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Mapping the self: challenges of insider research in a riot-affected city and strategies to improve data quality

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Ethnic riots in India rarely lead to convictions of perpetrators and redress for victims. By implication, antagonisms prevail years after violence has ceased and victims often find themselves sharing everyday spaces with their attackers. The task of identifying the risk factors leading to ethnic violence as well as the nuances of coexistence for individuals ridden with memories of violence and prejudice is rife with methodological and ethical challenges. Concerns surrounding data quality are enhanced when the researcher is also an insider. In a study spanning 26 months (undertaken between 2010 and 2015), I examine the challenges of an insider researcher in the context of Hindu–Muslim violence that occurred in Gujarat in 2002, and offer techniques to improve data quality. Strategies include the cross-verification of sources within official data; interviewing respondents in group and individual settings to address the attitudinal fallacy; and employing respondent-empowered cognitive maps. I argue that visual data, such as cognitive maps, enable a better understanding of abstract social concepts and also facilitate a balance between distance and involvement for the insider researcher.

Keywords: insider–outsider; riots; Hindu–Muslim; cognitive maps; Gujarat

Introduction

Research in conflict-affected field settings is a messy affair, rife with ethical and methodological challenges. The ramifications of these challenges are enhanced when the researcher also happens to be an ‘insider’ – someone whose biography (religion, nationality, age, region, and so on) gives them a familiarity with the group being researched (Griffith 1998). There is substantive literature on the challenges of conducting research in conflict sites, in South Asia and other regions (Dowler 2001; Finlay 2001; Tambiah 1997; Wilkinson 2009; Wood 2006) and, independently, on the consequences of the insider–outsider distinction on data quality (Dwyer and Buckle 2009; Kanuha 2000; MacPhail 2004; Mohammad and Sidaway 2013). This article examines the convergence of the two: the challenges of the insider researcher conducting fieldwork in a violence-affected site. In this case, the researcher is a Gujarati Muslim who lived through three waves of Hindu–Muslim violence in Ahmedabad, in the state of Gujarat in western India, then engaged closely – as a journalist and later as an academic – with the violence that occurred in the city in 2002.

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Writings of this nature can, frustratingly, veer towards navel-gazing unless the intent of recounting one's personal 'epiphanies' is to examine these experiences analytically, to enable inventiveness and rigour in data collection and its analysis despite the uncertainty of the field (Dowler 2001; Ellis, Adams, and Bochner 2011). It is with this intent that I reflect upon research conducted, intermittently, over a period of five years (2010–2015), in heterogeneous neighbourhoods of a highly tense and ethnically segregated city recovering from extreme violence, where protracted judicial redress had compelled victims of violence to live alongside perpetrators in the same neighbourhood – a comprehensible reality given that rioters rarely travel far from their residences (Dhattiwala 2016a; Kawalerowicz and Biggs 2015). The role of the State in perpetrating the violence, thereby offering it legitimacy, complicated the research process.

In 2002, Gujarat witnessed some of the worst ethnic killings in modern Indian history. Muslims were largely the targets of the attacks that began with the burning of a train in Godhra town on February 27. Fifty-nine Hindu 'karsevaks' (volunteers for a religious cause) were killed in the train fire, blamed on local Muslims (Commission of Inquiry 2008). The horrific incident triggered widespread attacks across the State, largely on Muslims, which the ruling Bharatiya Janata Party (BJP) government, under current Indian Prime Minister Narendra Modi, who was then the Chief Minister of Gujarat, characterised as 'spontaneous' outbursts of angered Hindus. At least a thousand Muslims were killed (Dhattiwala and Biggs 2012).

The scale and brutality of these attacks was extraordinary, yet several towns, villages, and neighbourhoods had remained unaffected. My research sought to explore why such variation had occurred. Were these differences in violence chance events or could they be systematically explained? In what ways did the behaviour of individuals who engaged in actual violence on the ground influence such variation (see Collins 2008)? I also proposed to explore the complexities of space and contact, best illustrated in the thesis of interethnic civic and associational ties as deterrents to violence (Varshney 2002), thereby reviewing the intuitive assumption that 'neighbours' are spatially proximate individuals who, by virtue of this proximity, spontaneously develop beneficial ties. If opportunity for contact were sufficient to initiate and sustain contact, this would be in stark contradiction of the common occurrence of neighbours attacking neighbours in collective violence (Kimura 2013; Tambiah 1997).

