Giving Up the Field
When Friendships Outweigh Scholarship

Stephen Lyon

Earning the trust of people in a community, and particularly in specific families, is time-consuming and imposes obligations on all parties. In ethnographic research based on long-term participant observation over several years, the relationships that develop invariably veer into a host of different forms. Not all of them become close or affectionate, but as with our natal family relationships, there are levels of intimacy that affect people across the entire network. I carried out field research in a Punjabi village in 1998 and have continued to visit this community on a semi-regular basis ever since. It took years to work through locals' expectations of how guests must be treated and how unrelated men could interact with local women. After more than a decade, I felt rewarded with a 'goldmine' of anthropological access. I worked with a handful of households in a community that was ostensibly very closed to outsiders but who had nonetheless generously opened their doors to me. It was at this point, however, that I began to realise there was a new barrier to my research ambitions. Although I now had privileged access to several households, documenting and reporting on their private details was tantamount to a betrayal of the people I had come to love and think of as my own family. On the one hand, I was in a much better position to understand the ethnographic phenomena that are the bread and butter of anthropology, but on the other hand, publishing ethnographic accounts from my 'primary' field site could hurt people. Like Martin Sökefeld, I believe I have come to prioritise the research that might be the most beneficial for the people I have come to care about, rather than ruthlessly, in

my view, pursuing the research questions that might be the most compelling to me personally. Ultimately, I am not convinced that my research contribution will be less significant, but I am aware there is knowledge that will never be shared with wider audiences – and that is not something I would have predicted would be part of my scholarly legacy. To be clear, it is unlikely that the people with whom I have worked will ever read my publications in particularly large numbers, but details about human weakness and mistakes can be used to undermine the reputations of individuals and households. To disguise identities whilst divulging private information would invite speculation that could wind up being directed at anyone. The last thing I want is for my work to become fodder for baseless accusations that could damage someone's standing in their community.

Best laid plans...

Anthropological research rightly encompasses scientific approaches, and it should be empirical, rigorous and, above all else, honest. I have taught multiple cohorts of students to think carefully about their research design before they commence fieldwork. I make them read methodological discussions in the discipline from the days of armchair anthropology, the early field working anthropologists, the challengers to paradigmatic unity following the Second World War and, of course, the heady days of Marxist and Feminist anthropology in the 1970s and the nod to literary approaches of the 1980s and 1990s. The fragmentation of methods has mirrored the breaking up of theoretical approaches in the discipline throughout this time. Some of this fragmentation has unquestionably been positive for the discipline, but it has resulted in an academic field that often appears in disarray and as if it cannot agree on its own boundaries or priorities. My aim in this chapter is not to lament the loss of an anthropological paradigm but rather to offer a brief description of my personal journey, from intellectual certainty to the shifting sands of a scholar who has had to prioritise people's wellbeing over some research aspirations.

My journey maps on to some of the decisions that Martin Sökefeld has made in his career. Like me, Martin's anthropological studies were conducted in the shadow of *Writing Cultures* (Clifford and Marcus 1986). We were both trained in Europe, but the differences between Anglophone UK traditions and those of Germany may have affected some of our decisions.

My doctoral fieldwork began in early 1998, when I arrived in a Punjabi village to understand local farming practices. I had a clear agenda that included participating directly in farmers' tasks. I wanted to till soil, join in with the routine maintenance activities of farm equipment as far as possible, sow seeds, engage in weeding and harvesting activities and then tag along when the crops went to market. My aim was to learn local dialects and hang out with the people who actually worked on the land. I had little interest in the powerful landowners, but of course, I understood that I would never be granted permission to carry out any research without their consent and support. So, my introduction to the 'village' was actually to the landowning households. My first visit was in the company of extension workers and agricultural scientists working for an international NGO and the national Agricultural Research Center. As a result of the specific entrée into the village, I wound up interacting with the peasant farmers associated with the household that first welcomed me.

