.S.
1994 *Handbook of Qualitative Methods*, London, Sage Publications

Denzin, N.
1970 The Research Act: Theoretical Introduction to Sociological Methods. Chicago : Aldine.

Denzin, N. K., and Y. S. Lincoln
1994 Handbook of Qualitative Research. Sage, London.

Desjerlais, Robert J, Arthur Kleinman and Leon Eisenberg,
1995 *World Mental Health Report*, New York,Oxford University Press.

Dey, I.
1993 Qualitative data analysis: a user-friendly guide. Routledge, New York.

Drass, K.
1980 The Analysis of Qualitative data: a Computer Programme. In *Urban life* pp.332-353.

Eco, Umberto
1984 Semiotics and the Philosophy of Language. Indiana University Press, Bloomington.

Emery, J. H
1976 Distinguishing functional and historical causality in anthropology. In *Anthropos*, 781(2): 180-225.

Fabes, R. A., and N. Eisenberg
1992 Young children's coping with interpersonal anger. In *Child Development*, 63 (1): 116-128.

Fabrega, H.
1989 On the significance of an anthropological approach in schizophrenia. In *Psychiatry*, 52: 45-65.

Fanon, F.
1961 Les damnes de la terre. Paris: Maspero.

Fernald, A., and H. Morikawa
1993 Common themes and cultural variation in Japanese and American mothers' speech to infants. In *Child Development*, 64 (3): 637-656.

Fielding, N., and R. Lee, editors
1991 Using computers in qualitative research. Sage.

Fielding, N., and R. Lee
1995 The hypertest facility in qualitative analysis software. In *Data Archive Bulletin*, January.

Fielding, N.
1995 Choosing the right software package. *In* Data archive bulletin, January.

Filstead, W. J. editor
1970 Qualitative Methodology. Chicago: Markham.

Fitzpatrick R., et al
1984 The Experience of Illness. London: Tavistock Publications.

Fivush, R., J. Kuebli, and P.A. Clubb
1992 The Structure of Events and Event Representation: A Developmental Analysis. In *Child Development*, 63 (1): 188-201.

Flaherty, J. A., F. M. Caviria, D. Pathak, et al.
1988 Developing Instruments for Cross-Cultural Psychiatric Research. In *The journal of nervous and mental disease* 176 (5): 257-263.

Foulks E. F., et al.
1986 The effect of patients' beliefs about their illnesses on compliance in psychotherapy. *In* American journal of psychiatry, 143: 340-344.

Furnham A., and P. Bower
1992 A comparison of academic and lay theories of schizo phrenia. *In* British Journal of Psychiatry, 161: 201-210.

Gaines, A. D. editor
1992 Ethnopsychiatry: the cultural construction of professional and folk psychiatries. New York: State University of New York Press.

Geertz, Clifford
1973 The interpretation of cultures. New York: Basic Books.

Geertz, Clifford
1988 Works and lives: the anthropologist as author. Stanford: Stanford University Press.

Gerson, E.
1984 Qualitative research and the computer. In *Qualitative Sociology*, 7(1,2): 61-74.

Gilbert, D.
1995 Between two cultures: geography, computing and the humanities. *In* Ecumene, 2(1): 1-14.

Goffman, E.
1976 Replies and responses. In *Language and society*, 5, 257-313.

Good, B.
1994 Medicine, rationality and experience. Cambridge, Cambridge University Press.

