RESEARCHING THE BHOPAL GAS TRAGEDY: ETHNOGRAPHIC FIELDWORK, LITERARY PERSPECTIVE, AND THE POLITICS OF ENGAGEMENT

The Bhopal gas tragedy, both the accident that happened on 3 December 1984 and the medical/legal/social/environmental disaster that continues to this day, has generated a lot of literature that ranges from the scientific and legalistic to the purely newsworthy and the more popular human interest stories. At the same time, except for the occasional ethnographic studies taken up by scholars from eclectic fields, very little has actually emanated from social science departments of universities. Bhopal remains an unexplored territory for research, in areas as wide ranging as cultural anthropology, environmental studies and disaster management. Actually, Bhopal is the classic case study for ethnographic fieldwork. It offers an insight into the daily lives of the impacted community. It also showcases a long and relentless struggle for justice, by marginal people, especially women, against the monolithic power of the State and Corporation. It provides ample scope for studying the history of social movements in India, drawing on conflicts of class and gender.

In this paper, I take another look at some of the proven methods of ethnographic fieldwork in relation to rehabilitation discourse, so as to highlight the reasons why research on Bhopal was fraught with challenges right from the beginning. As a manmade disaster Bhopal involved both the multinational corporate giant and its Indian subsidiary, diplomatic relations between two nations, and third-world victims. Given the magnitude of the disaster an entire state mechanism was soon put in place to deal with the crisis situation. In the years that followed, a great deal was swept under the carpet, and the official process of documentation fed into a travesty of justice that was unprecedented in its scale and fallout. Research had to be undertaken against the backdrop of a highly fractious political arena, where people’s knowledge/ignorance came in direct conflict with the ideological state apparatus and its methods of controlling knowledge. In an important sense, any kind of fieldwork involved getting survivors their rights and helping in the paperwork for getting aid
from different rehabilitation schemes set up by the government. There was also a felt need to do research on sensitive issues such as health care and social and economic rehabilitation so as to carry on with the mammoth task of raising awareness and bringing the truth to light. The first decade saw a flurry of such activities that reached its peak with the outrage against the 1989 settlement. But soon after, public attention began to wane and many scholars and activists packed their bags and left Bhopal.

I came to Bhopal in 2003 with absolutely no experience of fieldwork. In 2002 I had published a very well received children’s non-fiction titled Bhopal Gas Tragedy, A Book for Young People. Conceptualised as a Supplementary Reader for high school students it was factual in nature, and was mainly based on research done from archival sources. I was keen that a generation born after the disaster should tackle issues rather than get immersed in tales of calamities. But I also knew that my involvement with issues would take me to the field as a researcher. In 2002 I attended the 3 December anniversary protest march in Bhopal for the first time, and met survivors and listened to their stories. I have to admit that in the beginning I absorbed the stories as a fiction writer, as tales of suffering and pain. But there was something raw and forceful about these narratives that compelled me to use formal methods of fieldwork, namely recording the interviews and keeping field notes. It began a long and abiding association with the people of the Bhopal tragedy, which continues to this day.

I soon found myself going back to renew contact with the people I was interviewing, so as to trace the continuity in their stories. I was formulating certain hypotheses that fed into the kind of questions I was putting to them. My research methodologies were increasingly becoming theoretical in nature, and not long after I was involved in the usual academic pursuit of writing papers and getting them published. If anything set me apart in my anthropological pursuit, it was my training as a literature person, who had taught Marlowe and Shakespeare to college students for the better part of her career. A fellowship at Nehru Memorial Museum and Library (2005 -2008) gave me the much needed break from teaching, enabling me to plunge headlong into fulltime research on the oral testimonials of the women survivors. In the course of research I got inducted into the Bhopal Movement Study Group, headed by Dr Eurig Scandrett from Queen Margaret University (QMU), UK, on a research project (August 2007- August 2008) on the Ethnography of the Social Movement, under the aegis of QMU, with two Research Assistants, Dharmesh Shah and Tarunima Sen, who worked with us. We were an eclectic group drawn from different disciplines with varying experiences of working at the ground level. I was able to draw on the strengths of a cross-disciplinary approach that is central to literary studies as well as anthropological and sociological research, which helped me straddle the gap between the different disciplines. Even as I formally learnt the methods of participant observation,
I discovered to my delight that my literary training in placing texts in their cultural milieu in terms of gender and class dynamics, proved very helpful. The fact that as a novelist I was an empathic story teller became an added asset.\textsuperscript{2}