I had lived in Pakistan before, so I knew the significance of hospitality; as a guest, I would not immediately be allowed to engage in work that made me tired and got my hands dirty, but I was confident that I would eventually work past the constraints of hospitality and become someone more useful to my new neighbours. Following those initial visits to the village in early 1998, I returned to the UK and created a detailed research plan to cover not only twelve months of fieldwork, but also the writing up and publishing parts of the process that followed. I therefore arrived in the autumn of 1998, fully armed with a detailed, week-by-week research plan to learn as much as I could about local farming practices, social organisation and economic patterns.

It will come as no surprise that very little of my elaborated research plan survived beyond the first few weeks. I continued to try to force bits

of the plan back into my daily activities, but the resilience and determination of the local community proved far more effective than my will. In the end, I had to write a very different doctoral thesis, and my research questions were completely derailed to match the observable priorities of the landowning households. The capacity, and indeed the necessity, to adapt to local priorities is perhaps one of the more common features of ethnographic fieldwork.

The reality of my straitjacket came home to me in a number of visceral ways throughout that doctoral research period. For instance, on one occasion, I was hanging out with some local peasant farmers who were cutting fodder with a hand scythe. I squatted down and used the hand scythe to imitate them. I was doing an okay job, though clearly not as efficiently or as elegantly as the people around me. They found this very amusing and were happy to let me carry on for about 10 minutes. As I started to really get into my stride, a car drove by on the nearby road, about 100 metres away. On seeing the car, the men immediately jumped into action and took the scythe away from me, manhandled me onto the *charpai* and shoved a cup of tea into my hand. They frantically told me to relax and look like I was having fun.

I was a little taken aback and asked why I could not continue cutting fodder. They looked nervous and explained that the car belonged to people from a neighbouring village. They then went on to say that if those people reported to the landlords in our village that the European guest was being made to do manual labour, they would all be in a lot of trouble. I argued with them and said that it was my job – I was *supposed* to be doing manual labour. I told them, not for the first time, that I did not come from a rich family; part of my ancestry was Appalachian hillbillies who knew all about hard labour and surviving off the land. It will come as no surprise, however, that they were not persuaded.

I raised the matter with the landowners, who were equally unmoved. They reminded me that I was their guest, and they would never ask their guests to do manual labour. In one exchange that I now find amusing but which was frustrating at the time, I shouted at my best friend that he was interfering with my work. He shouted back that it was *his* village:

"The happiness of everyone in my village is *my* responsibility!" he shouted at me.

"You don't know what will make me happy!" I shouted back.

He shook his finger at me and shouted, "Trust me, Dr Sahib, you don't know what will make you happy! *I* know better than you what will make you happy in *my* village!"

I learned so much from this man, but much of it was difficult and even sometimes infuriating. After the fact, though, I can look back with eternal gratitude. He did not let me do what I wanted to do, but he *did* help me understand his village and his culture *far*, *far* better than my carefully crafted research plan ever could have done.

Narratives and reputations...

I therefore adapted my research plans by compromising my objective scientific aspirations out of respect for the constraints that people in the local area felt were non-negotiable. I had set out to produce a comprehensive account of indigenous, or at least local, farming practices that included etic metrics of crops, soil composition, fertiliser and pesticide systems along with an elaborated model of decision-making among primary food producers. In the end, I developed an argument around the narratives of conflict, resolution and prestige. At that point, it was relatively simple for me to publish in ways that did not compromise anyone's reputation, because the focus was on publicly known and shared narratives. I returned to the UK, completed my doctoral fieldwork, wrote up the thesis, turned some chapters into a few journal articles and published a version of it as a monograph (see Lyon 2004). All in all, I did not, at that point, realise just how much compromising I would wind up doing as my relationships became closer and people started sharing ever more 'secret' stories of their lives and their relationships. I also did not appreciate at the time just how comfortable I would be with *those* compromises.

After almost nine years from the first time, I visited the village I considered my 'primary' field site, and I was deemed trustworthy enough

to meet the ladies of the landlord families. This was not a pre-planned event, or at least if it were, the planning did not include me. I was in the village for a few weeks and somewhere in the middle. I was in my little room, alone after 10.00 at night, when someone came knocking on the door. He urged me to follow him, as one of the Maliks was requesting my company. I was a little tired and half felt like making an excuse but did not. As so often happens, those occasions when I overcome my fatigue or timidity turn out to be some of the most profound opportunities.

