Is anthropology legal? Earthquakes, blitzkrieg, and ethical futures

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Abstract: This article is a contribution to the growing literature that suggests that the methodological and writing practices of anthropology are out of kilter with the times. The processual structures and regulative mechanisms that produce anthropological knowledge were formed when objection and engagement were not the almost-inevitable consequence of publication. Those who inform anthropological research now frequently object to the ways they are represented. My argument here focuses particularly on the relationship between the ethical structures of anthropology and the nature of objection. Thus far, the consistent response from anthropologists has been to explain away objections as differences in epistemology. In this light, I draw on an objection to my own research on postdisaster reconstruction in India to ask why there should not be disagreement between anthropologists and those who inform research. I also illustrate why the epistemological explanation is now insufficient and why new structures of research and writing might be required to make the leap from an age of objection.

Keywords: ethics, legality, methodology, objection, post-exotic

blitzkrieg (German): lighting war; sudden and surprise attack

I do not know exactly when the age of objection dawned on the anthropological landscape. Those who lived in the houses close to Malinowski's tent probably thought he was a queer cove on occasion, but their opinions have not survived. Dawn was more clearly signaled by the emergence of anguished accounts by anthropologists who had published material that those who informed their ethnography objected to (see Brettell 1993 for an overview). Generally, those who objected did not like how they were represented. Specifically, they thought the anthropologist had made misleading connections, revealed secrets, and drawn on inappropriate evidence. Others have made the profound but difficult point that they have a right to lead "unexamined" lives (Scheper-Hughes 2001: xvi). Objections might also be raised to the perpetuation of the extractive and metropolitan traditions of anthropology in a more general sense.

Objection is an exciting challenge to complacency, a signal that things could be done differently. Objection also signifies that anthropology is being read, and read by those with both inclination and motivation to articulate their objections. Having said that, when objections were raised to my anthropology, I found the experience both stressful and unpleasant. Why? Also, why should it have come as such a confounding surprise? I had not been trained to expect, accommodate, or respond to it. I had spent more than twenty years diligently refining a disciplinary practice, which my objectors simply dismissed as "fiction"—in the "made up" sense, not in the nicer sense of Writing culture (Clifford and Marcus 1986).

Global structures and locations of power have changed since the coming of modern anthropology. As David Mosse succinctly puts it: the desk has collapsed into the field (2006: 937). People and things are mixed up more and differently: China builds roads and grows food in Africa; India is one of the largest investors in the economy of the United Kingdom; everybody's carbon emissions are melting polar and Andean ice. Anthropological knowledge

is no longer "our" exclusive possession; "we" are no longer a "we" (although the idea that "we" once were is usually overstated). For former colonists, the discipline helped to ease and explain the transition from colonial to postcolonial world order. Postcolonial anthropology inherited a mixture of guilt, self-interest, and intellectual curiosity, which, arguably, continue to form the structure the discipline.

Now there are political movements in many parts of the world that have deliberately rekindled memories of colonialism, integrating an idea of revenge or redistributive justice into their political mission. Invasions, bombings, and drones have given rise to new forms of inequality and conflict. Post-postmodern states of precariousness have been introduced. Parts of the world are off limits because they are too dangerous; other parts of the world restrict access through border and visa controls. Post-postcolonial anthropology faces new challenges: shifting empires and ethics and notions of democracy and truth. Anthropology should belong to these times.

Information technologies have transformed the accessibility, reach, and pace of communication. Social media in particular have encouraged a general awareness of the politics of self-presentation and representation. In some ways, social networking has made mainstream many of the ideas about relationships, kinship, and connectedness that anthropologists had been talking about behind closed doors for decades. The ways in which data are stored, sorted, and shared continue to shift. Transnationalism has created bridges and fences that do not simply recreate the older patterns and sympathies of nation-states. Phenomena such as deforestation, terrorism, and disease, once framed by the intricacies of particular localities, are often now considered global concerns; at other moments, one part of the world worries about such issues in another. Anthropology has new terrains and modes of engagement, as well as new publics and audiences.

The subject area of anthropology has both expanded and contracted because of new geopolitics. New forms of anthropology are emerging outside the traditional university and national settings. Significantly, for many anthropologists, radical difference or alterity has ceased to be the core generative idea of the discipline. Like colleagues in New Delhi, for example, with whom I have been debating such issues, I find the idea of absolute cultural difference or relativism or ontology untenable intellectually and politically. But then, those who objected to my anthropology perhaps think this stance is part of the problem, because they thrive on the public idea of cultural relativism. Instead, as I have gradually moved from ethnographic practice rooted in a particular location toward the anthropology of big stuff, earthquakes, infrastructure, and the like, I have found myself focusing on processes, consequences, and the ripples caused by things rather than on bounded groups of people. People still matter, of course; however, when previously religious, regional, or linguistic labels might have seemed the most salient units of analysis, I now tend to see such things as layers that are nestled within other kinds of layering such as institutions, policies, histories, ideologies, and so on.

Writing people

Together, these global trends alongside shifting topics and writing and publishing technologies mean that those who inform anthropology are often now in a position to read what is written about them. Consequently, anthropological writing about cheated, disenchanted, and angry informants/research participants/interlocutors is emerging as an important and challenging subgenre. This new tradition is clearly in its infancy, not having moved far beyond the self-justificatory trope. The fact of disagreement is to be explained, often as a piece of anthropology itself; rendering it an intra-anthropological subject matter,

which effects a double exclusion of informants. Disagreement or objection emerges, usually quite genuinely, as differing epistemologies and expectations. Objection has yet to be embraced, solicited, or understood to signal the possibility that anthropology needs to be done differently. However, before I continue with this tack, I want to rhetorically ask you why there should not be disagreement between anthropologists and those who inform research? Why should we expect informants to approve texts? What kind of relationship does this imply?