The year 2009 marked the 25\textsuperscript{th} anniversary of the tragedy and much of my work was a lead-up to this crucial juncture in the history of the social movement. It defined the relevance of research that could take in the second generation survivors, children born with congenital defects, and the new category of victims of water contamination. The Bhopal Movement Study Group played a modest but timely role of opening doors once again for researchers and academics to come back to Bhopal. I need to mention here that Eurig used all his contacts in universities in UK to foster the study on Bhopal. We were picking up threads from the sort of pioneering work that had been done, but had virtually disappeared from the discursive field.\textsuperscript{3} The 1990s and year 2000 had seen India open its door to foreign investment so that the battle for justice intensified against state collusion with corporate crime. Bhopal had the potential to become the worldwide symbol of human rights violation, and therefore our task was to bring it back into the limelight. At the same time, my own interest in testimonial gathering brought me in close touch with methods of knowledge formation that was both grassroots and indigenous in nature. It created a rich, untapped baseline for anthropological research. A lot of this material was available with the survivor groups, who had meticulously kept newspaper cuttings, pamphlets, newsletters and related reports that had been printed and distributed by non-governmental organisations. It offered a close reading of how resistance had shaped itself at the level of the mohallas, and how in the years that followed, legal and medical information building happened through the intervention of researchers/professionals/social activists from nationwide and global social action groups. The yoking of knowledge with activism became the bugbear of the elite, ruling class. In the process, much of the people’s perspective came to be ignored or simply discounted. Therefore, my research was as much about retrieving the knowledge base that had been lost and bringing it back into the mainstream of formal, academic research.

In the final analysis, my task was to contribute to a rich tradition of information gathering and method of documentation that already existed in Bhopal, and thereby strengthen its foundations. The fact that this tradition was spearheaded by men and women from the marginal class, many of whom were barely literate, made it unique. In the interviews that I conducted, the survivors spoke about their need to endorse a gender sensitive, rights-based rather than a needs-based model for rehabilitation. Their demands included better health care through government run gas-rahat hospitals rather than private hospitals, more jobs that took into account their medical problems, proper clean up of the contaminated site using the polluters-pay principle, and more importantly bringing the company and its executives to book. In the
process they were able to critique the notions of development that placed profit before people. A large part of their activism involved the use of the vast repository of knowledge as means for countering misinformation and power politics. Insofar as my own research and fieldwork fed into this very process of retrieving the alternative vision of justice for people, it helped to define my own role as author/researcher and campaigner. And it was precisely in this context that I was able to evolve certain methods of ethnographic research that made fieldwork both dynamic and politically relevant. In the rest of the essay I would like to show how I borrowed from field tested techniques of carrying on research and yet broke the mould in many ways. As a researcher my task was not just confined to gathering people’s testimonials, but bringing them back to the people so that they could reclaim their own voices and use them as tools for self-development and empowerment.

The next step came fairly naturally. I was able to use the research material as a pedagogical tool and form a student group named “We for Bhopal” (WFB), which began from the English department in Hindu College, but soon had student-members from all over the University of Delhi. The idea was to sensitize youth on issues linked to the possibilities of more Bhopals’ happening in the future. I had come full circle in addressing the new generation. This essay is about personal growth and research methodologies that were shaped through challenges arising out of ethnographic fieldwork in the much less trodden area of a devastating industrial disaster.

My acquaintance with the works of well known ethnographers was limited to such classic texts as Sir James Frazer’s The Golden Bough, E. E Evans-Pritchard’s account of The Nuer, Clifford Geertz’s study of The Religion of Java and Claude Lévi-Strauss’s monumental two-volume work on Structural Anthropology. I read them strictly in relation to the influence they had on the poetry of Yeats, Eliot and Pound. I must admit that it was largely my readings of the novels of Conrad and Coetzee that strengthened my understanding of the ethnographic text. Once my research project on Bhopal was in place, I moved away from literary sources and revisited these subjects with a much sharper focus on the research methodologies being used for studying societies that were dispersed in time and location by the diffusion of cultures. Interestingly, Evans-Pritchard’s description (1951:33) of the “indispensable corpus of ethnographic facts” continued to evoke the popular image of the western anthropologists, working in far flung geographical locations, under very trying physical circumstances, which I had found in the works of fiction. In most of the case studies that were taken up, cultures were spoken of in terms of their intrinsic otherness. Cultural remoteness became the moot problem and all research methodologies were geared towards seeking solutions to it. The solutions in turn, closely negotiated the gap between a theoretical approach to the study of social structures, and an empirical approach based on observations of cultural practice in society (ibid.: 21-42).
While Evans-Pritchard felt the need to retrieve stories from the social sphere to the more abstract plane of structural relations, Levi-Strauss highlighted the importance of formulating theories that were applicable to all societies. In his own words, the task before an ethnographer was to enlarge a specific experience to the dimensions of a more general one, which thereby becomes accessible as experience to men of another country or another epoch (Levi-Strauss, 1963: 17).

Research methodology almost seamlessly becomes the politics of engagement and objectivity. Lévi-Strauss went on to compare a historian’s task with that of an ethnographer and concluded that it is only a matter of perspective. The ethnographer furnishes the documents the historian will use, and so he writes the history of a society from the perspective of “lived reality”. But does the historian trust the ethnographer’s less reliable use of shifting images? It is a question that is not really answered by structural anthropology. However, my own literature background kept me interested in the mystique of the text that the historian and the ethnographer were interested in creating, by drawing on both written records and firsthand observations.