I followed the young man through the maze of little passageways through the village and went through a door I honestly had not even noticed before. It was small and I had to crouch down to get through. On the other side, it opened into a lovely spacious green garden. Sitting in chairs in a semi-circle were about a dozen women. They were all smiling and beckoning me to come sit down in an empty chair on the end of the semi-circle. I was a bit speechless and did not know exactly what to say or how to behave. I was acutely aware that 90% of the Urdu I had spoken in my life had been to men and largely about things that men do. I was not entirely confident that I had the right vocabulary to speak to women in Urdu.

We had a lovely dinner during which they asked about my children, my wife, my university and what I thought of the food they had been providing over the years and what I thought of the village. At the end of this world-changing evening, as I was walking back to my little room, one of the young landlords explained that they had known about me for nine years and wanted to meet me in person. It could have happened sooner, he explained, if the ladies had asked for the meeting. I will never know if that is true. It could be that the ladies had asked for the meeting many times before, but the men did not trust me, or that it was indeed an accurate account of why I was privileged on that occasion.

There was a sea change from that evening. I was not suddenly given unfettered access to family areas, but people were more relaxed about gender politics and more forthcoming in explaining women's involvement in household decision-making. When people called me 'Brother', it somehow felt more sincere than before. I was trusted enough to interact, however superficially, with the women of the landowning households.

What I had not appreciated was the extent to which this would render much of my 'research' on the landowning families problematic, if not outright impossible. This was brought home to me in a visceral way in two incidents that occurred a few years after I was allowed to cross the hitherto strict gender segregation barrier, referred to as *purdah*.

I had a small grant to study *khawjasara*, or the South Asian Third Gender. I was working with a colleague in the UK who was studying trans* communities in the UK and the US. At first, everything went smoothly. I explained my research questions to my village 'family', and they really expedited progress. One of their relatives living in the city had a *khwajasara* domestic worker who was apparently happy to sit down with me and talk about her life.

Trouble began when I explained that I wanted to visit a *khwajasara* household and spend time with the Guru and hangout observing the ordinary life of the third gender in Pakistan. My village brother was shocked. A conversation with a trusted *khwajasara* behind closed doors was one thing, but to publicly go to a *khwajasara* household and spend significant amounts of time with people from that community would do irreparable damage to those people closest to me.

I could not understand how this research project could possibly harm people who were not involved in it, beyond an initial introduction. Then my brother patiently explained that I was not at liberty to do whatever I wanted anymore – I was part of his family. Anything I did could impact the *izzat*, or honourable reputation of my village family. As he talked me through his rationale, I had visions of that evening not long past, when the women of the family had put their collective foot down and insisted I *was* a real brother who should be allowed to interact with his sisters and aunties. There was a price to pay, and this was part of that price. The reputation of that family mattered enormously to me – I did not want to harm them and have tried hard never to do so since. But once I was so intimately drawn in, I no longer had the authority to decide what protecting their reputation meant. Their own attitudes and values had taken priority over my reasoning, and so they needed to monitor and police my behaviour – just as if I had been born into one of the households. My

'guest' status had been revoked, and what I did mattered in a way that it had not before that fateful integration.

The example that finally ended my 'researcher' relationship with the village occurred several years after I abandoned my plans to study *khwajasara* properly. I had been given the opportunity to interview one of the oldest women in the village. She was happy for me to record these interviews, and I was ecstatic at the opportunity to really be allowed to have more in-depth conversations with a woman about life in the village. I was curious about how women influenced decisions, how involved they were in land disputes and a host of other subjects that could only be addressed by speaking directly to women.

We had several fantastic conversations, and this wonderful older woman dropped several bombshells of revelation on me. She had such a rich knowledge of her village and the landowning households, and she told me stories that contradicted some of the tales the men had told me over the years. She quashed any notion that women were only interested in food and children – an assertion that had been made by more than one man over the years.