The first time I deliberately shared my anthropology with someone in India was a pointed learning experience. The man in question, whom I felt I knew quite well, was very angry about what I had written. He said I had misquoted him, turned his story into a banality, and portrayed him as an eccentric. Furthermore, I had not even bothered to get his name right. Just moments before he told me this, I had been excited about sharing my work, believing I had quoted him precisely and generously, represented his story as something profound and bold, and portrayed him as a heroic pioneer. I had, of course, used a pseudonym rather than his real name, as was expected of me in universityland, which is why he thought I had got his name wrong. This humbling experience made me begin to understand what anthropology often does to people, and what it cannot do.

Anthropology puts people through the mill. At one level, anthropologists turn people into writing. Writing is words, sentences, and paragraphs, which cannot replicate the complexities and particular sense of self-integrity possessed by an individual. Anthropology can be neither a vehicle for the communication of the unspoken consciousness of an other nor a reflection of an ego. The gaze and techniques are geared to do other things, which have been characterized in many varied ways. Hastrup (2004), for example, suggests anthropology is a kind of explanation beyond the truth of events themselves. Anthropology is not simply knowledge about particular events, practices, and ideas but about the processes by which

these come to appear meaningful, inevitable or mandatory, contestable or ridiculous. My own experiences of sharing academic writing have also impressed on me that anthropology is a rather violent and abrupt process, which takes place slowly in a gradual and usually friendly fashion: pious fraudulence, professionalized duplicity, or ritualistic sanctimony?

In addition, there are other issues to consider here too, which add to the difficulties of non-anthropologists understanding anthropology, such as the precarious intellectual edifice which is an anthropological argument, and the drift and consequences of our own professional ethics codes, but I will deal with these at greater length in what is to come. This article points to some of the directions in which anthropology might need to shift in the age of objection. The research material is drawn from my own Gujarat ethnography and correspondence and encounters in the United Kingdom. I conclude with a salutary discussion of research ethics and questions about the possible legal status of published anthropology.

Background and key terms

In 2004 I contributed a chapter titled "Hindutva as a rural planning paradigm" to an edited collection on popular political mobilization in India. Nine years later, two sadhus ("priests") from a Hindu religious movement presented me with their written objections. In what follows, I reflect on what I wrote, how they responded, and what might have happened in the intervening years. The encounter made me appreciate that anthropology is run through with enchanted ideas that are largely incomprehensible to a generally educated reader expecting "science." Paradoxically almost, some forms of anthropology have moved toward becoming an exotic form of knowledge in a world that is increasingly seen as "post-exotic" (Elie 2012). However, the more constructive conclusion from this encounter is: anthropological knowledge is humanitarian, disruptive, and important but needs to develop more robust and

politically astute ways of explaining, presenting, and defending itself.

The "Hindutva" of the 2004 chapter's title means "Hindu-ness" and is associated with various kinds of Hindu culturalist and nationalist philosophy (Savarkar 1938; for contrasting overviews see Bhatt 2001; Hansen 1999; Mathur 2008). Today these ideas vary in status between unequivocal truth, common sense, pride, and a stance. Significantly, the various strands of nationalist philosophy deny a distinction between political rule and religion, seeing this as an artifice of "Western" cultures and a legacy of colonialism. Instead, the ideas of Indian civilization, which are understood exclusively as the ideas of Hindu civilization, see rule and religiosity as part of the same complex (Upadhyaya 1965, for example). Hindutva as a political ideology emerged strongly in the early twentieth century as part of the anticolonial movement. In the postcolonial era, as the ideology was encouraged to enter the mainstream at all levels, Hindutva became an increasingly nationalistic way of thinking about the organization of history, politics, and social life. In my 2004 chapter, I suggested that variants of such ideas served as paradigmatic guides for some of the organizations that had reconstructed villages after an earthquake in Gujarat, western India, in 2001.

My fieldwork at this time allowed me to see at first hand the aftermath of mass anti-Muslim violence in Gujarat during 2002. I sat through some of the ensuing "hate speeches" made by Hindu politicians in the name of post-earthquake reconstruction and nationalist politics. I also saw and heard horse trading as a neoliberal arrangement brought public and private partners together in the reconstruction initiative. Consequently, in my 2004 chapter, I also made the unremarkable claim that an earthquake in Gujarat had proved an opportunity for many organizations, especially those of religious and cultural bent, to advance particular agendas. I use the term "unremarkable" not to be self-deprecating but because opportunism was everywhere during those early years after the earthquake: I thought I was restating the obvious.

This immediate post-earthquake period also marked the deliberate intensification and expansion of nationalist politics (see Shah 2002; Shani 2007), which eventually brought Narendra Modi to power on a BJP ticket as the national leader of India in 2014. The BJP or Bharatiya Janata Party is commonly thought of as the Hindu nationalist party in India, and, as the then president of the party publicly said as far back as 2002, the "experiment" in populism in Gujarat was the preparatory step to pushing its agenda to the national level.[1] In fact, it seems unlikely that Modi would have been able to come to power in Gujarat in 2001, at least in the way that he did, without the earthquake. He and his advisors used the aftershocks to topple an incumbent Keshubhai Patel from the chief minister's seat; Modi became the chief minister of the state without having faced an election. His subsequent rule over Gujarat is commonly associated with strong economic growth figures, violence, firebrand politics, and rapacious industrialization. Against this backdrop, in my work on the aftermath of the earthquake, I chose to focus on, among other things, the everyday manufactured seep of nationalistic ideas into the lives of ordinary people. I saw that this "seep" was often encouraged by mechanisms and processes that were clearly both organized and orchestrated.

Plot, main cast, and bit players

Those who objected to my own anthropology in an unpublished and unsigned document called me an "academic fraud" writing with "improper ideological bias" and "substandard ethnography." Those I imagine to be the authors are members of a Hindu religious movement that had "adopted" a village in Gujarat through a public-private reconstruction initiative. This village was not particularly damaged by the earthquake, but this significant point seems to have been allowed to fade over the years. The movement built a new village slightly away

from the original location and gave it a new name. As other organizations were doing in hundreds of other villages, the movement facilitated the construction of infrastructure and houses. They also built a temple in the center of the new village; this was, significantly, a rather less common occurrence throughout the region as a whole.