I shifted attention to case studies that provided firsthand accounts of the field worker’s experiences, which in turn, became the basis for analysing research methodologies. Geertz’s fascinating study of the religious diversity of contemporary Java included an Appendix titled “A Note on Methods of Work” that proved to be really useful (Geertz 1960: 383-386). But it was not necessarily meant as a rulebook for students of ethnography. I was interested in what Geertz described as informal methods of participant observation activities, which included attending public events, meetings, cultural occasions, rituals, idle conversations in coffee shops and mosque yards, visiting families, and watching films together. He also drew attention to a large body of material that was less literal and verbatim, for it was based on paraphrase of what the informant was saying, rather than his/her exact words. Geertz underplayed the role of note-taking and the use of the tape recorder, but he was talking about the unique flavour of his own case study, which he felt was more descriptive than theoretical. It was helpful to deduce research tools from individual cases without necessarily comparing or contrasting them with my own.

It was the writings of James Clifford that guided me towards the critical stance that I was looking for. In *The Predicament of Culture* he spoke at length of the importance of field-work and the need to grasp “gestures empathetically” before “stepping-back” and situating the meaning in a wider context (Clifford 1988: 34). The importance given to experience over interpretation was a matter of strategic positioning of the ethnographer, who could lay claims to being an insider and participant. Also the rapport, empathy and acceptance that was built, helped in producing the authoritative text that gets written out. Clifford used the phrase “ethnographic thick description” (*ibid.*: 38) to describe the process through which what is unwritten gets “marked as a corpus”. In his
other seminal work *Writing Culture* Clifford states that writing is central to what ethnographers do in the field. It is not so much the clash between “transparency of representation” and objective methods of recording that is important; instead, text-making highlights “the constructed and artificial nature of cultural accounts” (Clifford 1986, 2). In an important sense, my own initiation into doing fieldwork in Bhopal was going through different stages. I was gradually moving away from a purely literary engagement with writing texts, to what Clifford traced as textual practices, which went beyond texts to the contexts of power and resistance. I was interested in the well-defined role played by the ethnographer in creating a shared, inter-subjective world through the political process of using texts as points of intervention. Perhaps, here was a solution at hand to the practical problems that Geertz had pointed out of reliability and balance.

Once I grounded my work in Bhopal I got introduced to recent works by Indian and western anthropologists working in India and South Asia. The context of fieldwork changed and I was particularly interested in theoretical positions that looked at cultural otherness while working in one’s own country. T. N. Madan’s statement that “one has to learn not only to live intimately with strangers but also to live (behave) strangely with intimates” (Madan 1994: 136) carried resonance in Andre Beteille’s concerns with the “The Tribulations of Fieldwork” (Beteille and Madan 1975: 99) and Srinivas’s eloquent testimony to fieldwork that took into account the conditions of working at the local level (Srinivas, Shah, Ramaswamy 2002: 19). Srinivas’s insistence on the importance of studying one’s own society over and above other cultures, laid the grounds for ethnographic work that looked at the on-going social processes within one’s own country.

The catchword was “intensive fieldwork” described in terms of “encounter” and “experience”. In both cases it is the absence of standardised techniques of fieldwork that is reiterated and is replaced by a shared understanding of purpose and methodology. Therefore, the ethnographer’s task is to share his/her experiences in the field and retrieve from it concrete and specific problems encountered in the field, which cumulatively can form guidelines for doing research. At the end of the day, individual predilections differ from person to person, so each experience becomes important. A lot of importance is given to on-the-spot techniques to deal with the unforeseen and intuitive insights rather than interpretative concepts. Field diaries become important tools of research overriding other technical aids like cameras and videotapes. Time and again it is the complexity of social reality that is highlighted, and the need to reframe theoretical concepts after scrutinising the field data.

It is Meenakshi Thapan who draws attention to “situated knowledge in fieldwork” (Thapan 1998, 7). According to her, communities that are studied in fieldwork become the “other” because ethnographers work in situations
and contexts that are far removed from their own world. So otherness is often posited as a methodological vantage point that works both ways, so the community is ‘other’ for the ethnographer and vice versa.\(^7\) The researcher is the participant observer who undertakes a lengthy and arduous process of enquiry that is time-bound. The role is that of an investigator who must then evolve strategies of data collection, pick up leads, select respondents and interview them in a systematic way. Acceptability, care, concern and involvement become important factors, so that anyone who has worked in the field talks of subjectivity as the very condition of work, which in turn determines research methodology. Empathy is replaced by interface with reciprocal sharing of confidences and building trust.\(^8\) B.S Baviskar puts it rather eloquently in the statement, “I witnessed evidence” (Srinivas, Shah, Ramaswamy 2002: 196). Thapan refutes the notion that an ethnographer’s main concern is academic and not political. Situated knowledge raises the issue of political engagement (1998: 21). She states categorically that in ethnography the other is not just found or encountered, it is made. So a methodological devise becomes the conditionality for doing research with its political consequences. This in turn highlights the agency of the other and leads to the discovery of people’s voices.