Good, B. J.
1977 The heart of what's the matter : The semantics of illness in Iran. In *Culture, Medicine and Psychiatry,* 1 : 25-28.
Goodenough, Ward H.
1981 Culture, Language and Society. Menlo Park, California: Benjamin Cummings Publishing.
Greenstein, D.
1994 A Historian's Guide to Computing. Oxford University Press.
Griaule, M.
1957 Methode de l'ethnographie. Paris: Presses Universitaires de France.
Griffin, H. J.
1961 Black like me. New York: Random.
Halasz.G
1994 Clinical Practice beyond Science : debunking the Scientific myth Australia and New Zealand Journal of Psychiatry. 28:7-13.
Hamel, J
1993 Case Study Methods. Delhi: Sage Publications
Hammersley M & Atkinson.P
1983 Ethnography -Principles in Practice London Routledge
Harding, S.
1991 Whose Science, Whose Knowledge? Thinking women's lives. Milton Keynes: Open University Press
Harris, Marvin
1974 Why a perfect knowledge of all the rules one must know to act like a native cannot lead to the knowledge of how natives act. In *Journal of Anthropological Research* 30 (4): 242-251.
Harvey, John, and Gifford Weary
1984 Current issues in attribution theory and research. In *Annual review of psychology* 35: 427-59.
Heider, K.
1976 Ethnographic film. Austin: University of Texas Press.
Henwood, K. L., and N. F. Pidgeon
1992 Qualitative research and psychological theorising. In *British Journal of Psychology,* 83: 83-111.
Henwood, K. L., and N. F. Pidgeon
1995 Grounded theory and psychological research. In *The Psychologist,* 8 (3): 115-118. Hess, John M
1968 Group interviewing. In *New Science of Planning,* edited by R. L. Ring. Chicago: American Marketing Association.
Hesse-Beiber, S., P. Dupuis, and S. Kinder
1991 HyperRESEARCH: a computer program for the analysis of qualitative data with an emphasis on hypothesis testing and multimedia analysis. *In* ualitative Sociology, 14 (4): 289-306.
HiggenboTham, J. B., and K. K. Cox editors.
1979 Focus group interviews: a reader. Chicago: American Marketing Association.
Higginbotham N., and D.L. Streiner
1992 The social science contribution to pharmaco-epidemiology. In *Journal of Clinical Epidemiology,* 44, Supplement 2:73-82.
Holy, L., and M. Stuchlik
1980 The structure of folk models. New York: Academic Press.
Hong, L.
1984 List processing free responses: analysis of open-ended questions with word processors. In *Qualitative Sociology,* 7(1,2): 98-109.
Hughes, L., and S. Less
1994 CTI centre for textual studies resources guide. (Easiest to get hold of by e-mailing ctitext@vax.ox.ac.uk.)
Hunt L., et al.
1989 View of what's wrong: diagnosis and patients' concepts of illness. In *Social Science Medicine,* 9: 945-956.
Ilfeld, F.
1976 Further validation of a psychiatric symptom index in a normal population. In *Psychological Report,* 39, 1213-1228.
Jadhav S. S.
1986 Help seeking behaviour, choice of healers and explanatory models. M.D.Thesis, National Institute of Mental Health and Neurosciences, Bangalore University, India.
Jadhav, S.
1993 Computer software for medical anthropologists. *In* British medical anthropology society newsletter, Summer issue, n.s.(I): 18-22.
Jadhav S. S. and Littlewood R. L.
1994 Defeat depression campaign: some medical Anthropological queries. In *Psychiatric bulletin,* 18(9): 572-573.
Jadhav S. S.
1995 The ghostbusters of psychiatry. Editorial Commentary. In *The Lancet,* 341, 1 April: 808-810.
Jasper. K
1963 General Psycholopathology Translated by Hoenig. J. Manchester Manchester University Press
Kaberry, Phyllis
1964 Malinowski's contribution to field-work methods and the writing of ethnography. In *Man and culture,* edited by Raymond Firth. New York: Harper and Row.

Kapur, M., S. R. Girimaji, G. G. Prabhu, G. N. N. Reddy, and V.G. Kaliaperumal
1994 Home environment and psychosocial development of pre-school children in south India. In *NIMHANS Journal*, 12: 9-18.

Kay, W.
1994 Geomagnetic storms: association with incidence of depression as measured by hospital admission. In *British journal of psychiatry*, 164: 403-409

Kau, Yeun-Hoi Kau, and Thomas J. Richards
1994 Knowledge representation for grounded theory: construction in qualitative data analysis. In *Mathematical and Sociology*, Volume 19, No.4, Gordon Breach, Amsterdam 1994.

Khamis, V.
1995a Coping with stress: Palestinian families and *Intifada*-related trauma. Manuscript submitted for publication.
1995b Psychological sequelae of *Intifada*-related trauma in Palestinian families. Manuscript submitted for publication.
1995c Psychological distress and well-being among traumatized Palestinian women during the *Intifada*. Manuscript submitted for publication.

Kirk, R.
1981 Microcomputers in anthropological research. In *Sociological Methods and Research*, 9: 473-92.

Kirmayer, L.
1989 Cultural variation in the response to psychiatric disorders and emotional distress. In *Social Science and Medicine*, 29: 327-339.