To provide any explanation for the spatial variation, first of all, it was critical to establish data validity or accuracy about the level of violence: how many died in the given spatial unit? Was there complete peace or were there incidents of arson or looting? Official data in civilian riots are frequently manipulated by police and politicians, both of whom have an incentive to under-report crime to portray efficient governance (Wilkinson 2009). This is especially problematic in India, where rationally-motivated acts of violence against minority groups, camouflaged as spontaneous outbursts of anger, have become a legitimate form of political expression (Brass 2003; Hansen 2008, 3; Spodek 2011; Tambiah 1986) – comparable with informal justice systems in apartheid South Africa, and legitimised 'punishment' beatings in Northern Ireland (Knox 2001). By implication, investigations are botched, often leading to protracted or lopsided judicial redress (Jaffrelot 2012; Kimura 2013; Mitta 2014).¹

Combined with the tangible challenges of the data itself, the 'do no harm' imperative in research ethics proscribes methods of data-gathering which could negatively affect the respondent as well as the researcher. Interviewing a victim of violence on their relations with a neighbour responsible for the attack is analytically valuable for the researcher, but fraught with danger for the respondent. It could harm them physically and emotionally, and adversely affect their social networks. The only known explicit source of rioter

testimonies in the Gujarat violence are the *Tehelka* tapes (Khetan 2007) and Ayyub's (2016) revelatory incursions into the State's complicity in the violence, both clandestine journalistic exposés. Research ethics would not permit similar deception. Therefore, within the parameters of ethnographic research, how far should ethics be compromised in the pursuit of quality data and its neutral analysis? One could argue that it is naïve to expect neutrality in the field and that the validation of data is futile because respondent testimonies are context-bound (Silverman 2000) and lead to multiple constructions of reality (Mercer 2007). As Weber (1904, 81) notes, an unemotional analysis of social phenomena is meaningless because 'all knowledge of cultural reality, as may be seen, is always knowledge from particular points of view'. But Weber also acknowledges that the subjective orientation of the scientist is separate from the work of science itself. Even if scientists themselves are socially situated, science can and should aspire for objectivity and value-neutrality (see also Gimbel 2016). In qualitative social science, especially, it is necessary for researchers to specify precisely the modalities of their research and adopt mechanisms that shield the social scientist from undesirable biases to avoid ambiguous ethnographies (Golafshani 2003; LeCompte and Goetz 1982; also, King, Keohane, and Verba 1994).

The first half of this article highlights the ethical dilemmas and methodological challenges of conducting insider research in conflict-affected environments; it then moves on in the second half to propose strategies to enhance data quality. I emphasise the use of cognitive maps as a method of triangulation. By offering the vantage point to the respondent, visual data produced by cognitive maps can reduce the researcher's positional bias and, at the same time, is better at capturing abstract sociological concepts, in this case, the 'neighbour'. I also propose using spontaneous discussions with respondents in groups and, separately, in individual settings to reduce the attitudinal fallacy – the discrepancy between the spoken word and actual behaviour – a major concern in qualitative research (Jerolmack and Khan 2014).

Data and ethical concerns in a riot-affected site

The primary puzzle of explaining spatial variation guided my research design. The tradition of studying geographic variation in collective violence was well established by Donald Greer's 1935 study of political executions during the period of Terror (1793–1794) in France (Kalyvas 2012). In the study of Hindu–Muslim violence in India, the value of this approach has been realised by several scholars (Berenschot 2011; Brass 2003; Varshney 2002; Wilkinson 2004). Whereas the culturalist framework, which has been dominant in the study of ethnic violence in India (Freitag 1989; Ghassem-Fachandi 2010), provides a rich background into the demonisation of minority groups, the difficulty lies in assessing the extent of such explanations and, subsequently, their analytical depth – it is unclear, probably unknown, why and how socially constructed fear varies across space, leading to different levels of violence across often contiguous spaces. At the time of my research, a dichotomy existed even within the literature on geographic variation, wherein explanations either took a top-down (e.g. Urdal 2008; Wilkinson 2004) or a bottom-up (Berenschot 2011; Varshney 2002) approach. Neither, in isolation, can successfully explain a social phenomenon, just as neither is mutually exclusive of the other. To explain how violence actually unfolds on the ground, one would need to control for variables proxying individual-level socio-economic characteristics. The political configuration of an electoral constituency would also need to be held constant in order to reveal the effect of individual-level factors.

True to expectation, the figures on casualties in the violence provided by the government were questionable (Dhattiwala and Biggs 2012, 491). Underreported official data often lead

scholars to rely on non-governmental organisations (Berenschot 2011). Unless triangulated with multiple sources, figures from advocacy groups may either overstate the violence (Dhattiwala and Biggs 2012, 491) or are withheld for fear of reprisal from the State (Kumar 2016). I proceeded, therefore, to compile an original dataset of the number of killings across 216 Census towns and rural areas of Gujarat, using newspapers as the primary source. A systematic analysis of the data rejected the spontaneity thesis (Dhattiwala and Biggs 2012; also, Berenschot 2011). Findings deemed the violence to be an anti-Muslim ‘pogrom’ orchestrated by the ruling government to consolidate non-Muslim votes: the worst anti-Muslim violence occurred in places where the BJP faced the greatest political competition.