I entered the field with the knowledge that there are no readymade toolkits that can be used uniformly. If anything I had to supplement standard procedures and be innovative. Since my literature background made me a novice in the field, the question of what to begin with – theory or empirical data – became redundant. I was faced with the far more daunting task of deciding what I intended to do with the available data and theory. Bhopal gas tragedy was a well trodden area and I had to move cautiously. The only research tool available to me was the time tested ethnographic conceptual model for doing fieldwork. As a working model it required that I select data, identify problems, and put forward propositions that had an empirical referent point. The idea was that my hypothesis had to be amenable to empirical investigation. But as mentioned earlier I was entering a politically sensitive field that had seen twenty-six years of ruthless confrontation between a beleaguered community and powerful players. So what is a methodological problem in ethnography took me into the heart of the problem of doing research in Bhopal.

Traditionally, ethnography works on the premise that all lay explanations of phenomena have to be empirically tested to determine their veracity. A quantitative approach locks horn with a qualitative approach. In the case of Bhopal, since it was a chemical disaster, which had ramifications in the fields of medicine, judiciary, commerce, economic development, it got mired in scientific discourse that needed specialists, professionals and a bureaucratic set up to unravel its maze like properties. Therefore, right from the beginning state sponsored research projects with the necessary committee of experts had to be put in place. In disaster studies quantifying responses
that covers a large segment of the population become necessary. Given the vast numbers of the affected population, survey method was the chosen research tool. The prime focus was on gathering information by using prepared questionnaires, and wherever the response was inadequate due to factors, such as illiteracy, increasing number of deaths in a family and severely ill people, the sample size was decreased or increased accordingly. It was followed by a process of tabulation and classification that became the much touted “paper proof” that was necessary for identification of victims of the gas tragedy. Research became a tool of oppression that was used against people in the court of law and the tribunal offices for disbursing compensation.

As a manmade, industrial disaster Bhopal saw the worst kind of violation of human rights in the name of rehabilitation. Rehabilitation was linked to compensation and the identity card got equated to real and false claims, which became issues of governance and law and order. An air of suspicion was created and veracity in empirical research got linked with the idea of fraud. Most of the interviews that I conducted with state officials, doctors and lawyers became an open confrontation with issues relating to truth and falsehood in research. Ethnography has always problematized the soft, qualitative approach to research involved in fieldwork. It is often branded as non-scientific and subjective. In the case of Bhopal a methodological problem carried political connotations. I had to be cautious of the approach I adopted in fieldwork, because I was dealing with a community, which had lost everything and was wary of any outsider who came to talk to them. I had to bring in statistical analysis of social data by exploring the possibility of finding documentary evidences for the kind of violations that I was talking about. At the same time, I had to bring alive the myriad stories the survivors wanted to tell me. In doing so I had to deal with the less measurable aspects of cultural beliefs, attitudes and ways of life. The task before me was to integrate the two approaches by breaking out of the mould that sees all experiences through the ethnographic lens eye as objective/subjective divide.

I began by delving into statistical surveys, which created a baseline of survivors categorised by age, gender, religion, location, medical history and economic condition. The catch is that all research in Bhopal is dependent on such official surveys, done by the government. NGOs have conducted their own surveys but the samples are small and are restricted to special areas of research, like healthcare, water contamination and the polluted factory site. There is still a lot of research that remains to be done on those who do not fit into these categories, namely the second generation born with congenital defects. Once I had narrowed down the focus of my research, the next step was to choose the catchment area from where I could draw my interviewees. Then I moved on to generalised interview sessions where I asked each one of them to individually and collectively describe the reality as they experienced it. This often took place inside homes or in the office-cum-workplace of different
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survivor groups. I had to work with leads that were given to me by group leaders and rank and file workers. But mostly the groups came forward with identifiable people who were used to doing the talking. Once in a while impromptu gatherings would form in the bastis and the shy and the inarticulate would join in the discussions. My field notes indicate that voices spoke individually and in unison rather than in opposition with each other. Speaking and listening to each other happened simultaneously.