Kleinman, A.
1987 Rethinking psychiatry: from cultural category to personal experience. New York: Free Press.
1988 The Illness Narratives : Suffering, Healing and The Human Condition. New York Basic Books.

Kleinman, A.
1975 Explanatory models in health care relationships. In *National Council for International Health: Health of the family.* Washington, D.C.: National Council for International Health, pp. 159-172.
1980 Patients and healers in the context of culture. Berkeley: University of California Press.
1986 Some uses and misuses of social sciences in medicine. In *Meta-theory in Social Science*, edited by R. Shweder. Chicago: University of Chicago Press, pp. 222-245.

1988a The illness narratives. New York: Basic Books.
1988b Rethinking psychiatry: from cultural category to personal experience. New York: Free Press.
1992 Local worlds of suffering: an interpersonal focus for ethnographies of illness experience. In *Qualitative Health Research*, 2(2): 127-134.
1994 Anthropological perspective on objectivity: observation, categorization and the assessment of suffering. In *Health and Social Change in International Perspective*, edited by L. C. Chen et al. Cambridge: Harvard University Press.
1995 Writing at the margin: discourses between anthropology In Press and medicine. Berkeley: University of California Press.

Kleinman, A.
1980 Patients and healers in the context of culture. Berkeley: University of California Press.
1986 Social origins of distress and disease: depression, neurasthenia and pain in modern China. China, New Haven and London: Yale University Press.
1987 Anthropology and psychiatry. In *British Journal of Psychiatry*, 151: 447-454.

Kleinman A.
1978 The failure of Western medicine. *In* Human Nature, 1: 63-68.
1980 Patients and healers in the context of culture: An exploration of the borderland between anthropology, medicine and psychiatry. Berkeley: University of California Press.
1990 Rethinking psychiatry: six anthropological questions. New Haven: Yale University Press.

Kleinman, A., L. Eisenberg, and B. Good
1978 Culture, illness and care. In *Annals of International Medicine*, 88: 251-258.

Kleinman, A. and J. Kleinman
1991 Suffering and its professional transformation. In *Culture, medicine and psychiatry*, 15(3): 275-301.
1994 How bodies remember. In *New Literary History*, 25(3): 707-924.

Koloss, H. J.
1983 The ethnological film as a medium of documentation and as a method of research. *In* Methodology in anthropological filmmaking, edited by N. C. R. Bogaart and H. W. E. R. AKetelaar. Gottingen: Herodot.

Korsch, B.
1968 Gaps in doctor-patient communication: doctor-patient interaction and satisfaction. In *Paediatrics*, 31: 632-634.

Krippendorf, K.
1980 Content analysis: an introduction to its methodology. Beverly Hills, California: Sage.
Krueger, R. A.
1988 Focus groups: a practical guide to applied research. Newbury Park, California: Sage.
Lajoux, J. D.
1976 Le film ethnographique. In *Outils d'enquete et d'analyse anthropologiques*, edited by R. Cresswell and M. Godelier. Paris: Maspero.
Langer, J.
1978 Clients: check qualitative researcher's personal traits to get more. In *Marketing News*.
Langer, L.
1991 *Holocaust Testimonies: The Ruins of Memory*, New heaven, Yale University Press.
Langness, L. L., and Frank Gelya
1981 Lives: an anthropological approach to biography. Novato, California: Chandler and Sharp.
Leighton, A. H.
1959 My name is Legion: foundation for a theory of man in relation to culture. *In* The Stirling County Study, Volume I. New York: Basic Books.
1991 Social science and psychiatric epidemiology: A difficult relationship. Lecture to the psychiatric epidemiology and social sciences world psychiatric association, Oslo.
Leighton, A. H., J. S. Harding, D. B. Macklin, A. M. Macmillan, and A. M. Leighton
1963 The character of danger: psychiatric symptoms in selected communities In *The Stirling Country Study*, Volume III. New York: Basic Books.
Littlewood R.
1990 From categories to contexts: a decade of the 'new cross-cultural psychiatry'. In *British journal of psychiatry*, 156: 305-307.
1990 Comparison of stigmatising attitudes to psychiatric illness across communities. In *Medical sociology*, (Awaiting publication).
Littlewood, R.
1990 From categories to contexts: a decade of the 'New cross-cultural psychiatry'. In *British Journal of Psychiatry*, 156: 308-327.
1991 Against pathology: the new psychiatry and its critics. In *British Journal of Psychiatry*, 159: 696-702.
1993 Pathology and Identity. Cambridge: Cambridge University Press.