From my point of view, evidence for my claim that the earthquake advanced a particular political project was ubiquitous. New temples appeared; villages, streets, and buildings were renamed; and "victims" of the earthquake were demonstrably pressured to adopt particular ways of being. Given the violence, jingoism, and general fervor that saturated Gujarat at the time, it did not occur to me that provocation would be found in my conclusions.

In 2009 John Zavos, chief editor of the original book, told me that "BAPS" had concerns about my chapter. BAPS (or Bochasanwasi Shri Akshar Purushottam) is the Hindu movement that had built one of the three villages referred to in the 2004 chapter. Founded in 1907, BAPS is particularly popular in Gujarat and among the sizable diaspora overseas. I do not know how BAPS learned of the chapter or developed semipublic concerns. John Zavos emerged as a broker, although aside from brief email exchanges I do not recall him asking me directly about the contents of the chapter. At the time, he was also starting to conduct research on the movement himself.

In between the original publication and the delivery of the objection, I edited a book along with Aparna Kapadia called *The idea of Gujarat* (Simpson and Kapadia 2010). Hanna Kim, who conducts research with BAPS, was one of the contributing authors. During the production of that volume, she forwarded our considerable editorial correspondence about her chapter to BAPS for scrutiny and comment. She later apologized to me for this unusual act, I believe sincerely. However, the episode made me aware of the kinds of relationships researchers working with BAPS have to entertain to retain research access.

Kim's chapter (2010) argued that Swaminarayan (BAPS) ways of being are not dependent on Western notions of individualism, autonomy, and subjectivity. There is poignancy in this conclusion because as a woman Kim cannot speak to the sadhus in person and has to use a proxy. She critically reviewed the literature that portrays the movement as a player in right-wing political Hinduism (especially Nussbaum 2007; McKean 1996; Shukla 1997). At the time, our editorial line was that Kim's project was a worthwhile and unsettling one, but that we should not uncritically reproduce ideas given to her by sadhus without contextualizing them against a broader backdrop of events in Gujarat. However, Kim's claim (also 2009, 2012) that the movement was "apolitical" raised particular intellectual and methodological concerns for me.

Like Kim, BAPS also repeatedly stresses this sentiment and has developed a well-rehearsed narrative to support the claim to be "apolitical"; but what BAPS appears to mean by this is that—variously, and not consistently—the trustees of their temples are not members of political parties. Their statement is not and cannot be a denial of involvement in what social scientists generally consider as politics in the more general sense, as I discuss further below.

At the suggestion of John Zavos, I arranged to meet with the sadhus for a discussion. Despite having invited one person, four men turned up at the School for Oriental and African Studies (SOAS) and caused considerable disruption in the corridor. The spokesman on that occasion told me that when they had visited Dr. Zavos at the University of Manchester, he told them I was writing a book. They also told me that they had come with an "open heart to improve my scholarship," but that they were also interested in just punishment for my slander, institutional discipline, or my resignation to save the face of my institution. The sadhu said that anthropology should be "representative" and that there should be "triangulation" in my method. I tried to explain, naïvely with hindsight, that ethnographic

fieldwork had given me a good overview of the region, and we could also think of this tacit or background knowledge as part of the "triangulation" process.

The original spokesman came again with another sadhu in 2013, this time carrying 110 printed pages of objection. In my 2004 chapter, there were four pages about the activities of BAPS (Simpson 2004: 154–158; they also comment briefly on 2008). Their document is titled "BAPS response to Dr Edward Simpson's works." The first section sets out their case and runs through at great length a matter on which there is no disagreement, although our conclusions vary. The second carefully outlines the policies and procedures through which village adoption and design took place. The third section is an attempt to demonstrate that BAPS has no favorable relationship with the state in Gujarat and no links with any political form of Hinduism.

The document presumes the existence of "facts" that can be accurately presented irrespective of methodology, discipline, or political positioning. It reads and is structured like a legal case. What strikes me most, however, is the straightforward nature of the world they describe: government policy determines how things happen; regulations regulate; building codes produce concrete realities; BAPS simply acts within the procedures of a regulatory state. In their presentation of post-earthquake reconstruction, there is no slippage of meaning, ambiguity, or room for different interpretations. While the authors could well have had their own reasons for presenting the world in this way, I doubt it was a self-conscious strategy. Of the two sadhus who may have written the document, neither was directly involved in the adoption and reconstruction of villages in Gujarat. At the time, one was an undergraduate student and the other was yet to be initiated; neither has a background in social science. Instead, I suspect their approach to what I had written points to a more general disparity between how critical social anthropologists describe the world and how others see the world as working.

At the most general level, anthropology has become a discipline for which things are contextual, in-the-making, hybrid, illusive, and ambiguous. In part, of course, the origins of this style lie with the "crisis of representation" and the "self-reflexive turn" in anthropology (attributed to Clifford and Marcus 1986). At another level, it is underpinned by the organizing ideas current within the discipline: discourses make realties, power lies between the cracks, uncertainties produce realities, ideas have histories which are contingent and can be taken apart, and so forth.

Moving closer to the subject of my own 2004 chapter, works by Scott (1999) and Ferguson (1990) have convinced us of how the state really works. For many such authors, bureaucracy, for example, has come to be thought of as a form of negotiation and competition rather than an uncontested process (Hull 2012). The state and other organizations have been shown to work within discursive realms, which mean they act quite at odds with how other people think they should act. Charisma, authority, and history drown even the possibility of planning decisions and bureaucratic procedures, as they exist on paper, actually determining how things are done in any straightforward sense. Humanitarianism can be deconstructed and historicized (Fassin 2012). Development projects can be shown to act according to their own institutional culture rather than in the interests of those they are supposed to serve (Mosse 2005). In sum, the certainties on which current anthropology is based have become most "unscientific" (in the common usage of the world). The science of anthropology, if it can be thought of in that way, is about complexity, ambiguity, partiality, and shades thereof.

