MAT Medicine Anthropology Theory

FIELD NOTES

Of Ethnographic (Mis)Translations on a Ward

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Abstract

In this Field Note, I take the opportunity to reflect on some of the concrete dilemmas that I was faced with in trying to negotiate, secure and maintain access to my field-site. These reflections derive from my engagement with infectious diseases physicians, at a renowned corporate tertiary care hospital in Southern India, who are working towards mitigating antibiotic/antimicrobial resistance. By drawing on the difficulties of felicitously translating my concerns, as an ethnographer, to the epistemological universe that animated (but did not wholly determine) my site of investigation allows me to think through what might or might not emerge as strategically useful in the varied loci that anthropologists are increasingly engaged with.

Keywords

Hospital Ethnography, Antibiotic Resistance, Infectious Diseases, Studying Up.

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Getting there: ‘Social scientists talk in paragraphs . . . ’

‘Social scientists talk in paragraphs, pure scientists talk in values, and doctors are somewhere in the middle,’ Dr Z ventures. He nods and smiles in response to my somewhat nervously offered explanation of what an anthropologist of (bio-)science and medicine does.

I am sitting in the tiny infection control room with Dr Z and two medical residents, chatting during a short break between ward rounds, doing my best to explain what ethnography entails. I laugh at Dr Z’s admittedly cogent summing up of our respective disciplinary inclinations. His humorous and perhaps facile-sounding remark, however, evinces not only the different epistemological worlds that we occupy but also indexes the perceived hierarchy between our disciplines. In this Field Note, I think through the disciplinary (mis)translations that animated—challenging, impeding and at other times furthering—my 14-month-long stint of ethnographic fieldwork in varied and surprising ways. I take the opportunity here to consider how, during the course of my engagement as an anthropologist with infectious diseases (ID) doctors in a large corporate tertiary care hospital in Southern India, my access to the field was challenged by our distinct, if not always discordant, epistemological worlds.

I arrived at this sprawling hospital on a stuffy April morning in 2022. On my first day, I nurse my trepidation and make my way to the infection control room, located at the end of a corridor on the second floor. The considerable drop in temperature inside the building does little to quieten my nerves; hospitals tend to make me uneasy and the imminent meeting with my primary interlocutor to go over the details of my proposed research adds to my nervousness. Upon reaching the room I am invited inside by the infection control nurse. I introduce myself briefly to her and we sit and chat there waiting for Dr Z to finish his morning ward rounds. I am here, after several months of exchanges with Dr Z and the ethics committee (EC) over emails, to conduct ethnographic observations on how the dwindling efficacy of extant antibiotics—an imminent/ongoing global health crisis referred to as antibiotic/antimicrobial resistance (ABR/AMR)—is being negotiated within the clinical encounter in India.

The doctor arrives twenty minutes later with two medical residents in tow. He greets me with a loud and spirited ‘hello’, and his friendly demeanour puts me at ease. He hands me and the two residents three mugs and insists that we all go downstairs for coffee. I exchange a few pleasantries with the two residents and then we all saunter downstairs to the coffee machine. On our way downstairs Dr Z apologises for how long it was taking for the project to get the approval of the EC

and management. During the course of the conversation he remarks that ABR/AMR is a 'multi-sectoral problem' that requires a collaborative approach between different disciplines in order to be addressed fruitfully. His description of ABR/AMR as a multi-sectoral issue is a further salve to my nervousness, as it suggests my presence in the hospital will be more amenable to the doctors and the administration.

However, Dr Z also reveals that the still-pending green light from the EC is conditional on the creation of a memorandum of understanding (MoU) with my parent institution, and submission of a pro-forma document with a spreadsheet outlining the details of the data I wished to collect. The EC requires me to list the variables my study would identify prior to its conduct and how I wish to hypothetically relate these variables. The spreadsheet would detail these variables and tabulate their hypothesised relationships. Hearing this makes me a little nervous again since my intention is to conduct ethnographic observations at the hospital.