1995 Psychopathology and personal agency: modernity, culture change and eating disorders in South Asian societies. In *British Journal of Medical Psychology*, 68: 45-63.
Lomax, Alan
1973 Cinema, science and culture renewal. In *Current Anthropology* 14 (4):474-480.
Lomax, A., I. Bartenieff, and F. Paulay
1969 Choreometrics: a method for the study of cross-cultural pattern on film. In *Research film*, 6: 505.
Lynd, Robert and Lynd Helen
1929 Middletown. New York: Harcourt.
1937 Middletown in transition. New York: Harcourt.
Malinowski, B.
1922 The Argonauts of the Western Pacific. London: George Routledge and Kegan Paul.
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1922 The Argonauts of the Western Pacific. London: George Routledge and Kegan Paul.
Marsella, A., G. Devos, and F. Hsu, editors
1985 Culture and self: Asian and Western perspectives. New York: Tavistock.
Marsella, A. and G. White, editors
1982 Cultural conceptions of mental health and therapy Dordrecht: Reidel.
Marsella, A., G. Devos, and F. Hsu, editors
1985 Culture and self: Asian and Western perspectives. New York: Tavistock.
Marsella, A. and G. White, editors
1982 Cultural conceptions of mental health and therapy Dordrecht: Reidel.
Marshal, C., and G. Rossman
1989 Designing qualitative research. Newbury Park: Sage
McCracken, G.
1988 The long interview. Delhi: Sage Publications
Merton, R. K.
1946 The focused interview. In *American Journal of Sociology*, 51.
1987 Focused interviews and focus groups: continuities and discontinuities. In *Public Opinion Quarterly*, 51.
Merton, R. K., M. Fiske, and A. Curtis
1946 Mass persuasion. New York: Harper.
Merton, R. K.
1946 The focused interview. In *American Journal of Sociology*, 51.
1987 Focused interviews and focus groups: continuities and discontinuities. In *Public Opinion Quarterly*, 51.

Merton, R. K., M. Fiske, and A. Curtis
1946 Mass persuasion. New York: Harper.
Merton, R. K., M. Fiske, and P. L. Kendall
1956 The focused interview. New York: Free Press.
Merton, R. K., M. Fiske, and P. L. Kendall
1956 The focused interview. New York: Free Press.
Michael, A.
1982 Towards an ethnographic language. In *American anthropologist*, 84(4): 779-795.
Miles, M., and E. Weitzman
1994 Technical appendix. In *M. Miles and A. Huberman*, Qualitative Data Analysis. Beverly Hills: Sage.
Miles, M., and E. Weitzman
1995 Computer programmes for qualitative data analysis. Beverly Hills: Sage.
Mischler, E.
1984 The discourse of medicine: dialectics of medical interviews. Norwood, New Jersey: Abbex.
Mischler, E.
1984 The discourse of medicine: dialectics of medical interviews. Norwood, New Jersey: Abbex.
Mixon D
1974 'If you won't deceive, what can you do?' in Armistead.N (ed) Reconstructing Social Psychology. Harmondsworth, Penguin.
Mixon D
1974 'If you won't deceive, what can you do?' in Armistead.N (ed) Reconstructing Social Psychology. Harmondsworth, Penguin.
Morgan, D.L.
1988 Focus groups as qualitative research. Newbury Park, California: Sage.
1993 Successful focus groups: advancing the state of the art. Newbury Park, California: Sage.
Morgan, D.
1988 Focus groups as qualitative research. Newbury Park : Sage.
Morgan, D. L.
1988 Focus groups as qualitative research. Newbury Park, California: Sage.
1993 Successful focus groups: advancing the state of the art. Newbury Park, California: Sage.
Morgan, D. L.
1988 Focus groups and qualitative research. Delhi: Sage Publications