My own work too is framed by a twist of reality which I think, in part, led the sadhus to understand what I had written in the way that they did.

Research in Gujarat

I had conducted research in Gujarat before the earthquake of 2001. Partially because of this, my initial attention was drawn to my friends who survived. When I first visited after the earthquake in the second half of that year, it was obvious that the prevailing worry of those I knew was uncertainty about what was to come next. They were struggling to interact with and understand the many organizations who had come as humanitarians in the aftermath. Many of those who came to intervene saw the subjects of their humanitarianism as victims in a double sense: victims of tragedy and victims of impoverished cultural and religious regimes. The public-private partnership apparatus, filtered through the state government as a condition of loans from development banks, allowed those intervening to gift new forms of understanding. State sanction allowed these interventionists to build villages imbued with particular ideologies, some possibly with longer-term collaborative goals in mind. The desire to help those who are suffering usually appears as an unquestionable moral imperative. It is then a delicate matter to critically engage or question the "good works" of humanitarians.[2] I do not think for one moment that "good work" is inherently bad, but it is never neutral or innocent as the document the sadhus prepared claims. From the perspective of critical anthropology, "good work" can only be a form of politics and intention.

As the research went on in Gujarat it became increasingly obvious that most of what is known about disasters is produced by those who have a stake in presenting reconstruction as a success. The reasons for this might be many, but to name a few: to preserve a mandate, satisfy auditors, or to demonstrate that stated aims have been fulfilled. The hundreds of reports produced by organizations such as BAPS, development agencies, and governments about the earthquake are not about failure, any more than they are about the lives of ordinary people. As Foucault aptly words it: "discourses are practices which systematically form the objects of which they speak" (1972: 49). The types of discourse or general knowledge that

emerge about natural disasters serve particular ends and actively make disasters appear in particular ways.

It is against this backdrop that I eventually attempted to describe the aftermath of a natural disaster from the point of view of those who were affected by external interventions (Simpson 2013). If we take this naïve perspective seriously, then quite often humanitarians appear as strangers and trespassers. Their actions, gifts, and ideas of a better future no longer seem routinely sensible or so morally untouchable. On the contrary, humanitarianism may well appear as a form of self-interest when viewed from the perspective of those who are affected by it. Of course, this is not how humanitarians understand their role—it cannot be; but that is the very point I have attempted to make in my work.

My framing of post-earthquake reconstruction was not what the sadhus expected social science to be, let alone a good account of humanitarian reason. The argument was not intended simply as a criticism of humanitarianism inspired by religious and organizational values but rather as an analysis of the interplay of post-earthquake reconstruction initiatives, policies, and institutions. The example of the village at the core of this dispute was an exploration of a general theme that characterized the landscape of post-earthquake reconstruction in a broader sense; indeed, other parts of the 2004 chapter told similar stories about the consequences of the intervention of other organizations. This was not a story about the interventions of a particular religious movement; rather, it was a story about a world in which there was an advancing tide of a distinct nationalist politics within which there were multiple points of view and truths.

The account I wrote focused on people who lived outside and beyond planned interventions, as well as on those who were directly affected by them. These people introduced distracting and unpleasant details that are at odds with the hermetic description of the world an institution might provide. My method refused, deliberately, to explain the

outcomes of post-earthquake interventions in the language of the development organizations. In short, I was writing from outside the paradigms through which institutions see and understand their own interventions. This approach was meant to be unsettling and challenging, but not to produce high dudgeon.

Putting air back into the vacuum

The bruising document presented to me at SOAS is also concerned with discrediting my research. In this respect, I began to read both the document and the mechanisms through which it had been brought into SOAS as part of a greater attempt to police and manufacture the boundaries of a discursive realm of transnational political Hinduism. In other words, there were politics and intentions in the objection, which spoke to ideas and debates far beyond the content of my anthropology. Other scholars have shown that it is part of the intellectual project of the Hindu right, of which they see BAPS to be a part, to shift the basis of ethical judgments, insert the trope of "Hindu hurt" within British politics, and reconfigure the nature of political identities (Mukta 2000). This essentially seems to me to be what sections of the sadhus' document attempt to do.

The third part of their document makes the strong claim that BAPS has no formal links with the Hindu right. Indeed, there are no underground tunnels or charters that I am aware of. However, the persistent and conspicuous nature of this distancing claim in the literature and web presence of the movement is curious. I know people in India and London who regularly attend the movement's temples and have views that could not readily be mistaken for anything other than those of hard-line Hindu nationalists. It is as if the lack of official affiliation or membership card is sufficient evidence to sustain the claim that a religious movement is apolitical.

It is difficult to translate the claim "apolitical" back into the language of anthropology: no word exists with currency. In the conventions of the discipline we should render it *apolitical*, as we do with terms from other languages that do not translate well into the languages in which anthropology is written. It is not just that the realm of the political has spread like a slick into all areas of social life, the very claim to being "apolitical" can itself only be a political contrivance. From the perspective of critical anthropology, the kind of Hindu sociality the movement promotes—its ideas about history, exclusivity, and everyday morality—resonate strongly with the interests of the Hindu political right; consequently, while the management of movement may be formally "apolitical," many of the sect's followers are not. This in turn gives the sect considerable clout in local political setups and reflects what social scientists in the Weberian tradition would call a strong "elective affinity" with the political organizations of the Hindu right. Such affiliations are, of course, a routine part of the democratic process, in which lobbyists, interest groups, and so forth combine to exert greater pressure.