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Two weeks prior to my arrival in the city I had had a phone call with Dr Z in which I had tried to explain the nature of ethnography. I fumbled while trying to convince him that ethnography was the most suitable method for the research I wanted to conduct. He hadn't seemed to register much of what I had said over the phone and, by way of response, had offered instead that he understood as a social scientist I wanted to conduct KAP surveys. Knowledge, Attitude and Practice surveys are common quantitative surveys used in health research to study barriers to behaviour change. Medical practitioners often presume that social scientists are primarily concerned with KAP surveys.[1]

The point of departure for my research, however, is a grouse shared by social scientists of different shades against isolating individual behaviours as the primary target and the locus of change in policy concerns around ABR/AMR. I wanted to understand the broader gamut of social factors that articulate prescribers and consumers' relationships with antibiotics, and how ID doctors sought to (re)configure these relationships at a time marked by the crisis of their dwindling efficacy. My phone call with Dr Z had been the first indication of a poor translation of my concerns as an anthropologist. In the infection control room I tried my best again to convince Dr Z that what I intended to do did not translate well into KAP surveys. I told him that I had written at length about the methodological and theoretical stakes of my project in the proposal I had sent to him. He chortled and responded that he hadn't read my proposal in its entirety since it was too long.

[1] See Chandler 2018 on the pitfalls of KAP surveys and on ethnography as a methodological alternative.

According to what he had gleaned from my 'lengthy' proposal was, however, that my methods, as an anthropologist, were 'descriptive' and 'subjective', whereas the language of the EC was 'objective', and what we needed to do was to figure out a was of translating my concerns into their language. I attempted to communicate to him that my intentions, as an anthropologist interested in ethnographic observations, were difficult to felicitously translate into concrete categories which demanded the pre-(re)cognition of dependent, independent and control variables. I explained further that ethnography is often motivated by an openness (Spyer 2010) to the 'field'—however articulated—and to its guiding pulse and direction. This sentiment, however, did not make much of an impression on the doctor.

Our conversation ended with Dr Z reassuring me that my presence, as a social scientist, would be of value to them but, until we worked out the 'exact' details of my research project, I needed to write a letter to the director of medical services requesting an initial 'observership', which would allow me to come to the hospital as an 'observer', while I waited for a formal approval from the EC. Although I knew *exactly* what I wanted to do, my unwillingness to communicate my concerns in the language of independent, dependent and control variables failed to perform this certainty. I found myself in a situation in which my primary interlocutor was enthusiastic about my presence in the hospital but his idea of what an anthropologist should do was not in concordance with my intentions.

The more recent turn to science and (bio-)medicine in anthropology has directed an ethnographic eye on laboratories and hospitals—principal institutions of science and medicine respectively. Reflecting on these novel sites of engagement, and considering the varied issues of access and epistemic challenges that engaging with the sciences and (bio-)medicine bring to the fore, scholars have discussed the struggle between maintaining a critical distance and of being ethnographically complicit with our often more powerful interlocutors. Here, the challenges of what Laura Nader ([1969] 1974) dubbed 'studying up' were evident. By encouraging anthropologists to 'study up' Nader had wanted the field to direct its attention to the 'powerful' and not just the downtrodden—reversing the power equation usually encountered in the anthropological field and complicating the ethics of our practice.

In my case, the issues of access and the epistemological challenges of studying up were layered and interlaced. Like any institution, my access to the space depended on being granted permissions by various gatekeepers. The hospital was a teaching institution as well, and it had provisions in place for people to conduct research. However, what compounded the problem in my case was that the EC, conducting its activities in a medical setting that was primed for clinical trials and biomedical research, had no ready template for ethnographic research. This meant

that the bureaucratic hurdles—in terms of gaining approval from the EC—were deeply entangled with epistemological issues concerning our different understandings of what constituted data, how I was to go about collecting it, and how we appraised the division between objective and subjective modes of knowing in our respective disciplines, as would become clearer through the course of my fieldwork. The objectivity that Dr Z, and the EC, expected needed to be articulated through pre-(re)cognised categories that were explicit and delimited. Since the contours of my project were appraised as being more nebulous, according to these demands, it seemed to both lack rigour and engender suspicion. The demand to produce these categories to make my research legible to my interlocutors made the hierarchy of our disciplines evident—a classic symptom of studying up. My inability to do so engendered feelings of confusion and anxiety in me. It was not just the (in)congruity of the translation that concerned me but also what it would mean in terms of the practices I would merit having access to.