This finding provided the rationale for explaining microspatial variation in the violence; that is, if political incentives were critical in explaining spatial variation across towns and villages, why did neighbourhoods within the same electoral constituency or municipal ward in a town experience different levels of violence? Because newspapers did not always identify the exact location of deaths within neighbourhoods, ethnographic fieldwork became a better strategy to address this question. I compared four neighbourhoods in Ahmedabad within two kilometres with varying levels of violence. All four neighbourhoods were located in the same municipal ward, thereby allowing me to hold the political configuration constant. Another three neighbourhoods, in different municipal wards, became sites of study in 2014–2015 (Table 1).²

Spontaneous testimonies, rather than structured interviews, were less intimidating for the respondent and facilitated crucial assessments of the violence. The process of analytic induction (Glaser and Strauss 1967) helped unravel the role of ecological factors – ‘bhugol’ (geography) or ‘naksha’ (map) – in the actors’ decision-making processes during attacks (Dhattiwala 2016a). One disadvantage of the open-ended discussions was contradictory information. Rashomon-like, multiple realities began to emerge from a single episode (see Brass 1997; Roy 1994; Wilkinson 2009), as every other respondent, Hindu or Muslim, professed to being a ‘victim’ duty-bound to engage in violence out of ‘self-defence’. When the State is the perpetrator of violence against civilians, the meanings of the terms ‘victim’ and ‘perpetrator’ get defined on the basis of who ‘deserved’ punishment at a certain time (Knox 2001; also Ghassem-Fachandi 2010; Jasani 2011; Jeffery and Jeffery 1994). Indeed, had my study aimed to examine the subtexts in respondents’ testimonies or the meaning-making processes in the aftermath of violence, this was rich material for analysis.

To illustrate ambiguity in testimonies, I recall here a meeting with a local police inspector in early 2012 to verify the deaths of two Muslim men in a commercial market on 28

Table 1. Research sites in Ahmedabad, Gujarat.

Serial no.	Neighbourhoods	Municipal ward, as in 2010	Level of violence in 2002	Religious composition
1	Ram Rahimnagar	Behrampura	Peaceful	Mixed
2	Santoshnagar	Behrampura	Arson and looting	Mixed
3	Parikshitlalnagar	Behrampura	Four killed	Mixed
		(part Danilimda)		
4	Kabadi Markets 2 and 3	Behrampura (part Danilimda)	Two killed, looting	Muslim-owned markets
5	Naroda Patiya	Naroda Road	97 killed	Mixed
6	Parmanand Patel chali and vicinity	Gomtipur	2 killed, looting	Mixed, substantial Christian population
7	Makarba	Sarkhej	Peaceful	Mixed, substantial Hindu population

February 2002. Police records provided to me earlier had no mention of these deaths despite testimonial evidence by shop-owners and the wife of one of the murdered men.

Me: (About the deaths) ‘Some Muslims I met said that there was trouble.’

Police Inspector PX: ‘If there is nothing in our records, nothing must have happened.’

He offers to help, however. He asks a colleague who denies any deaths. PX then phones a man, addresses him as ‘Salim’. I assume Salim is a local police informer, a Muslim.

PX (on phone, in Hindi): ‘Oh ... two watchmen died? Ah, okay ... they got burnt when the market caught fire?’ (looking at me) ‘Yes, two watchmen died. The market must have caught fire and they both died in it.’ *His words imply that the deaths were accidental, not intentional.* (Fieldnotes, 13 February 2012)

The interview with the policeman proved to be a fruitful validation measure for it verified the two murders and also ascertained that official records were incomplete. But it also uncovered ambiguity surrounding the deaths – were the two men killed or was it an accidental death? Further, the sharing of neighbourhoods meant that targets of violence knew their perpetrators, but most refrained from identifying them. A conversation between a Hindu and her Muslim neighbour in Gomtipur was illuminating:

Dhara (Hindu), Munaf (Muslim) and I are talking about relations with their neighbours. The Hindu-Muslim violence of 2002 comes up during the conversation.

Dhara: You, Munaf, I saw you that day! You pelted stones on us, didn’t you! (they both laugh)

Munaf: Let’s forget that now, we are friends now!

Dhara: (looks at me) Yes, it was different then. But now we (Hindus and Muslims) have no problems with one another. (Fieldnotes, 17 February 2015)³

Identifying riot participants – actual participants in violence rather than those who ‘joined the mob’ but refrained from attacks – was not straightforward. Despite the State’s complicity in the violence, ongoing judicial proceedings and the apprehension of imminent violence in mixed neighbourhoods had made respondents distrustful of each other and researchers. Kimura’s (2013, 117) relative ease of access to perpetrators during her fieldwork in Nellie, Assam, 20 years after the killings was, she argues, because perpetrators believed all was forgotten as no one had been prosecuted.

Data and ethical concerns of the ‘insider’ ethnographer

My position as an insider researcher amplified the problem of potential bias already present because of incomplete official data and ambiguous testimonies. The insider, as a frame of reference, is almost always seen as mutually exclusive from the outsider; the emic or subjective perspective of the insider, in contrast with the etic or objective perspective of the outsider (Headland, Pike, and Harris 1990). Others have rejected these sharp boundaries, instead emphasising the ambiguity of being caught in a ‘continuum’ (Merton 1972; Mercer 2007; Mohammad and Sidaway 2013). In accordance with this literature, I illustrate how my position moved between insider and outsider in unexpected ways. In certain situations, I was perceived as an insider, when I assumed I was the outsider, and vice versa. Being caught in a continuum was less detrimental to data, for it cancelled out the potentially significant biases emerging from fixed categories. Even so, positional ambiguity enhanced my concerns about distance and involvement – described by Wilson (1995, 255) as ‘being simultaneously detached and yet intensely engaged’.