Morris, D.
1992 The culture of pain. Berkeley: University of California Press.
1995 About suffering: voice, genre and moral community. In Press In *Daedalus*.
Morris, Desmond
1978 Manwatching. London: Grafton Books
Morris, Desmond
1978 Manwatching. London: Grafton Books
Morris, D.
1992 The culture of pain. Berkeley: University of California Press.
1995 About suffering: voice, genre and moral community. In Press In *Daedalus*.
Moustakas, C.
1990 Heuristic research: Design, methodology and applications. Delhi: Sage Publications
Mudimbe, V. Y.
1988 The invention of Africa. Bloomington and Indianapolis: Indiana University Press
Mudimbe, V. Y.
1988 The invention of Africa. Bloomington and Indianapolis: Indiana University Press
Muhr, T.
1995 Atlas-ti: a prototype for the support of text interpretation. In *Qualitative Sociology*, 14 (4): 349-71.
Pan American Health Organization
1993 Promoting equity: a new approach from the health sector. PAHO, Washington, D.C.
Padilla, R.
1995 Using computers to develop conceptual models. In *Qualitative Sociology*, 14 (3): 263-74.
Pagels, H. R.
1988 The dreams of reasons: the computer and the rise of the science and complexity. Simon and Schuster.
Pandolfi, M.
1990 "Boundaries inside the body: Women's suffering in southern peasant Italy", *Culture, Medicine and Psychiatry*, (14), p. 255-273.
Pandolfi, M.
1991 Itinerari delle emozioni. Corpo e identita femminile nel Sannio campano. Milano: FrancoAngeli.
Peter, Laurence, and R. Hull
1969 The Peter principle: why things always go wrong. New York: William Morrow.

Pfaffenberger, B.
1988 Microcomputer applications in qualitative research. Beverly Hills: Sage.
Podolefsky, A., and C. McCarty
1983 Topical sorting: a technique for computer assisted qualitative analysis In *American anthropologist*, 84 (4): 866-71.
Polunin, I.
1965 Stereophonic magnetic tape recorders and the collection of field data. In *Current Anthropology*, 6: 227-230.
Polunin, I.
1970 Visual and sound recording apparatus in ethnographic field work. In *Current Anthropology*, 11: 3-32.
Radcliffe-Brown, A. R.
1922 The Andaman Islanders. Cambridge University Press.
Raguram. R.
1993 An exploratory study of coping styles in schizophrenic patients. *Indian Journal of Psychiatry*, 35, 22-26
Reid, A.
1992 Computer management strategies for text data. In *Doing qualitative research*, edited by B.F. Crabtree and W. Miller, pp. 125-45. Beverly Hills: Sage.
Riessman, C. K.
1993 Narrative analysis. Delhi: Sage Publications
Reissman, C. K. ,
"Making sense of marital violence: One woman's narrative" in *Storied Lives, op. cit.*, p.231 - 250.
Richardson, S., B. Dohrenwend, and D. Klein
1965 Interviewing: its forms and functions. New York: Basic Books.
Ricoeur, P.
1976 Interpretation theory: discourse and the surplus of meaning. Fort Worth: Texas Christian University Press.
Ricoeur , P.
1984 *Time and Narrative*, Vol. 1, Chicago, University of Chicago Press.
Ridgeway, C. L.
1983 The dynamics of small groups. New York: St. Martin's.
Riesman, D.
1950 The lonely crowd: a study of the changing American character. New Haven, Connecticut: Yale University .
Robins, K., and F. Webster
1989 The technical fix: computers, industry and education. Macmillan.

Rosenwald, G. C.
1995 "Conclusion: Reflections on narrative self-understanding", in *Storied Lives: The Cultural Politics of Self Understanding*. (Ed. George C. Rosenwald and Richard L. Ochberg, Yale University Press, New heaven, 1995.
Rouch, J.
1975 The camera and man. *In* Principles of visual anthropology, edited by Paul Hockings. The Hague: Mouton.
Said, E.
1978 Orientalism. New York: Pantheon.
Sartorius, N. A., Jablensk, and R. Shapiro
1978 Cross-cultural differences in the short term prognosis of schizophrenic psychoses. In *Schizophrenia Bulletin*, 4: 102-113.
Scheflen, A. E.
1964 The significance of posture in communication systems. *In* Psychiatry, 27:316-331.
Schwartz, D., K.A. Dodge, and J.D. Coci
1993 The emergence of chronic peer victimisation in boys play groups, In *Child development*, 64 (6): 1755-1772.
Seidel, J., and J. Clark
1984 The ethnograph: a computer programme for the analysis of qualitative data. In *Qualitative Sociology*, 7 : 110-125.
Seltiz, C., M. Jahoda, M. Deutch, and S. Cook
1965 Research methods in social relations. New York: Holt, Rinehart & Winston.
Silverman, D.
1993 Interpreting qualitative data: methods for analyzing talk, text and interaction. London: Sage Publications.
Spence, D.
1983 *Narrative Truth and Historical Truth*, University of Carolina Press.
Sperger, D., and D. Wilson
1982 Mutual knowledge and relevance in theories of comprehension. In *Mutual knowledge*, edited by Neil Smith pp.61-131. New York: Academic Press.
Spradley, J. P. editor
1972 Culture and cognition: rules, maps and plans. San Francisco: Chandler.
Sproull, L., and R. Sproull
1982 Managing and analyzing behavioural records: explorations in non-numeric data analysis. In *Human organisation*, 41: 283-90.