Being There: Collecting ‘data’/writing stories?

I was initially granted a one-month-long observership which allowed me to follow the ID physicians and observe them conduct their daily labour. During this month I was not permitted to collect what the EC and the hospital establishment at large considered to be ‘data’. However, my interlocutors had no issue with me taking down ethnographic notes and making observations during this period. This month proved to be a blessing in disguise. My liminal position in the hospital helped deflect any suspicions my interlocutors might have harboured towards me while giving me enough time to build a rapport with the entire ID team. Learning the language is imperative for gaining credence during fieldwork and establishing a firm relationship with one’s interlocutors. It was no different in my case. Learning medical speak was necessary for me to gain the trust of my interlocutors. It was one of the most integral ways of negotiating my access to the field and helped me navigate the interdisciplinary challenges that the field threw up. Over the course of the month I participated in tutorial sessions, read papers with the residents and registrars, and attended various talks and discussions—all doubling as strategic ways to get better acquainted with medical speak as well as opportunities to cultivate rapport with my interlocutors. During the ward rounds Dr Z would often question me playfully to gauge my knowledge of the policies and the involvement of the medical community in mitigating ABR/AMR in India and globally. Brushing up on my notes the night before and making sure I was up to date on all the tutorial readings and prepared to answer the questions Dr Z asked me proved immensely beneficial.

Although speaking the language of medicine and microbial sciences helped me communicate with my interlocutors and win their trust I could not, in the usually understood sense, 'participate' in the daily goings on of my site. Without the requisite training, and its testament in the form of a degree, my site of engagement did not afford 'participation'. Wind (2008) has discussed the difficulties of conducting participant observation in hospitals. Participating in a hospital setting is limited to participating as a) healthcare professional/patron; b) patient/client c) an attendant. However, Wind circumvents the problem of participation by stating the only role suited for social scientists in this setting is that of a 'researcher' who is, ipso facto, a non-participant. Although, my role as a researcher was to be officially recognised, and its performance allowed in the setting, the contours of this recognition would remain murky.

My presence at the hospital was, however, eased through my taking up of other roles (not discussed by Wind) that made themselves available in the orchestrated conduct of everyday clinical care. One of these roles that I would be often called upon to play, by the physicians themselves, was that of translator. A large portion of the patient body in this hospital was comprised of people from Eastern India and Bangladesh—a sizeable chunk of them only understood and spoke Bengali, which is also my mother tongue. Although there were, of course, translators hired by the hospital, they were often overextended and not always available immediately. I would, in many instances over the course of the fieldwork and at the insistence of my interlocutors, act as a translator between these patients and the doctors. This was another strategy that helped me negotiate my presence in and maintain access to the field. There were several other factors that eased my presence in the ward. Interestingly, as an infection control and prevention protocol, doctors were advised against wearing lab coats, since these tended to be carriers of germs. Because there was no visible signifier of being a doctor, it was much easier for me to blend in. It was also fairly easy to negotiate my presence within the clinical encounter. The patients were used to engaging with several doctors, residents and registrars who circulated through the wards on a daily basis, and my presence seldom engendered additional curiosity.