Then, her grandson came to me and told me not to publish anything she told me. I was surprised because she seemed so relaxed about it all and wanted her story to be recorded and known. I have had many opportunities to meet her since my conversation with her grandson, and occasionally I ask her about these restrictions. The first time I asked, she laughed and said, very easily, that the men in her family would not like some of those stories to be known outside their own intimate circle. I was, she said, her son, and so I needed to know these variations in the family history, but they were not for people outside the family.

The privilege conferred on me by that family is immeasurable. It's humbling and it's huge. It's also a substantial barrier to satisfying one of my professional duties as a scholar. I always tell my students that research that is not published is incomplete. I believe that wholeheartedly, but I also believe that the recordings I have produced of this lovely woman are invaluable and should be preserved forever, but *never* shared. Her words of wisdom are borne from a lifetime of involvement and observation of the challenges of being part of a farming family in a rainfed

part of Punjab. It is not an easy life for anyone, but it was unimaginably tough for much of her life. I benefit so much from being allowed to listen to her that to ask to publish as well seems not only greedy, but also immoral. My 'job' as a scholar of that village is incomplete and will remain so. I changed the focus of my research questions to new parts of Pakistan and different households. I continue to go by the village to catch up on gossip, as any good family member should, but I hardly ever take notes anymore and have no intention of publishing the 'data' I may generate. Anything I write about that village family in the future will be historical and carefully curated to ensure that their reputation is not impacted.

Following what I consider my familial adoption, I effectively ceased to 'do research' on my original communities. Instead, I used the conceptual understanding I had gained from those farming communities to understand the wider electoral cultures of Pakistan (Lyon and Mughal 2016; Lyon 2019a; 2019c; Lyon and Hassan 2022). I also shifted my attention to the political and environmental contexts in which my earlier ethnography existed (Lyon 2019d; 2019b; Lyon and Mughal 2019). Finally, I also began writing for non-academic audiences about some of the challenges facing rural communities in Punjab (see for example Lyon 2022).

The hand of Martin

So how does this relate to Martin Sökefeld's work? I argue that it aligns very closely to the humanity he has always brought to the field. He is fortunate enough to have worked with some households that do not appear to have exercised such strict *purdah*, though he has encountered some comparable gender-segregated households in Gilgit as well. Theoretically, one can see this in his careful analyses of the concept, function and implications of 'identity' (Sokefeld 1999; Sökefeld 2001; 2008). At the analytical level, he thoughtfully deconstructs both essentialising and anti-essentialising approaches in anthropology (and, more broadly, in social sciences and philosophy). It seems to me that this is driven, in part, by a genuine understanding of the political implications of such

a concept for marginalised communities, both in Pakistan (Gilgit) and in Germany (Alevi Muslims). As he frames his theoretical arguments, he situates them within political contexts in which Alevi communities in Germany, for example, are subject to discrimination based on an essentialised attribution that renders them vulnerable within both a wider Germany and Turkey as a whole, as well as within what he effectively demonstrates is a fragmented German-Turkish population.

Martin's theoretical positioning is robust and couched in philosophical language that avoids the first-person, anecdotal style that I have adopted here. This is both laudable and necessary to ensure academic credibility, but I believe there can be no doubt that resting at the heart of his analysis is an awareness of the political consequences of reductionist and easy arguments about identity.

Martin is nothing if not eclectic in some of his methodological and empirical approaches. Knowing the sensitivities of the political situation in the northern areas of Pakistan, which remain formally disputed territories with India, regardless of the political realities on the ground, he has delved into the historical record of the region to make sense of the politics of rumours (Sökefeld 2002). Through a fascinating focus on the political rumours evident throughout the northern frontier of British India (Gilgit, but called Yaghestan at the time), he teases out their significance in relation to local politics between the British and local populations, as well as those circulating about World War I. This use of historical material allows him the flexibility to comment on the interconnectedness of global political conflicts that find their way into local political discourse. He, of course, has not shied away from analysing contemporary political conflicts, but the consequences of missing the mark when discussing Gilgit-Baltistan must not be underestimated. He treads carefully because he knows that to do otherwise may compromise the wellbeing of those he clearly cares about.