In the final analysis, I was not merely interested in integrating the quantitative with the qualitative approach. I discovered a more political agenda that research had to fulfil in Bhopal. And this came from speaking to the grassroots level activists who were involved with fighting for justice. I was drawing upon resources that were already available. There are seven survivors’ organisations that are active in Bhopal today. The oldest are Bhopal Gas Peedit Mahila Udyog Sangathan, which takes care of the issues of livelihood and fighting legal battles, Bhopal Gas Peedit Mahila Stationary Karamchari Sangh, which is the only surviving workers’ union of women who were given jobs in the work-sheds that were set up soon after the disaster, and Gas Peedit Nirashrit Pension Bhogi Sangharsh Morcha, which looks after the welfare of gas affected widows who are getting pension and Bhopal Group for Information and Action, which began as a coalition of intellectuals and middle class activists who were actively involved in the process of documentation and strategy planning. The Gas Peedit Purush Mahila Sangharsh Morcha was a breakaway group and works in coalition with the International Campaign for Justice (ICJB) in Bhopal, which is the international face of the movement. The ICJB is an umbrella organisation that straddles the local/global nexus of the social movement; it works in diverse areas such as health care, clean up of the factory site and bringing the offending corporation to book. However, a close look at the demands placed by different groups will show that agendas more or less overlap. The latest to enter the fray is Children against Carbide, which consists of second generation gas survivors including those affected by water contamination. Entering the field in Bhopal is in no way a merely academic exercise. I had to deal with prejudices inherent in research that gets mixed up with activism. And much of it had to do with my own intellectual training. But the resolutions came from inside the field, through the question/answer sessions that I was conducting. Bhopal tragedy has been a failure of documentation. The statistical research that was undertaken by the state and corporation in collusion with each other, had led to gross misrepresentation. So any qualitative research method that I was going to use had to serve the larger purpose of righting a wrong. And this could only happen when I was able to create the space where rectification and correction replaced integration as the aim of research. I had the onerous task of putting in place the political process of recording memories, experiences and the lived reality of disaster in a way that could counter the official attempt to erase people’s voices and identity.
In a way, my role as an ethnographer was tailor made and given to me. It made my task that much simpler and complex. Right from day one survivor groups had conducted several sessions of interviewing at the basti level that were published and distributed as activist material. In ethnographic terms this was the kind of intervention that transformed oral sources into written records. The idea was to gather documentary evidences that could be used to strengthen observations. Was my task that of retrieval or was it building on resources? Here I was called upon to play the dual role of researcher and campaigner. As a college teacher I had another kind of resource that I could build upon. It played upon pedagogy and education as vital parts of any social movement. One of the demands that had got the go-ahead from the PMO’s office was making Bhopal part of the school and college curriculum. It was a major lacuna that had to be filled. Like all unfulfilled promises it has continued to remain in the pipeline. WFB was created prior to the twentieth anniversary and it is important to note that its founder members were all students of literature. The idea was to create a forum that redefined student’s politics outside the narrow confines of political parties and regionalism and took studies beyond the classroom. Innovative action plans were conceptualised the year round, but the one that instantly comes to mind is the joint programme that WFB conducted with FM Radio 98.4; it was a live coverage of the Bhopal survivor’s arrival in Delhi, at the end of their padyatra from Bhopal to Delhi in the year 2004. Sumit Ray, a student and member of WFB was on air with a popular radio jockey and spoke at length about the nature of the struggle for justice in Bhopal. Once the padyatris entered the city, live interviews were conducted with them, thus bringing the voices of people to our very doorstep.

My research work in NMML had much more to do with consolidation of resources that I had already tapped into, through the kind of multitasking that I had been doing as teacher-advisor/writer/researcher. So the expertise that I was able to lend to the Bhopal Movement Study Group was eclectic in nature. While Eurig’s background as a foreigner working in a third world country, matched our shared academic and pedagogical training, Dharmesh brought in his experience of activism that went well with Tarunima’s post graduate degree in sociology from Delhi University. Both Dharmesh and Tarunima, as research assistants, were based in Bhopal for a year. Eurig and my fieldwork involved a succession of short field trips. Ours was collaborative work with the survivor groups so that the frame of references we used for our interviews were indigenously evolved. The background material had its historical significance and helped in framing the questions that were put to the respondents. The data became a valuable tool for deconstructing given hypotheses and preconceived theories. Newspapers constituted an important source, but corpus of research material also included personal documents, autobiographical sketches, dairies, poems, plays, novels, and folk songs. A
key feature of our research was active participation in events organised by survivor groups. A lot of the interviews were thus conducted in the field, not defined in the narrow sense of location, but had much more to do with winning the confidence and trust of the survivors by showing solidarity with the movement.

I would like to go back to the common adage that an ethnographer needs to “plunge into the community” in order to perceive them through their otherness (Srivastava 2004: 20). In Bhopal this gets problematized in a particularly political way. Thus, participant observation and the question of how to maintain objectivity in analysis, ceases to be a merely methodological problem. Instead, we are looking at the question of who defines and controls information and how it is disseminated. Can the intersubjective become the ground for objective forms of knowledge? It is a question that is often raised in ethnographic studies. But an industrial disaster begs the more important question of justice. Rehabilitation and compensation cannot be seen as separate from issues of human rights, the polluter pays principle and bringing the culprit to book. In Bhopal the search for meaning has to begin from the premise that Bhopal is not the other and we are not outsiders. The otherness is a political construct that propagates the theory that marginal people must pay the price for development. The distance that separates the ethnographer and the survivor is a mirage that keeps our world insulated and at a distance.

How far can situated knowledge break this barrier? Perhaps, we can take another look at what Geertz said in Local Knowledge. He prioritised the literary over the scientific in any description of cultural phenomena. He said that we are in search of symbols, meaning, conception, form and text so as to construct the “imaginative make up of a society” (Geertz 1993, 5). Perhaps, he was seeking an answer to the more political question of whether an ethnographer can speak authoritatively on behalf of the other. He did acknowledge the richness of an ethnographic text once it was placed “in local frames of awareness” (ibid.: 6). But he was not quite sure of the possibility of transcultural identification with the native subject. Ethnographic writing has been compared with literary genres such as travel writing and personal memoirs, but only in a pejorative sense. In Writing Culture Clifford admits to the rising popularity of the literary approach in ethnography. He says the use of metaphors, figuration and narratives can render the factual in a more evocative way. But that is where the comparison between the two disciplines stops. I was not able to find any comparative study of the two disciplines by any Indian scholar.