Over the course of my stint as an 'observer' it became clear to me that the observership worked extremely well for my purposes, since what I essentially wanted to do was to shadow the ID doctors. However, the observership could only last for a couple of months at a stretch because the hospital did not approve longer periods. So, in the end, I had little choice but to go through the process of being affiliated to the ID department as a researcher. During the month of my

observership an MoU was drawn up and executed between my parent institution and the hospital. The MoU identified two doctors, Dr Z at the oncology ward and Dr Y at the general hospital, as the Principal Investigators (PIs) of my project. Although the EC had initially stressed the need for a spreadsheet that would detail in tabulated form the 'variables' of interest of my research project, they eased this requirement once the MoU was signed and the legal liability was transferred to the two PIs of the project. Through the execution of the MoU my 'responsibility' now rested with the two doctors, who were not directly hired by the hospital but affiliated to it as consultants. Even though the EC had eased the requirement for the spreadsheet, Dr Z continued to insist on it since for him it continued to carry the emblem of objectivity.

At one point, and induced by Dr Z's continued insistence, I considered producing an ad hoc spreadsheet and then continue making 'additional' ethnographic observations, but soon realised that this could threaten the access I might have to the 'field'—not just spatially, in terms of how much I was given access to, but also temporally, for how long this access was granted—by limiting what and how much I was allowed to observe. The conduct of ethnography demands a more deliberate temporality and requires an extended period of engagement. (Mis)translating my concerns into this language might have also affected the amount of time Dr Z thought my project warranted. Every time Dr Z mentioned the spreadsheet I rattled off the advantages of ethnography and tried to convince him that the 'objectivity' he expected didn't work well for anthropologists and what the discipline demanded. I can't say that I managed to convince Dr Z through the force of my arguments but after having witnessed my sincerity—in coming to the hospital everyday and taking part in the activities of the department diligently—he seemed to finally relent.

In the end, and between the expectations of the EC and my interlocutors' openness, I was able to haggle for a little place for myself in the 'negotiated order' (Strauss et al. 1963) of the hospital without having to perform what seemed to me a kind of disciplinary monolingualism, and I am grateful to my interlocutors for this. I ended up spending close to 14 months in the hospital working primarily with two senior ID physicians, several registrars and residents who circulated through the department in these months, nurses and clinical pharmacists. Although this disciplinary (theoretical and methodological) multilingualism, for the most part, was welcomed in the field by my interlocutors, its contours were often nebulous and had to be negotiated everyday, as has been demonstrated by the instances highlighted above. The doctors affiliated to the ID department took pride in their academic inclinations which fostered an exciting climate of research in the department which, I suspect, made my interlocutors supportive of what I was

doing, even if it wasn't always clear to them what my intentions were.

However, this generally supportive curiosity towards my endeavours co-existed with an implicit level of unease. My notebooks would often engender curiosity and I was asked several times by my interlocutors to show what I was writing in them. A lot of my notes catalogued the minutiae of everyday clinical encounters between ID physicians and patients who required (therapeutic and prophylactic) care in the form of antibiotics while also doubling as a means for me to acquire the language of (bio)medicine. My field notes, given as they tended to be lengthy descriptions of these daily happenings, were often jokingly referred to as 'stories' by Dr Z. Categorising them as 'stories' indicated that they remained outside the realm of the 'objective' for Dr Z who would, as I have mentioned above, continue to ask for the spreadsheet detailing the data I wanted to collect through the first few months of my fieldwork, and suggest various topics of interest for me to pursue. No doubt that these suggestions were well-intentioned but they expressed his ongoing doubts about my research—the spreadsheet, with its implied catalogue of variables, performed a kind of objectivity that my field notes could not.

Conclusion

Several months into my fieldwork and during an informal exchange with Dr Z I asked him a question which made it evident that even though I was able to cultivate a little place for myself in the epistemological order of the hospital, the limits of what was permitted to, and expected of, me had to be carefully negotiated everyday. Dr Z was angered by my question and didn't understand its pertinence to my project and thought its tone to be too 'journalistic'—a descriptor which implied that my question was perhaps motivated and marred by a 'politics 'that he found unpalatable. He reprimanded me saying that I should have communicated my intentions more clearly to him and the EC and that, if I had done so, he would have reconsidered supporting my project. Latour and Woolger ([1979] 1986), in their ethnography of a biochemistry laboratory, have described how often scientists' lack of knowledge about other fields of inquiry can manifest itself either in a marked disinterest or outright suspicion. They further mention how ' . . . it is often assumed that outsiders' interests must focus on the seedier aspects of scientific life because investigators are seen to be posing questions which are essentially irrelevant to practical scientific activity' (19). Anything that falls outside of the purview of the known disciplinary template can, then, likely, engender suspicion. My presence and intentions, as a disciplinary interloper, were parsed through the emic disciplinary categories available to Dr Z and the epistemological world he occupied which led to such mistranslations. I eventually managed to