Other scholars have also noted the concerted and efforts BAPS makes to distance itself from the institutions of the organized political right (Anderson 2010; Dwyer 1996). In 2005 a UK parliamentary commission investigated the links between the BAPS's temple in London and "international terrorism" (Denham 2005). The use of the word "terrorism" was misguided and must have served as a useful foil to obscure rather more routine connections with the Hindu right. Inevitably, given the terms of its engagement, the commission concluded that no evidence existed to link the temple to terrorism.

In the course of the evidence, nothing however was presented to show that BAPS and the political organizations of the Hindu right do not have routine connections, such as overlapping bases of support and interests in common; nor was there any discussion of the relationship between the BJP and the making of the grassroots support for the party and

movement. There was no account of BAPS's plan for becoming a national movement in India, which according to Raymond Williams (2001: 233) includes emphasizing "all-India" aspects of Hinduism and stressing the close association of national identity and Hindu "culture." It also includes building temples in key political and symbolic cities in the country, such as Delhi and Ayodhya (Anderson 2010). Furthermore, there was no public exploration of the movement's designs on the Gujarati diaspora and no analysis of its extremely particular presentation of religion and history, which some scholars have likened directly to Hindu nationalism (Mukta 2000).

The suggestion that BAPS had links to the Hindu right in India in my 2004 chapter seemed unequivocal at the time. In 2001 Narendra Modi was shoehorned into government never having faced an election. In February and March 2002, he was chief minister while there was mass killing in the name of religion in many parts of Gujarat. His government has consistently been found by various investigative committees as somewhere between supine and complicit in the bloodshed. The BAPS's village was inaugurated by him in May of the same year, and given press coverage, at the height of the communal surge in the state. Similarly, the temple in North London was inaugurated in 1995 by L.K. Advani, who was then the firebrand leader of the BJP in India. The inauguration of religious spaces by political figures who are well known in India to represent strong forms of Hindu nationalist ideas suggests an active relationship and identification between the movement and politics. As Anderson (2010) has also documented, BAPS hosts figures from the Hindu political right in North London and has made public pronouncements in support of some of the key aims of the formal nationalist movement.

Prior to the parliamentary investigation of 2005, there had been other allegations about the use of public funds from Britain to further the cause of the Hindu right in the aftermath of the earthquake in Gujarat. One fund-raising initiative that came in for close scrutiny was

SEWA International, an offshoot of the Hindu Swayamsevak Sangh (HSS), an organization

that is in turn modeled on the Rashtriya Swayamsevak Sangh (RSS). The RSS is the

organization most associated with the promotion the Hindu cause in India. A report also

published in 2004 by the activist organization South Asia Watch Limited claimed that more

than two million pounds raised by SEWA International in the UK had gone directly to

funding the Hindu right in India.[3] A senior UK politician disassociated himself from the

organization as a consequence. Investigators from the Charity Commission were refused

visas to visit India. Ultimately, SEWA International was cleared of financial wrongdoing in

the UK, but investigations laid bare the connections, modes of operation, and the ethos of the

organization.

In 2012 the same SEWA International opened a rural training center adjacent to the

BAPS's temple in the village mentioned in my 2004 chapter. At that time, I had suggested

BAPS was furthering a broader agenda. "No," the sadhus had said. Narendra Modi himself

landed on the village's new helipad to inaugurate the building. Not many villages in India

have helipads, and this one had presumably been built to facilitate the arrival of spiritual and

political leaders from faraway urban centers. On this occasion, Modi paid homage to Dr.

Hedgewar, the founder of the RSS.[4] In his speech, he confused (with a smile) the actual name

of the village with the spiritual leader of BAPS, calling it "Pramukh Swami's Village"

(*nagar*). Perhaps, I thought, while watching Modi's speech on the Internet, with 110 pages

BAPS simply doth protest too much.

The publisher's take

Varying in form and intensity, this story has been with me for most of this century. As

various other institutions and publishers became involved, their reactions began to make me

suspect that most anthropology has a precarious legal status. At the time I was first presented with the document objecting to my anthropology, I was working on a book about the earthquake. As part of my immediate response to the document I wrote a long and detailed chapter based on a conventional range of anthropological source material about the various roles BAPS had played in post-earthquake reconstruction.

The manuscript got stuck with the publisher. Academic reviewers saw no problems with it—one wondered why I did not just use the term "fascists" for the general political turn I was describing in Gujarat; or, more carefully, as "promoters of neo-corporatism in the classic form." However, the pragmatic publisher knew better. He pointed to the ways in which colonial and postcolonial history has allowed Hinduism to appear as "innocent" and "truthful" in the United Kingdom, and how the growing narrative of "Hindu hurt" was finding an increasing place in the media. He also suggested that if an Islamic organization had turned up in a university and made similar claims against an academic, then the story would make the national newspapers as a story of intimidation rather than rival truth claims.

After considerable discussion, he insisted that nothing was to appear in the chapter of which I did not have a recorded interview. Should matters turn toward a court or the threat of legal costs, then we would stand a chance. In a stroke, his clear censure suggested to me that most written anthropology can probably be made to appear illegal. Most of what anthropologists present as evidence would be treated by the courts as hearsay, defamation, and/or slander. The publisher added that there was no point in hiding behind "allegedly" or "insinuation" or "parody," as these devices would not withstand scrutiny either.

To be clear, against the strongest fieldwork and methodological injunctions of the day (Behar 2003; Borneman and Hammoudi 2009), the accumulated sense of things anthropologists gain from long-term research and participant observation in the field were inadmissible—unpublishable in this context. I had written an anthropological monograph,

and now I was being asked to refine and streamline sections of it and phrase it in the recorded words of other people. What about being there? What about participant observation and apprenticeships in ways of being, as in Bourdieu's "reflexive sociology" (2008)? What about the apprenticeship of learning to see things from a different perspective? What about fieldnotes, surveys, and diaries? What about the unexpected but influential conversation, the seminal error of judgment, or the moment mistrust became friendship? What about the hunches, sensations, inclinations, the moment of a doubtful glance, or tension and embarrassment of an obviously overstated reassurance? The publisher was suggesting in no uncertain terms that the methods and materials from which anthropology is traditionally constructed were insufficient to withstand legal scrutiny should it come to that.