Lastly, it would be remiss if I did not highlight one of his more lighthearted contributions to the discipline. Martin has made a name for himself as an anthropologist who takes the ubiquitous truck art of Pakistan seriously (Sökefeld 2000). In this research, Martin examines an art form largely overlooked by 'connoisseurs'. He makes important points

about the aesthetic and techniques that are to be rightly admired, but through his deft weaving of humour he promotes and validates a sector that suffers from low prestige in the wider population (less so now, in part, thanks to him). In short, he uses his analytical credibility to do what the best anthropologists have always done, namely listen carefully to the ignored and the neglected and treat their activities and narratives with as much respect as their more elite and powerful neighbours.

The consequences and politics of compromise

So, what does it matter if people choose to carefully consider the consequences of our writing? On the one hand, it is incumbent upon members of anthropological scholarly associations, all of which place a premium on the well-being and security of the people with whom anthropologists work (for the UK, see *Association of Social Anthropologists of the UK and the Commonwealth (ASA) Ethical Guidelines for Good Research Practice* 2011). Increasingly, research funding institutions similarly demand robust ethical reflection as part of the design process (again, for the UK, see UKRI 2023 for guidance from one of the most important funders of anthropological research). However, neither scholarly associations nor funders quite capture the real driver of compromise for field-working ethnographers who return to the same sites over multiple years. What drives Martin and me, I believe, is a deep and profound respect for the people with whom we work. Martin's decades-long commitment to both his Gilgiti and Alevi friends and research participants is a strength, in that it has obviously given him access to confidential aspects of those communities. It is no doubt also a constraint. There will be actions and narratives that will never make their way on to the pages of his astute and erudite scholarly publications. There are no doubt secrets he will take with him to the grave. For the most part, these are probably relatively banal in the grand scheme of things, but they might embarrass or compromise an individual or a household within their own social worlds.

I can live with the compromises that I have made, and I assume that Martin is similarly at ease with the choices he has made. It strikes me

that the depth of trust and ease in the relationships that emerge from such considerations allows for producing a powerful and effective empirical dataset. While some of that may never be explicitly shared with 'outsiders', it nevertheless serves to strengthen and clarify the anthropologist's understanding of other people's perspectives and experiences. We can never truly walk in the shoes of another, but the process of forming sincere and open relationships ensures we can begin to understand better some salient aspects of *other* people's lives. These relationships introduce a responsibility that goes beyond scholarship. Our scholarly associations instruct us to do no harm, but harm is itself a culturally contextualised concept. So, part of what we must learn in the field is understanding what the people we work with consider harmful. It makes for some messy ethical reviews, but ultimately it has the potential to generate robust explanations and descriptions that are both moral and useful.

Anthropology is littered with the tragedies of anthropologists who have chosen to publish regardless of the potential harm that may arise from revelations. Some of the horror stories may well be blown out of proportion, and some of the most famous, or infamous, cases inspire vigorous defence from both the original ethnographers as well as their allies. Some of these celebrated cases have produced ethnographic material that I have enjoyed and benefited from, but it becomes uncomfortable reading when one realises that the subjects of those studies may have experienced physical, material or social harm as a result. I will not list them here, because I also owe a debt to my own anthropological 'community' that seems to descend into distracting self-sabotage all too frequently. Rather than list problematic ethnographies that will trigger endless arguments, I hope to focus on the humanity that *should* drive our research and our relationships. The price for such humanity, though, may seem high at different points in a career. It's relatively easy for me to step away from my primary field site after more than a decade of research and publishing, but had I felt compelled to take such a decision after the first year, then the cost would clearly have been considerably more painful. Regardless of when and how we decide to navigate these culturally and socially specific ethics, we must always strive for honest and credible accounts of the people we work with – we owe them and our

discipline no less – but to do so with cavalier disregard for the feelings and context of the people we work with is profoundly disappointing (at best).

It is therefore with some pride that I put myself in the same category as Martin Sökefeld, as an ethnographer and an anthropologist who has sought always to consider the people who have been generous enough to welcome me into their communities. *Their* sensibilities and *their* ethics must weigh as deeply and consequently on our scholarly output as those of our home institutions and our broader disciplinary communities.