Ahmedabad was the city in which I was born – a Muslim – and lived through three major waves of ethnic rioting, in 1985, 1992, and 2002. My experiences of escaping violence at one time, and reporting about it at another time, as a journalist in 2002, motivated me to study violent conflict. But this desire, or rather obligation, was fraught with self-doubt. Despite my areligiosity, the relative immutability of one's own religious identity in India implied that any communication with attackers and victims of violence would have emotional and methodological ramifications for me. I was also acquainted with some of the respondents for several years as a journalist. At times, I believed my familiarity, as a researcher, would make communicating with them somewhat easier. But I also feared that, unlike an outsider, I did not have an external vantage point. There remained a danger that my findings may be, in a sense, an artefact of my presence rather than being firmly rooted in evidence. Of course, there are obvious advantages to being the insider. Besides the motivation to explore certain sociological phenomena, the insider is also believed to have privileged access to particular kinds of knowledge (Merton 1972), and an intuitive grasp over coded language that may escape the outsider (Kanuha 2000). For example, analyses of the Israel-Palestine conflict have tended to be biased; particularly as outsiders relied entirely on translations of vernacular text, and failed to understand the intended cultural or historic nuances of idiomatic expressions, such as the English translations of Hebrew documents (Hermann 2001). It is what Merton (1972, 15) terms the outsider's 'structurally imposed incapacity to comprehend alien groups, statuses, cultures and societies ...'

Conversely, the insider can risk an early saturation of data by myopically assuming several cultural and political events to be of relatively lower significance. During the analysis, the insider is also likely to face the dilemma of being caught between siding with their community, and maintaining academic standards leading to either a self-deprecating approach, or a pro-victim bias (Hermann 2001). On the basis of my religion, nationality, and local knowledge of the riots and the city, I entered the field believing myself to be an insider. I decided to begin my research in the peaceful neighbourhood, Ram Rahimnagar, which is considered as an exemplar of harmony for having maintained complete peace since the first major episode of violence in 1969. This was a neighbourhood I thought I knew well. I had visited it several times in the past five years, as a journalist and then as a social worker, and had excellent rapport with five of the most influential founding members of the neighbourhood. I assumed that negotiating access would be easy, because the founders would be my gatekeepers and the residents would see me as an insider, as one of them. I soon realised that residents had viewed my visits in the role of a journalist with a leading English newspaper as justifiable: the newspaper would speak of their chivalry, which would enhance their positive social identity among their peers. I, in turn, would show them the copy of the newspaper the next day, thus reinforcing my functional role in their community. Without my newspaper, I was perceived as a 'free-rider'. Negotiating access with the residents, beyond the founding members, became an ordeal. It had never occurred to me until then that the gatekeepers would not allow anyone (even journalists) to go 'beyond the gate' and meet the people, unless in their presence. Due to this, no resident dared verbalise anything beyond the perfunctory.

MacPhail (2004) describes a similar experience in a completely different setting – a sports club. Discussing the failings and benefits of having the dual role of an ethnographer, conducting field research of a sport she actively engaged in, and in the same sports club of which she was a member, she writes, 'While I maintain that my background in sport and continued involvement in athletics in particular encouraged conversation and access to information from some research informants, it was not necessarily aided by my role as an athlete at FAC ...'. Like MacPhail, my previous experiences helped me to access my

key informant, Salman, easily. However, my seemingly purposeless daily visits led to evasive responses. Eventually, Salman had to explain to me: ‘They think you are a political spy.’ Their closure, however, gave me an important insight into their peacekeeping: residents would not speak to strangers whom they suspected to have political motives, unless directed by their leaders.

Being a woman proved to be advantageous in my early days of fieldwork. Respondents found me trustworthy enough to allow initial access into the fieldsites.⁴ But my gender had in fact also led to Salman’s inexplicable disappearance and reappearance after six months: ‘People have been asking me why I move around with a woman alone,’ he had confessed later. Apart from the moral scrutiny, I perceived minimal physical danger to myself, despite my gender. The streets of Ahmedabad, which have witnessed some of the most terrible violence, are also, paradoxically, benign to the woman traveller on a regular day.⁵

Deception

Growing up in Ahmedabad had made me aware of the casual manner in which questions about religion and caste are asked, in Gujarati: ‘*Tamey kevā?*’ (literally, ‘What are you?’) is a customary form of introduction, referring to one’s caste or religion. This posed the following ethical dilemma: When asked, should I announce my real religion, and risk my and the respondent’s life? Or, should I lie, thus establishing deception? The bigger challenge was that, even if I chose to neither announce nor lie and simply skirt the question, how would I avoid being identified as a Muslim, simply by what Burton (1978) refers to as ‘telling’ in context of Northern Ireland ‘Troubles’? ‘Telling’ – the pattern of signs and cues by which religious ascription is arrived at in the everyday interactions of Protestants and Catholics – is, as Burton suggests, endemic to conflict societies. It acts as a mechanism to determine the identity of the ‘other’, and subsequently to build and sustain areas of trust. Finlay (2001) recalls his experience interviewing a Catholic woman in Northern Ireland who could pick up familiar signals during the course of the interview, and ‘tell’ that he was a Protestant, although she was not explicitly told so. If both the interviewers and the interviewees engage in ‘telling’, depending on the outcome of the process, they may never, or only rarely get beyond the bland, superficial, coded communication (Finlay 2001, 60).