I ask you as interested readers, how much of your research would make print if everything had also to be supported by recordings?

Research ethics from the black-and-white era

In recent years, many scholars of India have been harassed in the name of religious nationalism. My own treatment has been tame in comparison to others. The accusations leveled against me were about the representativeness and accuracy of my research, rather than my moral or sexual character. In the context of South Asia and its diasporas, objection to anthropology often builds on the same gendered and nationalistic arguments scholars are critical of, which makes constructive dialogue difficult; neither party is shaken from its conviction. In fact, if anything, the sequence and nature of these encounters only seems to strengthen the original belief or attitude, in a way that simply introducing greater analytical humility will not resolve. In other contexts, objection could be an opportunity to think differently and to engage in new forms of conversation. This was not my experience, and

neither do I think that was the intention of those who produced the arguments against my scholarship. However, on the positive side, the experience has led me to think harder about anthropology's relationship with objection, especially when this implies criticism and conflict.

The question of perspective in research is an old one, perhaps addressed in most forthright terms by Howard Becker (1967) who simply asked: "whose side are we on?" I would rhetorically add to this: how and why? The "how" is intended to flag a methodological caution by drawing attention to the mechanics of taking sides in research and the epistemological implications of doing so. The "why" raises an awareness of the origins and texture of our own pre-theoretical personal politics, which influences, consciously or not, which side we take (what in ethics might be called "ordinary ethical sense"). Such methodological concerns and the influence of personal anthropology have, of course, been discussed extensively within the discipline. Still, I will claim to have been on the side of ethnography, regardless of how incomplete such a statement may be. However, the "whose," "how," and "why" of side-taking takes on different complexion when ethnography is written and made public, as I have shown. Who is on whose side is a different matter entirely when read by plural publics. This is an increasingly significant area of concern and one that remains insufficiently theorized, taught, and represented in ethical and procedural codifications of disciplinary practice. We might perhaps think of this as an ethics of engagement, the consequence of going public and engaging with objections. Indeed, the actual challenge here might be better conceived of as developing a method of openness through which objections could be seen more as contributions and welcomed rather than taken badly or as affront; however, in my experience this was never an option; therefore, the previous suggestion is a possibility but not an injunction.

In the United Kingdom there are few resources or guidelines for such engagement, or

established modes of writing that encourage or actively ask for contributions from others. There is also little institutional support for academics from either universities or research funders when the heat is turned up. At SOAS, the principles of the Research Ethics Code are integrity, honesty, openness, confidentiality, voluntary participation, avoidance of harm, independence and impartiality, and cultural sensitivity. Such principles are softly framed versions of complex areas of law, which have histories, case precedents, and specialized legal practices. It is therefore quite surprisingly straightforward for research ethics matters to move from the university to the legal arena if someone so desires.

These codes guide research undertaken in the name of SOAS and are taught to all postgraduate researchers with the aim of professionalizing and focusing research practice. We also teach with the Ethical Guidelines for Good Research Practice issued by the Association of Social Anthropologists of the United Kingdom and Commonwealth. This document forms a basis for professionalized and disciplinary-specific research ethics. Unlike the general SOAS code, therefore, it is written specifically to anticipate the sorts of issues that might arise during long-term field research when using participant observation. However, these codes are only softly bent toward the discipline of anthropology from their origins in medicine. They are also tinged with what resembles postcolonial guilt.

Despite recent revisions, these codifications seem oddly out of step with global politics and the realities of the research that many anthropologists actually do. On the one hand, if the guidelines were followed loyally, it would be difficult to conduct any research at all. On the other, if the general drift of the guidelines were followed as a guide to methodology, the result would be completely out of kilter with what a small and independent publisher would consider printable. I would also argue, quite generally and in no particular order of importance, and with no disrespect to unnamed authors of the codes of ethics, that the principles outlined assume greater power always to lie with the anthropologist than with

those being researched. They are undergirded by the idea that people and communities are what anthropologists research, rather than institutions, ideas, or horses. They have little to say about intellectual property rights. Perhaps unsurprisingly, but unusually, they place the burden of protection and duty of care solely upon the anthropologist. Any form of informed consent or contracted agreement between researcher and researched makes no demands on the behavior of the researched whatsoever (which makes it a rare form of contract). More significantly, such guidelines rule out the possibility of research that is critical or damaging to the ways of those who primarily or secondarily informed it. Therefore, because of the principle of nonmaleficence, no "studying up" or writing critically about deforesters, polluters, torturers, warmongers, or predatory humanitarians.

These codes are guides that help researchers think through and anticipate problems in research. Both research ethics codes/guides referred to above are focused primarily on the conduct of research but can be sensibly stretched to include the writing as well. However, they say nothing of the subsequent ethics of engagement, which strikes me as outmoded when universities and funding institutions currently place strong emphasis on both the impact and the dissemination of research. Furthermore, it is worth pointing out that although the research ethics code might be seen as regulating, standardizing, and professionalizing research practice, one greater effect is to emolliate the institution, rendering the individual researcher accountable and therefore potentially vulnerable.

The age of objection now necessitates something more than explaining objection as the result of differing epistemologies. As I have suggested, there is good self-interested reason to do so, as much method and writing will not withstand legal scrutiny, which is a mechanism accessible to some objectors. In addition, in the UK context there is a tangle of other regulation and legislation that has a direct relationship to research practice: free speech, the contradictory tugs of the acts determining the freedom of information and the protection of

data, as well as the requirement of many funding bodies for research materials to be placed in public repositories. The amassed consequences of these regulations and requirements are poorly understood and are only slowly finding a way into doctoral training programs, which is a discourtesy to a future generation of anthropologists and could lead to a severe crisis of representation or fines and prison sentences.