In the course of doing fieldwork, ethnographers are called upon to grapple with the question of how effective is the interview method. It is both a theoretical as well as a problem of empirical investigation. In disaster studies the problem is that of garnering the right kind of information. Should the interviewer use free floating conversations in tandem with structured
interviews? I was to discover that the problem was not simply that of earning trust. The questions had to mediate the personal and the impersonal without getting too probing. Prepared questions often did not elicit the right kind of answers. When we are dealing with people’s memories of trauma we are treading treacherous grounds. Factual details are often glossed over, and sometimes rhetoric replaces reasoning. It is difficult to have a timeframe for interview sessions and more often than not I had to come back the next day. There was very little hostility, but standardised responses created barriers that could only be broken down by continuous effort on my part. I re-read the field notes at the end of the day. Since I was transcribing what was being said in Hindi into English almost simultaneously, much of it was paraphrasing rather than verbatim. I was later to compare this method with the one used by The Bhopal Movement Study Group, where the interviews were being videotaped and then transcribed and translated by a trained translator. But the difference was marginal. I was often surprised by the emotional and imaginative depth and range of experiences that my questions were able to evoke. The language was both colloquial and metaphorical in the strictly literary sense. The interviews were replete with homespun wisdom, sharp analogies and a deep sense of satire. I was more than happy to use narrative style to make the interview method effective. I was not so much interested in information gathering as encouraging them to relive events – involving loss, grief, and pain – in a way that was almost therapeutic. In an important sense, the actual task of realising inter-subjectivity in the field brought me back to literary sources and styles of interpretation. One of my essential readings throughout this period was the plays of Shakespeare. I believe that ethnography comes closest to literature in the way it recreates events in the lives of people, by drawing on the strength of personal narrative that is then shared. It is therefore much more than a mere stylistic tool. It helps in studying cultural practice from the perspective of those who are involved in scripting it and are therefore, part of the change in society. Facts cease to exist as mere facts and change into the story that is being told. I am reminded of what Clifford had said in The Predicament of Culture of ethnography being the art of writing the narrative. I would add that it is a performative art that involves listening, enacting and transcribing.

Clifford went on to describe the process by which a discourse becomes a text in ethnographic writing (1988: 39). According to him, the speaking subject gets separated from “specific utterance” and experiences are transcribed into field notes. He uses rather strong words when he writes:

*situational aspects of ethnographic interpretation gets banished from the final representative texts (ibid.: 40).*

My contention is why banished? I was told by one of the survivor group leaders that academics had come to Bhopal but left soon after publishing their articles and books. Not surprisingly my first arrival in Bhopal was met with scepticism.
But since I introduced myself as a writer of a children’s book on Bhopal, my creativity got foregrounded and I was seen as someone who made texts that could blend fact, imagination and creativity. This is precisely what I sought to do in a way that would widen the scope of ethnographic research. I was not interested in excluding the respondent. Like a literary text that gives autonomy to its characters, I wanted to restore the narrative back to the people in a way that would enable them to possess their own stories.

In one of workshops conducted by the Bhopal Movement Study Group we discussed the video diary, in which the tape could be played back to the interviewee so that a self-reflexive process of revisiting issues was established. We managed to carry it through in many of the cases. Finally, each interviewee was given a copy of the CD in its completed form. The main purpose of the research undertaken by the group was to add to the local level knowledge-building, by initiating a process of recording, disseminating and exchanging information, so as to create a common platform for people to come forward and speak. Otherness was replaced by the notion of exchange as a methodological vantage point, which could encourage academia, other professionals, non-governmental organisations and survivor groups to come forward and help in this knowledge-building process.

Actually, there is no dearth of personal narratives in ethnographic studies, but they are mostly legitimized as a sub-genre, which is included in anthologies as part of the introduction, or in separate articles that describe the personal experience in vivid details. The idea is to keep the subjective apart from the authoritative text. This probably explains why literature is discredited. My contention is that Bhopal needs to be re-read and re-written, by inducting the respondents into the process of writing the text as co-authors. The Bhopal Movement Study Group made a modest attempt in such a direction when they published their research findings in a book that was titled *Bhopal Survivors Speak*, without using the name of individual authors on the front page. The idea was to promote the collective as contributing to the writing of the book.