To my advantage, few respondents could ‘tell’ my religion outright. Religion-revealing cues are largely the name, dress, and language (and diction). My name is common to both Hindus and Muslims. I ensured dressing in loose *salwār-kameez* and a *dupatta* – a dress common to both religions and spoke both Hindi (and Urdu) and Gujarati. Frequent usage of all these languages, as part of my journalistic profession earlier, had removed traces of a specific diction. Once in a while, a cagey Hindu autorickshaw driver would warn me about ‘crossing the bridge’, an occurrence that was disturbing yet reassuring, for it meant that my Muslim identity was not evident.⁶

Because no one could ‘tell’ my religion with certainty, I would often be asked: ‘*Tamey kevā?*’, or: ‘Are you Hindu or Muslim?’ The following is one such conversation, which shows my attempt to respond to the question:

Respondent: Are you Hindu or Muslim?

Me: Let’s not get into that (with a laugh)

R: What’s the harm? Just tell me, I am curious.

Me: My husband is Jewish.⁷

R: So you are Jewish ... yes ... But what religion were you born into? What's on your birth certificate?

Me: Actually I cannot recall because I've lived away from India for quite some time ... (Fieldnotes, 8 February 2012)

The neighbourhood where I lived also proved contentious. Often, respondents could not tell my religion from the other cues, and would ask for my residential address. In a segregated city like Ahmedabad, the postcode can signal one's caste and religion. If I named the Hindu majority neighbourhood in western Ahmedabad where I resided at the time of fieldwork, Hindus felt more comfortable, whereas Muslims did not. To Muslims, therefore, I would give the name of the Muslim-majority neighbourhood in the walled city where I grew up. Both the strategies were useful to avoid further questioning, though they also implied an attempt at deception. Ethnographers have suggested that deception, or 'mild deceit to some extent' (Bulmer 1982, 253, emphasis in original; Davies 1999) in sensitive research is inescapable, providing that deliberate deception is avoided. In conflict-affected environments, especially, ethics necessarily become flexible. Flexibility, however, would not discount more serious ethical obligations, such as causing harm to the respondent – advertently or inadvertently.⁸ My behaviour did not efface the possibility of raising suspicion in the mind of the respondent, and I became prepared to lose out on potentially significant testimonies. As one Muslim respondent warily asked: 'If you have a house in western Ahmedabad and your parents' house is in the walled city, who are you? It doesn't make sense ...'

Over time, it also became clear that the insider–outsider category was a continuum, rather than each category being mutually distinct. Because of my religion-neutral identifiers, unexpectedly, respondents saw me as an insider at times when I thought I was the outsider. My attempt to negotiate access in Ram Rahimnagar is one illustration, where I took my 'insiderness' for granted. I also recall how a conversation with a Muslim respondent turned hostile after my marriage with a Jew became known. This was not information I parted with consciously, but one that the respondent obtained from their own sources – a disadvantage of being an insider is that word spreads. In this instance, I was the outsider when I thought I was one of them. Not viewing me as an 'authentic' insider could have potentially damaged the quality of my data. In this instance, the best measure to avoid future complications was to choose a different site for study, which I did. The pervasiveness of fear attributed to certain religions, such as Islam for much of the West, or Judaism for many Muslims, is likely to reconfigure the relationship between the 'researcher' and the 'researched'. As Mohammad and Sidaway (2013) argue, the Muslim researcher for the Muslim participant becomes a suspect rather than an insider, and a potential source of breaches in confidentiality and danger within the community. It was hard to know if my labelling as a 'spy' was on account of my religion. Even if I believed it was not, the potential biases such suspicion would generate demanded greater reflexivity. Conversely, I gained insightful information in a parallel situation. A Hindu police inspector candidly opened up about his resentment towards Muslims, and why the police were justified in siding with rioters in 2002. It was likely that he saw me as one of his own:

You see, a lesson had to be taught ... we Hindus (pointing to him and then to me) are tolerant people and, unlike them (Muslims), believe in the concept of *paap* and *punya* (sin and virtue). But there is a limit to tolerance.

That my position in the research process was not static helped, at the least, in offsetting what could have manifested as exclusively insider or outsider biases.