As I have said, the ethos of existing research ethics codes is paternalistic and oriented toward geographies which have disappeared. Current guides encourage mindfulness of important issues but will not protect or guide anthropologists in the fields of critical writing or objection. To a great extent, these codes assume anthropologists want to like and get to know those they work amongst; they also assume that those anthropologists work amongst will want to get to know, like, and learn from the researcher, which seems like a form of complacency from the era when things were black and white. How then is the new normative anthropologist to be ethically recoded and positioned as a post-postcolonial being?

Conclusion

The *blitzkreig* worked. The sadhus set in motion a process that restricted what I could publish. Along the way, although no one said it openly, it was obvious to me that for the most part academic institutions are not in a position to match objections or threats from influential and nimble transnational organizations. Having had a few years to consider the objections raised, I am no closer to being persuaded that the perspective they presented to me was sincere. In fact, I find much of what they had to say to resemble what other scholars have characterized as a deliberate and strategic attempt to control what is publicly known about them. Having said that, I joyfully acknowledge that they have pushed my anthropology on. I had been trained in a conservative and careful ethnographic tradition of British Social

Anthropology, and I had paid insufficient attention to thinking about and refining how that writing might be read; nor had I really come to terms with whom or what I was writing for.

Anthropology as a discipline could probably carry on regardless. An objection piloted into the ivory tower is unlikely to bring the whole edifice down. However, this experience gave me the opportunity to see the consequences of the structures of anthropology more clearly than before. I do not think we should tear up the canon and start from scratch. There is, however, a need to develop more robust mechanisms for explaining and defending what the discipline is, what it does, and why this matters. Finally, I think we need to retire the figure of anthropologist-as-Malinowski from our ethical codes. The style of anthropology he represents continues to strongly inform the ways in which the anthropologist is conceived as an ethical being. What if the codes were reconceived using the frames, tropes, and engagements of contemporary practice within anthropology?

There are many figures to choose from, of course, and a combination of figures would probably be better than one, but imagine the consequences of replacing Malinowski with someone like Didier Fassin, for example. Fassin's (2013a) research on policing in Paris might form part of the basis for a more general rethink about the shape and direction of ethical engagement in anthropology. He focuses on the everyday life of police patrols in neighborhoods mostly populated by working-class families from North and Sub-Saharan Africa. Using a method he describes as "non-participant observation," he followed the everyday and mundane activities of patrols, the relationships between police officers and with various publics, against the backdrop of a broader context, specifically the racialization of state discourse on immigration and security. He shows how policing has become an enforcement of the social order. Police have become subject to quantified objectives, which lead them into roles far removed from their own expectations; the result is violent and counterproductive policing.

Significantly, the resulting publications make a strong case for the importance of ethnography in public life. The struggles of power, race, and representation he describes are in time with the world. He navigates the reluctance of institutions to grant "access," and chooses sides carefully. He sides with the ethnography, loyalty to both the police he has known through professional interaction and to those policed through a general concern about social justice. Fassin makes explicit and clear what I think many anthropologists do anyway: "I do not chose one side—that of the youth—but being physically present on the other side— that of the police—I am in a position to observe what happens during the encounters between the two and later relate these findings to the larger picture of state interventions" (2013b: 641). The active focus of his research (police) is not the primary focus of his interest (social justice). This methodological conjuring or displacement is entirely absent from the way ethics codes are currently conceived and research practice commonly articulated.

An ethics rewritten around the structures, politics, and geographies of research informed by ideas drawn from contemporary research practice would gently reorient the discipline. Fassin's research has also gone far beyond the academy and has become part of a national debate in France about the structures of policing and the racialization of public culture. In the process, his writing theorizes the ways in which ethnography can become a public good and a way of creatively and openly exploring the moral sentiments of contentious processes. In this way, using the practice of someone like Fassin as a basis for refinement, the focus of research ethics could be broadened, or bumped forward, to include engagement as part of "normal" practice rather than as a scary and unanticipated epilogue.

Leaving aside the restrictions placed on the material I could include in some sections of the book I eventually wrote on the earthquake, I would like to think that the content was positively influenced by the uneasy experiences recounted here. My firm intention was that a reader would not have to be an anthropologist to understand my purpose. There remain

critical sections oriented by ethnographic authority—but I wrote as myself, made all the more aware of myself by the encounters with sadhus. I have a line, but I try to make it clear how and why I walk that line and not others. There are also views from different perspectives on a range of post-earthquake issues. I have not tidied everything up and have left uncertainty to protrude when that is what ethnographic encounters suggested. Differing views of the disaster became one of the principal motifs, including the perspective the sadhus presented. However, when the book was reviewed by the UK's *Financial Times* the reviewer complained that my refusal to judge was frustrating and the ethnographic examples I used were too "anecdotal" to be conclusive. Ho-hum!

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Notes

1. Extracts of the speech given at the swearing in of Modi as chief minister were posted on Redff.com on December 23, 2002, at 15:24 IST (from PTI) under the title "BJP to replicate Gujarat experience: Venkaiah."

2. The English term "good work" is used in Gujarat for the types of humanitarian intervention discussed here. The term captures the spirit in which many organizations with clear religious and political agendas chose to present their activities.

3. 2004. "In bad faith? British charity and Hindu extremism report summary." London: AZAAZ—South Asia Watch Ltd.

4. Sewa International, Annual Report: April 2012–March 2013.

References

Anderson, Edward. 2010. Swaminarayan Hinduism and the British Gujarati diaspora. M.Phil. thesis, University of Cambridge.

Becker, Howard. 1967. Whose side are we on? *Social Problems* 13(3): 234–247.

Behar, Ruth. 2003. Ethnography and the book that was lost. *Ethnography* 4: 15–39.

Bhatt, Chetan. 2001. *Hindu nationalism: Origins, ideologies, and modern myths*. Oxford: Berg.