Srinivas, M. N.
1952 Religion and society among the Coorgs of South India. Oxford Clarendar Press.
1955 The social system of a Mysore village. In *Village India*, edited by McKim Marriott. Chicago
1976 The remembered village. University of California Press.
Stewart, D., and P. Shamdasani
1990 Focus groups: theory and practice. Newbury Park, California: Sage.
Stifter, C. A., C. M. Coulehan, and M. Fish
1993 Linking employment to attachment : the mediating effects of maternal separation anxiety and interactive behaviour. In *Child Development*, 64 (5): 1451-1469.
Strauss, A. L., and J. Corbin
1990 Basics of qualitative research: grounded theory, procedures and techniques. Newbury Park, California: Sage.
Tallerico, M.
1991 Applications of qualitative analysis software: a view from the field. In *Qualitative Sociology*, 14 (3): 275-285.
Tesch, R.
1990 Qualitative research: analysis types and software. New York: Falmer.
Tesch, R.
1990 Qualitative research: analysis types and software tools. Falmer.
Tyler, Stephen
1969 Introduction. In *Cognitive anthropology*, edited by Stephen Tyler. New York: Holt, Rinehart and Winston.
Uchoa, E.
1993 Espace devolu, espace desire, espace revendique indifferenciation et folie d'Ajaratou. In *Folie et espace de sens*, edited by E. Corin. Anthropologie et Societes 17, Special issue, 1-2: 157-172.
Vattimo, G.
1987 Ermeneutica come koine, AUT AUT, 217-218: 3-11.
Weaver, A., and P. Atkinson
1994 Microcomputing and qualitative data analysis. Avebury.
Weber, M.
1946 Essays in sociology. New York: Oxford University.
Webster, F., and K. Robins
1986 Information technology: A Luddite analysis. Ablex.
Weiss M. G., A. Desai, S.S. Jadhav, et al.
1988 Humoral concepts of mental illness: patterns of conti nuity and change in India. In *Social Science and Medicine*, 5: 471-488.

Weiss M..G., and D. R. Doongaji
1992 The Explanatory Model Interview Catalogue (EMIC): contribution to cross-cultural research methods from a study of leprosy and mental health. In *British journal of psychiatry*, 160: 819-830.
Weiss, M. G., Raguram. R., & Channa Basavanna, S.M.
1995 Cultural dimensions of psychiatric diagnosis - a comparision of DSM 3R and illness explanatory models in South India, British Journal of Psychiatry 166: 353-59
Wells, W.
1974 Group interviewing. In *Handbook of Marketing Research*, edited by Rober Farber, pp. 133-146. New York: McGraw-Hill.
Westermeyer, J.
1989 Psychiatric epidemiology across cultures: current issues and trends. In *Transcultural Psychiatric Research Review*, 26, 5-25.
Whitehurst, G. J.
1979 Meaning and semantics in GJ Whitehurst and B J Zimmerman (Eds). The functions of language and cognition. New York, Academic Press.
Whyte, W. F.
1981 Street corner society. (3rd edition) Chicago: University of Chicago.
Wood, M.
1980 Alternatives and options in computer content analysis. In *Social Science Research* 9: 273-286.
World Bank.
1993 World development report: investing in health. New York: Oxford University.
World Health Organisation
1994 Qualitative research for health programmes. Geneva: WHO Division of Mental Health.
World Mental Health
1995 World mental health report: problems and priorities in low-income countries. Harvard Medical School. New York: Oxford University.

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