Strategies for better data quality

Cross-verification across official sources

The technique of triangulation, or the use of multiple data sources, is the standard first step towards strengthening a researcher's confidence in the data collected (e.g. Creswell and Miller 2000). I do not need to reaffirm its usefulness, though its importance should be acutely underlined, because the temptation to rely upon only interview data is high when the official data are biased. Over a pursuit of several different types of information, I learnt that, despite the obvious biases in official data, it remains a highly effective source. The key lies in connecting multiple pieces of information within these sources. In the earlier example of the death of the two watchmen, public hospital records and crime scene police reports (*panchnamas*) – both government documents – could eventually confirm that two deaths had, indeed, occurred in the site and were murders, as opposed to accidental deaths.

The best approach to identifying participants of riots is to include only those individuals who were arrested and then charged in court (Kawalerowicz and Biggs 2015). No convictions had been made in my field sites at the time, which made police charge sheets (formal police records of individuals accused of attacks) the best available official source.⁹ The charge sheets helped me identify 708 persons accused of attacks in my field sites, with their address, the time, and the place of the incident. As illustrated in the example of the deaths of the watchmen, the validity of this source was low; data were either missing or addresses were incorrect (see Shah 1973 for similar problems). However, this was a good lead. Once I had a fairly dependable list of rioters, a meticulous trust-building exercise became essential to gain access to them. In the largely semi-literate population in my field sites, calling myself a 'researcher from Oxford (University)', where I was a student at the time, drew a blank for a majority of the respondents in the field sites. The subject of my study, *samāj vīgyān* (sociology), continued to be mistaken for *samāj sevā* (social work). I was given errands suitable for a social worker, such as speaking to the local police to reclaim confiscated rickshaw licences, or summoning municipal officers to fix or supply drainage facilities. This helped, insofar as I knew the local authorities from my previous days as a journalist, though eventually I ran out of steam, because a lot of the work needed repeated follow-ups. By then, people knew I was a well-wisher. After several months of building trust, seven Hindu rioters in my research sites admitted their participation in attacks. Together with the detailed testimonies of 105 other respondents, access to these 7 rioters provided a unique perspective on the violence. At all times, I obtained verbal consent, rather than written, to avoid directly linking the respondents to my written work and, consequently, putting them at risk. As Hamill (2011, 22) notes, consent in sensitive research becomes implicit once trust has been established between researcher and respondent. There were two potential risks for the respondents. The first involved causing distress in respondents' having to recall the prior events of the conflict. The second entailed causing harm to the respondents' social networks, in the disclosure of attitudes towards interethnic contact and prejudice. To minimise the first risk, I verbally restated the nature of the interview, should respondents choose to withdraw. The second risk was minimised by anonymising participants' names. At the same time, rather than masking identities and places as a kind of ritualistic practice, I concur with Murphy and Jerolmack (2016, <https://contexts.org/blog/ethnographic-masking-in-an-era-of-data-transparency-2/>)

in using anonymisation only when necessary. I did not change the names of neighbourhoods (cf. Berenschot 2011; Goffman 2014), in order to facilitate greater (external) reliability of the data. That is, allowing another researcher to visit the same field site, and ‘use the original study as a kind of “baseline” comparison in order to specify how observed changes in interaction and the social order of the setting are the result of intervening historical forces’.¹⁰

Spontaneous discussions in group vs. individual setting

Unravelling the discrepancy between what people say, and what they actually do, is a great methodological challenge in the social sciences (Jerolmack and Khan 2014). The probing nature of in-depth ethnography can often uncover deeper information. However, inferences about situated behaviour based only on verbal accounts are likely to be fallacious, because contradictions between expressed attitudes and actions are commonplace. In ethnically tense societies, verbal accounts of past events are more likely to generate inaccuracies, both intentionally (because of fear), or unintentionally (due to memory lapses). To address this concern, I met with respondents in a group setting, followed by separately held individual conversations. By ‘group setting’, I refer to a situation where members of different religious groups are present together, with prior consent. ‘Individual conversations’ provided an unrestricted environment for the respondent, whereas group conversations closed in on group interaction, focusing on socially desirable behaviours. Because attitudes and action are context-dependent, Erving Goffman’s ‘interaction order’, or how people relate to other people and in particular situations, is more effective in understanding social action than structured interviews or surveys (Jerolmack and Khan 2014, 181; also, Eliasoph 1998). The conversation that occurred between Dhara and Munaf, illustrated earlier, took on a different meaning when I met Dhara a day later: ‘Because we all live together, some *vyahavar* (decorum) has to be maintained.’ Elsewhere (Dhattiwala 2016b), I explain the superficial cordiality apparent in group conversations to be a form of everyday negotiation aimed to assuage antipathy between neighbours at risk of imminent violence. The crucial point was not the fact that people lied – that is inevitable in conflict sites – but why they lied when they did. The different interview settings helped me identify mismatches in testimonies reasonably well. This, in turn, suggested that the ‘untruths’ were a mechanism to build a cohesive space of shared interests, in an otherwise highly fragmented landscape.