Authorial intention is rarely highlighted in literature. Therefore, the personal does not become merely subjective. Literature is hard hitting, probing and seeks to tell the truth An interview method, however objective its parameters might be, runs the risk of becoming too in-grown in nature. It is interesting to note that a lot of popular, journalistic writing on Bhopal uses the human-interest story as a literary trope. Needless to say, when this is done without the rigour of ethnographic research the writing becomes puerile and superficial. At the same time, it feeds into misinformation and the more dangerous playing out of the politics of engagement. This is what happened in Bhopal. In a lot of writing the mood evoked is retrospective. We are voyeuristically asked to listen in to narratives of pain and suffering. Then in a deliberate move, the past is set aside, so that we can move on. Simultaneously,
Bhopal gets catapulted into the future by evoking images of more Bhopals waiting to happen. It is this vicious cycle of remembering and forgetting that needs to be demolished. It can only happen when the process of transcribing fieldwork ceases to be the exclusive domain of outside authorities. Only then will ethnography go beyond issues of validity, reliability and authenticity. No doubt, there cannot be an unproblematic, authentic insider/outsider perspective. But so long as situated knowledge indicates personal involvement with people in the field we are already talking about adding dimensions of advocacy and action to research. Ethnographic writing has argued that compelling narratives, which can transform a respondent into a subject rather than object of research, can go hand in hand with rigorous analysis. My submission is that we need to take research even further by using narrative voices that are both personal and representative, so that they become means for enacting rather than verifying the hypotheses, in a way that empowers the community to speak on their own behalf. Only then can we hope to remove the exploitation of people and their voices for purposes of either research or activism. Is this possible within the academia or at the field level is a question that ethnographic research needs to ask, though honest answers will hardly be forthcoming.

In the concluding section of this paper I would like to share a few pages from the field-diary that I maintained during field-trips to Bhopal. I possess three such diaries. They were used extensively in my writing, and helped in strategising before each of the interview sessions. They provided the pen portraits that I drew of characters, which captured the nuances of gestures, voice inflections and expressions. The entries were done on a daily basis and were self-reflexive in nature for I was working out my own role as an ethnographer. Once in a while when the argument raised in the course of the interview session became too complex I wrote it down almost immediately. On one such occasion Hazra Bi found me sitting on the street kerb and writing furiously after an intensive session inside her house in J. P. Nagar. She asked me what I was doing. I told her that I was writing down the details of what she had said because I wanted to capture the mood of our conversation. Seeing the curiosity on her face I read out bits and pieces of what I had written in the diary. It proved to be a very useful exchange for it helped me to conceptualise the way I was going to use tools of research, drawn from my own discipline, in ethnographic fieldwork.

I noted down in the margins what she said to me:

This is very nice. I feel as though I am watching myself on the big screen of a film that you have made on me. This is better than simply taping my voice on a tape recorder. That will only tell you what I have said but not the way I have said it.

Hazra Bi was able to make the vital connection between the subject/object and the writing of the text. I tried to assuage any doubts she might have
about the accuracy and objectivity of the text but she did not seem to be unduly worried. She said:

You are a writer and you know best how you write. It is not my headache. The only thing that is important is that people who read this must believe in the truth of what I am saying.

I knew right then that Hazra Bi had shifted the onus of truth-telling to me, even as she laid claims to possessing her own voice.

I believe that this kind of empowerment can only come through years of sustained struggle against all odds, and all that my research findings can claim to do is help in strengthening the foundations of the struggle. Right then, I made up my mind to draw on the strength I possessed as a literature person, to write on their behalf without dominating the process of writing, and lending them the eloquence without taking away their ideas. But at the end of the day, both Hazra Bi and I were still worried about middleclass readership and their taste in reading. The fact that Hazra Bi recognised this, made me cringe. But then as I told her, all I was hoping to do was make a small dent somewhere.

12 April 2003

My diary:

My taping sessions with Rashida Bi or Appa as she is known to everyone, is about to begin. She is a little late in coming so I wait. I am watching everything intently, much like reading and absorbing. You have to experience this place in order to enter its spirit. Appa is a strong person, dignified and iron-willed. You sense that she is self-made and much of who she is cannot be captured in words. You also sense her mission, her commitment to the cause. She is here because she has acquired an identity that is larger than who she is as a person. But when she arrives she looks quiet and smiles shyly, bringing in the different parts of her selves in happy conjunction. There is a rapport between us and that sets the mood of our conversation. She says, let us meet in my office and I readily agree, because Appa must appear in my writings in her proper context.

Just off the busy Hamidia Road is a narrow lane that leads into the Mahila Stationary Karamchari Sangh office. It is the only functioning government scheme for rehabilitation of women gas survivors by giving them employment. The place is vibrant but ill kept. I am led into a room that is dark and ill-lit with broken window panes and dilapidated furniture strewn around. The walls have not been whitewashed in years and I wonder if the premises have ever been fumigated. Appa enters the room with some purpose and offers me a chair. Champa Devi Shukla or Didi as she is called is the Secretary of the Worker's Union, and Appa is its President. The room feels damp and cool. Bhopal is hot in the daytime. A blackened fan is whirring monotonously, sending gusts of stagnant air in my direction. I am served tea in chipped cup and saucer. I set my tape ready. Appa begins to speak. Didi is slated for another day so she keeps quiet. Once or twice the others peep in, bringing the taping to a halt but Appa sends them away. I notice that she has considerable authority over the others.”
(Appa speaks at length about the nature of her involvement in the social movement and what role the Stationery Union plays).