Borneman, John, and Abdellah Hammoudi, eds. 2009. *Being there: The fieldwork encounter and the making of truth*. Berkeley: University of California Press.

Bourdieu, Pierre. 2008. *The bachelor's ball: The crisis of society in Béarn*. Translated by Richard Nice. Cambridge: Polity.

Brettell, Caroline B., ed. 1993. *When they read what we write: The politics of ethnography*. Westport: Bergin and Garvey.

Clifford, James, and George E. Marcus, eds. 1986. *Writing culture: The poetics and politics*

of ethnography. Berkeley: University of California Press.

Denham, John. 2005. *Terrorism and community relations: Sixth report of session 2004–05. Volume 3: Oral and additional written evidence*. House of Commons Papers 165-III 2004–05. London: Great Britain Parliament House of Commons Home Affairs Committee, TSO.

Dwyer, Rachel. 1996. Caste, religion, and sect in Gujarat. In Roger Ballard, ed., *Desh pardesh: The South Asian presence in Britain*, pp. 165–190. New Delhi: B. R. Publishing.

Elie, Serge D. 2012. The production of social science knowledge beyond Occidentalism: The quest for a post-exotic anthropology. *Third World Quarterly* 33(7): 1211–1229.

Fassin, Didier. 2012. *Humanitarian reason: A moral history of the present*. Berkeley: University of California Press.

Fassin, Didier. 2013a. *Enforcing order: An ethnography of urban policing*. Cambridge: Polity.

Fassin, Didier. 2013b. Why ethnography matters: On anthropology and its publics. *Cultural Anthropology* 28(4): 621–646.

Ferguson, James. 1990. *Anti-politics machine: Development, depoliticization, and bureaucratic power in Lesotho*. Cambridge: Cambridge University Press.

Foucault, Michel. 1972. *The archaeology of knowledge, and the discourse on language*. Translated by A. Sheridan Smith. New York: Pantheon.

Hansen, Thomas Blom. 1999. *The saffron wave: Democracy and Hindu nationalism in modern India*. Princeton: Princeton University Press.

Hastrup, Kirsten. 2004. Getting it right: Knowledge and evidence in anthropology. *Anthropological Theory* 4(4): 455–472.

Hull, Matthew. 2012. *Government of paper: The materiality of bureaucracy in modern*

Pakistan. Berkeley: University of California Press.

Kim, Hanna. 2009. Public engagement and personal desires: BAPS Swaminarayan temples and their contribution to the discourses on religion. *International Journal of Hindu Studies* 13(3): 357–390.

Kim, Hanna. 2010. The Swaminarayan Movement and religious subjectivity. In Edward Simpson and Aparna Kapadia, eds., *The idea of Gujarat: History, ethnography, and text*, pp. 207–228. New Delhi: Orient Blackswan.

Kim, Hanna. 2012. The BAPS Swaminarayan temple organization and its publics. In John Zavos, Pralay Kanungo, Deepa S. Reddy, Mayer Warrier, and Raymond Williams, eds., *Public Hinduisms*, pp. 417–439. New Delhi: Sage.

Mathur, Shubh. 2008. *The everyday life of Hindu nationalism: An ethnographic account*. New Delhi: Three Essays Collective.

McKean, Lise. 1996. *Divine enterprise: Gurus and the Hindu Nationalist Movement*. Chicago: University of Chicago Press.

Mosse, David. 2005. *Cultivating development: An ethnography of aid policy and practice*. London: Pluto.

Mosse, David. 2006. Anti-social anthropology? Objectivity, objection, and the ethnography of public policy and professional communities. *Journal of the Royal Anthropological Institute* 12(4): 935–956.

Mukta, Parita. 2000. The public face of Hindu nationalism. *Ethnic and Racial Studies* 23(3): 442–466.

Nussbaum, Martha Craven. 2007. *The clash within democracy: Religious violence and India's future*. Cambridge: Belknap.

Savarkar, Vinayak Damodar. 1938. *Hindutva*. New Delhi: Central Hindu Yuvak Sabha.

Scheper-Hughes, Nancy. 2001. *Saints, scholars, and schizophrenics: Mental illness in rural*

Ireland. 20th anniversary edition, updated and expanded. Los Angeles: University of California Press.

Scott, James. 1999. *Seeing like a state: How certain schemes to improve the human condition have failed*. New Haven: Yale University Press.

Shah, Ghanshyam. 2002. Caste, Hindutva, and hideousness. *Economic and Political Weekly* 37(15): 1391–1393.

Shani, Ornit. 2007. *Communalism, caste, and Hindu nationalism: The violence in Gujarat*. Cambridge: Cambridge University Press.

Shukla, Sandhya. 1997. Building diaspora and nation: The 1991 "Cultural Festival of India." *Cultural Studies* 11(2): 296–315.

Simpson, Edward. 2004. "Hindutva" as a rural planning paradigm in post-earthquake Gujarat. In John Zavos, Andrew Wyatt, and Vernon Hewitt, eds., *The politics of cultural mobilization in India*, pp. 136–165. New Delhi: Oxford University Press.

Simpson, Edward. 2008. Was there discrimination in the distribution of resources after the earthquake in Gujarat? Imagination, epistemology, and the state in western India. NGPA Research Paper 23. London: London School of Economics and Political Science.

Simpson, Edward. 2013. *The political biography of an earthquake: Aftermath and amnesia in Gujarat, India*. London: Hurst.

Simpson, Edward, and Aparna Kapadia, eds. 2010. The idea of Gujarat: History, ethnography, and text. New Delhi: Orient Blackswan.

Upadhyaya, Deendayal. 1965. *Integral humanism*. New Delhi: Bharatiya Jana Sangh.

Williams, Raymond Brady. 2001. *An introduction to Swaminarayan Hinduism*. Cambridge: Cambridge University Press.

Zavos, John, Andrew Wyatt, and Vernon Hewitt, eds. 2004. *Politics of cultural mobilization*

in India. New Delhi: Oxford University Press.