Chronological and cognitive maps

Two other useful validation methods involved mapping space and time. A time map involved the construction of a detailed chronology of the events by date and time using the multiple sources of information. This created a credible timeline of the sequence of events, disentangling ambiguous pieces of information that came up in testimonies. Mapping space was a two-step process. In the first step, I used the newspaper-based dataset compiled earlier to match the precise location of violent incidents on an administrative map of the field site. Replicable visual data of this kind became the base to compare participant-empowered visual data acquired through cognitive maps. The second step was the cognitive mapping exercise, which helped to buttress interview and numerical data. The typical cognitive map is a conceptual manifestation of a person’s place-based experience (Golledge and Gärling 2003; Kitchin 1994; Tubaro, Ryan, and D’Angelo 2016), produced in the form of external representations of space, such as sketch maps. In

an experiment in the 1970s, 200 Parisians were asked to draw maps of their city ‘in which they were to mention all of the elements of the city that came to mind’ (Milgram 1977, 78). The points of interest for a 33-year-old butcher – slaughterhouses and stockyards – mattered little to a 50-year-old woman, who instead made sure to annotate landmarks such as the Louvre and the Palais Royale. As this illustrates, people create their own beliefs about the environment they live in and may, sometimes, also act accordingly (Wong et al. 2012).

I provided respondents with a blank paper and pen, marked with a circle indicating their own house. Relative to their house, they were then asked to sketch a line drawing to show the house of their ‘favourite neighbour’ (*aapka sabse achcha padosi* in Hindi; *tamara manpasand paadoshi* in Gujarati) (see also Grannis 2009). A majority of respondents indicated, by way of their line drawings, their favourite neighbour as someone *not* spatially proximate. Individuals were found to be living several blocks away and, in some cases, more than a kilometre away. Crucially, although neighbourhoods were characteristically heterogeneous, they were not necessarily cosmopolitan (Dhattiwala 2016b). Figure 1 is a sample of one of the 49 maps collected this way.

Information of this nature was difficult to acquire through interviews. Before deciding to employ cognitive maps, I had put two questions to respondents, in both group and individual settings. First, with which neighbour do you share positive relations?¹¹ Second, how would you describe your relations with your next-door neighbour? The answers to the second question generated almost regimented responses (along the lines of ‘all’s fine between us’), in turn indicating the compulsion of sustaining ‘good’ social relations with contiguous households. To the first question, respondents were likely to make rapid hand gestures, pointing towards the individual’s house, which revealed little about spatial distance. Some respondents offered to give me the full address of their neighbour. This was best avoided, because it would have linked people who had not consented to participate directly to the study.

Participant-empowered visual data became a methodologically and analytically valuable alternative. Data from sketch maps are found to be not only reliable (Blades 1990),

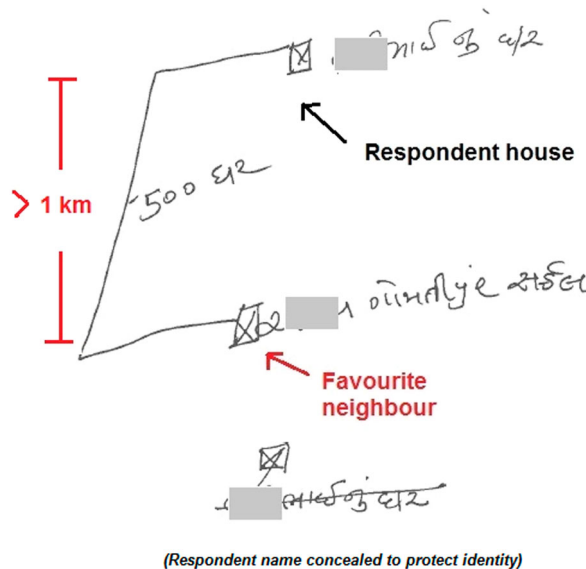


Figure 1. Sample of a cognitive map.

but also more successful in capturing abstract social concepts (Tubaro, Ryan, and D'Angelo 2016). Allowing the respondent to take control alleviated data contamination from potential subjective biases emerging from my position. Indeed, visual data are a reliable technique of cross-verification, most valuable when used in combination with testimonial and documentary evidence, rather than on their own. Any biases arising from the respondent's subjectivity would, therefore, be reduced. Over time, I realised that the exercise also served to empower the participant emotionally. Holding a pen and a paper in hand lent a tangible sense of dignity to the largely semi-literate participants. One of them, a school dropout, said she felt nervous but 'happy on the inside' to put pen to paper after many years.

Teasing out the association of relational and geographical distance was the main rationale behind asking the respondent to identify the 'favourite', rather than 'beneficial', neighbour with whom they shared positive ties. Compared to neighbours, kin ties often produce relatively close, frequent contacts among those who are at a great geographic distance, because extended kin are ranked higher on the variable of ascription or permanence (Parsons 1949). A favourite neighbour was very likely to be a relative, which would explain the greater geographic distance. It was, therefore, significant to find that a majority did not profess their self-identified favourite neighbours as kin, despite the greater distance.

Conclusions

Research on the field is a constant endeavour to ensure good data quality within ethical constraints. For an insider researcher conducting fieldwork in conflict sites, their own positional bias enhances data concerns that are inevitable in an uncertain field site – concerns of partisan official records and inconsistent respondent testimonies.