This is not the first time that I am listening to these accounts of the early days and the remorseless nature of the struggle. I realise that Appa is a trained speaker and she knows what kind of information I am seeking. In her own way she is acknowledging my work, and giving it importance. So the ground for mutual respect is an unwritten code. At the start of the interview session I tell her what I plan to do with all this material. She nods quietly. She is eager that I get all the facts right. And then she tells me that I must speak to the other women in the Union, but she is not sure that they can get everything right. At one point she asks Didi to take out some files and show it to me so that I can verify the data. Then she says, you can Xerox whatever you find useful.

There are so many stories to be told. There is a curiously repetitive quality about them as if you are being sucked into a bottomless pit. Endless outpouring, an emptying out that can never put an end to the telling. There is no nostalgia here, only the stridency of telling it again and again till it gets etched in the crevices of your brain. You hear every detail but it sounds like a scream. Blood and gore, terror and corpses, shit, vomit, the drumming sound of people screaming, shouting, running, falling on the wayside and dying, they press against you, till you feel pinned against the wall, your eardrums throbbing, your brain numb.

We take a break and Appa and Didi take me around the premises. The halls are spacious with mounds of brown envelopes and white sheets lying around the place. A few primitive machines stand in the corners. It is lunch time. The women are eating from tiffin boxes and some are taking a quick snooze. It is a workplace that is dogged by negligence and government apathy; diminishing orders and huge losses has made it into a white elephant. But these women speak with a sense of pride. Militancy is low but they continue to participate in any call for action.

Appa and Didi look tired. They suffer from numerous ailments. Their faces — weather-beaten and frail — are yet valorous. They are draped in almost identical saris and I notice that Appa is wearing green bangles. They have a frugal lunch and I share a glass of drink with them. Both from different faiths are keeping a fast and I marvel at the symbolic value of their togetherness in the battle for justice. I am quite satisfied with my days recording.

NOTES


2. My novel Across the Mystic Shore (London: Macmillan New Writing, 2006) was published around the same time that I was researching on Bhopal. In an important sense my academic and fictional writing have been part of the same literary impulse.

4. Worth mentioning here is the report that students of WFB brought out titled *Closer to Reality: Reporting Bhopal Twenty Years after the Gas Tragedy* (2004). It was accompanied by a 24 minutes video film of the same name, and was based on ethnographic research done by students at the field level.

5. Almost all the classic ethnographers dealt with otherness as an intrinsic condition of doing research. Of particular interest was how Geertz defined his own work in relation to the otherness of the subject of his research: “In all the three societies I have studied intensively, Javanese, Balinese and Moroccan, I have been concerned, among other things, with attempting to determine how the people who live there define themselves as person, what goes into the idea they have of what a self, Javanese, Balinese and Moroccan is. And in each case, I have tried to get at the most intimate of notions not by imagining myself someone else, a rich peasant or a tribal sheik, and then by seeing what I thought, but by searching out and analyzing the symbolic forms – words, images, institutions, behaviors – in terms of which, in each place, people actually represented themselves to themselves and to one another” (1983, 58). It was intriguing to find the close association that ethnography had to literature and needless to say, this was one of the main reasons why I was drawn to fieldwork. But, in the course of my work I was to detach myself from symbolic forms.

6. See Vincent Crapanzano’s essay “Hermes’s dilemma: The masking of subversion in ethnographic description” for its brilliant analysis of Geertz’s essay “Deep play: Notes on a Balinese cockfight” (Clifford 1986, 51-76). What Crapanzano finds problematic is the asymmetrical relationship between the anthropologist and the native, with the former standing behind and above the latter, in reading the Balinese experience through a metacommentary that tells the natives about themselves. The constructed nature of cultural text is best captured in verse in Michael M. J. Fischer’s essay “Ethnicity and the post-modern arts of memories” (Clifford 1986, 194-233):

   Transference
   My ancestors talk
   to me in dangling
   myths
   Each word a riddle
   each dream
   heirless
   On sunny days
   I bury
   words
   they put out roots
   and coil around
   forgotten syntax
   Next spring a full
   blown anecdote
   will sprout

   (Diana Der Hovanessian, “Learning an ancestral tongue”).

7. Also see Kirin Narayan’s essay “How native is a native anthropologist?” for the outsider/insider dynamics and Veena Das’s essay “Anthropological knowledge: Alterity and the autobiographical voice” for an intriguing analysis of how an anthropological journey into distant lands can be seen also as a journey to the unreachable parts of the self (Thapan 1998: 163-187 and 41-53).

8. See Saraswati Haider’s essay “Dialogue as method and as text” for a succinct account of how subjectivity and objectivity go hand in hand in ethnographic studies. She then
goes on to give us extracts from her conversations with Shanno, a *jhuggi-jhopdi* dweller (Thapan 1998, 217-266).


10. It is Mary Louise Pratt who describes it as a sub-genre (41). It gets connected to the more academic problem of choosing careers in anthropology, and the question of how far fieldwork adds professional credentials to the writing of the dissertation. Also see George E. Marcus’s “Afterword: Ethnographic writing and anthropological careers” (Clifford 1986, 262-266).


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