References

Association of Social Anthropologists of the UK and the Commonwealth (ASA) (2011): *Ethical Guidelines for Good Research Practice.* http://www.theasa.org/ethics.shtml.

Clifford, James/George E. Marcus (eds) (1986): *Writing Culture: The Poetics and Politics of Ethnography a School of American Research Advanced Seminar,* Berkeley: University of California press.

Lyon, Stephen M. (2004): *An Anthropological Analysis of Local Politics and Patronage in a Pakistani Village,* Lampeter: Edwin Mellen Press. http://www.mellenpress.com/mellenpress.cfm?bookid=5732&pc=9.

Lyon, Stephen M. (2019a): "Challenging the Landed Elite in Contemporary Pakistani Politics." In: *Journal of Legal Anthropology* 3/1, pp. 41–61. https://doi.org/10.3167/jla.2019.030103.

Lyon, Stephen M. (2019b): "Patronage and Clientelism." In: Paul Atkinson/Sara Delamont/Alexandru Cernat/Joseph W. Sakshaug/Richard A. Williams (eds.), London: SAGE Publications Ltd. https://doi.org/10.4135/9781526421036814729.

Lyon, Stephen M. (2019c): *Political Kinship in Pakistan: Descent, Marriage and Government Stability,* Lanham, MD: Lexington Books. https://rowman.com/ISBN/9781498582186/Political-Kinship-in-Pakistan-Descent-Marriage-and-Government-Stability.

Lyon, Stephen M. (2019d): "Conflict and Security." In: *The International Encyclopedia of Anthropology.* https://doi.org/10.1002/9781118924396.wbiea2011.

Lyon, Stephen M. (2022): "Internally Displaced People: A Social-Cultural-Technical Challenge." In: *Geo News*, November 10, Opinion. https://www.geo.tv/latest/451473-internally-displaced-people-a-social-cultural-technical-challenge.

Lyon, Stephen M./Sohaila Ashraf Hassan (2022): "Kinship, Factions and Survival in Pakistani Politics." In: *Soziale Systeme* 25/1, pp. 145–64. https://doi.org/10.1515/sosys-2020-0006.

Lyon, Stephen M/Muhammad Aurang Zeb Mughal (2016): "Ties That Bind: Marital Networks and Politics in Punjab, Pakistan." In: *Structure and Dynamics: EJournal of Anthropological and Related Sciences* 9/2, pp. 110–22.

Lyon, Stephen M./Muhammad Aurang Zeb Mughal (2019): "Cultural Models of Nature and Divinity in a Rain Fed Farming Village of Punjab, Pakistan." In: Giovanni Bennardo (ed.), *Cultural Models of Nature Primary Food Producers and Climate Change*, pp. 141–64, Oxford: Routledge. https://www.routledge.com/Cultural-Models-of-Nature-Primary-Food-Producers-and-Climate-Change-1st/Bennardo/p/book/9780815356585.

Sökefeld, Martin (1999): "Debating Self, Identity, and Culture in Anthropology." In: *Current Anthropology* 40/4, pp. 417–48. https://doi.org/10.1086/200042.

Sökefeld, Martin (2000): "The Road Is Ours!: Pakistani Truck Art; Photoessay." https://doi.org/10.5282/UBM/EPUB.69425.

Sökefeld, Martin (2001): "Reconsidering Identity." In: *Anthropos* 96, pp. 527–44.

Sökefeld, Martin (2002): "Rumours and Politics on the Northern Frontier: The British, Pakhtun Wali and Yaghestan." In: *Modern Asian Studies* 36/2, pp. 299–340. DOI: 10.1017/S0026749X02002020.

Sökefeld, Martin (2008): *Struggling for Recognition: The Alevi Movement in Germany and in Transnational Space,* New York: Berghahn Books.

UKRI (2023): *Research Ethics Guidance.* https://www.ukri.org/councils/esrc/guidance-for-applicants/research-ethics-guidance/.