This article illustrates the methodological and ethical dilemmas of an insider researcher – someone who is a member of the group being researched – conducting fieldwork in the heterogeneous, urban neighbourhoods of Ahmedabad (western India), the site of extreme Hindu–Muslim violence in 2002. This episode of violence, among the worst in modern India, was characterised by the active role of political elites in orchestrating attacks; empirically grounded studies have since deemed the violence a planned 'pogrom' against Muslims, rather than a spontaneous 'riot' between Hindus and Muslims. Notably, in absence of timely judicial redress, a number of targets of the violence were compelled to share neighbourhood spaces with perpetrators making the research process more problematic.

The central aim of the study was to identify the risk factors leading to the violence, by comparing spatial units with different levels of violence. It was crucial to collect data that provided a reasonably accurate timeline of events before the actual violence. In order to explain the behaviour of individual participants in acts of violence, identifying the participants was critical – an inevitable difficulty in civilian violence. Therefore, by recounting the dilemmas I faced on the field, my intention in this article has been to demonstrate the subsequent strategies of data collection and its analysis that were used to limit the bias of ambiguous testimonies, one-sided official data, and my own idiosyncratic experiences as an insider. Indeed, value-neutrality in the social sciences in the absolute sense is impossible, regardless of the paradigm of research. But that would not discount the obligatory methodological and ethical measures that the researcher should take to improve data quality on the field.

I propose three different strategies to improve data quality. The first illustrates triangulation within official data sources. Given that the government rarely provides complete or accurate data about violence, hospital records and police charge sheets help to connect the dots. Police charge sheets, even when incomplete or inaccurate, provide useful pointers at identifying probable participants in violence. The second method employs spontaneous

interviews in individual as well as group settings, a strategy that seeks to reduce the attitudinal fallacy, or the mismatch between the spoken word and actual behaviour. The third method expounds upon the use of visual data, in this case cognitive maps, as a means to conceptualise the abstract notion of the 'neighbour'. The maps allow respondents to take control of data, thereby alleviating the researcher's positional bias from contaminating the data. I emphasise the use of these methods in combination, rather than in isolation.

Cognitive maps and the interview methods suggested here bear important implications for neighbourhood studies in multi-diverse societies. Multi-diversity is not sufficient to induce cosmopolitanism, because the mere distribution of groups in close geographic proximity does not guarantee beneficial contact. Race relations in densely populated US cities have been found to develop as a consequence of residential street patterns, which rarely cross racial lines, rather than spatial proximity (Grannis 2009). Compare this with dense, heterogeneous neighbourhoods in the so-called developing world, where urban street networks, as planned on paper, begin to reflect a different reality, as spaces shrink and illegal settlements grow. When coupled with ethnic tensions, such urban spatial arrangements veto the choice of mobility for many residents. Explaining social phenomenon becomes difficult, but at the same time, the aspiration for rigour and ethical principles goads the researcher into creative methodological frameworks. The strategies suggested in this article provide one such framework that could help develop particular practices towards better research.

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Notes

1. Muslims accused of the Godhra train burning incident faced immediate arrest in 2002 and 31 were convicted in 2011 (*The Hindu*, February 23, 2011), unlike manipulations in investigations of attacks subsequent to the Godhra incident (Mitta 2014).

2. Ahmedabad had witnessed the worst violence across all urban centres, yet 22 of its 43 municipal wards had remained completely peaceful.
3. Respondent names are anonymised; place names are unchanged.
4. In a study of the influence of gender on trust, Buchan, Croson, and Solnick (2008) find that women are perceived as more trustworthy than men whereas men trust more than women.
5. This is arguable, of course. Yet, compared to the unapologetic masculinity of the streets of North India, where I now live, growing up and commuting alone in Ahmedabad was always less challenging (see Kumar 2016).
6. The bridges on the Sabarmati River are symbolic of the deeply fractured geography of Ahmedabad, dividing neighbourhoods along religious lines.
7. My husband is, indeed, Jewish. Therefore this was not a lie. Few Hindus in my fieldsites, however, knew who Jews were ('Yahudi' in Hindi and Urdu), until I mentioned Israel and explained the association. The Israel-Palestine conflict is a familiar subject to many Indians, and Israel (and Jews) are looked upon as allies (situated along the lines of the India-Pakistan conflict). Because of a patrilineal society, it was understood that the religion of my husband is my religion.
8. For a very useful discussion on ethics in sensitive ethnography, Steven Lubet's review of Goffman (2014) in *The New Rambler* is revealing. Available here: <http://newramblerreview.com/book-reviews/law/ethics-on-the-run>
9. I could get a comprehensive list of convicted perpetrators for one of my fieldsites, Naroda Patiya, only in 2012, when legal documents of the case became available.
10. From a blogpost by Murphy, A. and C. Jerolmack, March 19, 2016: <https://contexts.org/blog/ethnographic-masking-in-an-era-of-data-transparency-2/>
11. It was iterated that the respondent should identify a neighbour, not a friend.

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