convince Dr Z that my intentions were not ‘journalistic’ and that anthropological thinking and writing required contextual knowledge that did not seem immediately relevant to the matter at hand.

My engagement with the ethnographic field came riddled with such epistemological tensions that not only made access challenging but also (re-)raised questions about what the co-production of ethnographic knowledge might mean when we don’t partake of the same epistemological ground as our interlocutors. Although my interlocutors and the EC (which had a couple of members with social science degrees) seemed open to engaging with social science research they, nevertheless, had a very delimited idea of what that meant. What I did either seemed to remain in the realm of ‘description’ and the ‘subjective’ for them or aroused suspicion when it fell beyond the disciplinary template they were familiar with. The invocation of ‘stories’ as a descriptor—and the consistent collapse of the distinction between the literary genre and my field notes that it hinged upon—functioned, I maintain, to an extent to de-fang my research and make my presence more palatable (if not always comprehensible). In the end, the negotiations, however imperfect, that I engaged within the field allowed and furthered my presence as an anthropologist there even though a concordance in my expectations and those of my interlocutors were not ever fully realised.

Ethnographic fieldwork demands the cultivation of a certain set of skills and the negotiation of a relational ethics. How these skills are to be mobilised and the position of the ethical subject is to be fruitfully engaged vary according to the ‘field’ or site of their cultivation. Additionally, knowledge production in anthropological research is explicitly recognised to be a product of interlocution or co-production. The concerns that became evident through my fieldwork motivate me to consider, by way of a conclusion to this text, what it might mean to not only work with those who are explicitly engaged in the production of knowledge (this concern has been parsed earlier in the text through the idiom of ‘studying up’) but with whom we might not share the premises of this knowledge production (here the concern that gets foregrounded is the tussle between maintaining critical distance and of submitting to an ethnographic complicity)—knowing well that a position of critical disengagement from the stakes of the ‘real’ is not feasible. Stefan Helmreich (2012) says that knowing a language well—be that of science—does not mean that we have to become monolingual. Annemarie Mol (2002), on the other hand, has demonstrated, through her fieldwork in a hospital, the multilingualism which exists within scientific practice itself—or what is done—that a focus on knowledge—or what is known—tends to mask. The struggle for me, however, arose in communicating what data looked like to me and, in so doing, balancing the kind of access I had to the hospital with being minimally legible to my interlocutors who also served as the PIs of my project; this meant that I had to

learn to negotiate the imposition of a monolingualism by the 'field'. Exploring the 'indeterminacy that divides and conjoins ethnographic distance and ethnographic complicity' as Helmreich (2012) encourages anthropologists to do might require not only the recognition of the multilingualism of the practices we study but also the refusal of a monolingualism—for instance, through the various strategies highlighted in the piece—that might be imposed on anthropological researchers.

Authorship statement

I am the sole author of this work.

Ethics statement

The research conducted is a part of my larger PhD project which has the clearance of the Ethics Committee of the Geneva Graduate Institute. Moreover, the project was also approved by the ethics committee of the hospital where the ethnographic fieldwork was conducted.

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About the author

Purbasha Mazumdar is a doctoral candidate at the Geneva Graduate Institute and a visiting scholar at the King's College London. Her doctoral research, ethnographically and historically, explores the making of the crisis of antimicrobial resistance by tacking across, and dissecting the coming together of, scales ranging from the geopolitical and the microbial/the environment and the individual/the global and the local. The project attempts to understand how this crisis plays out, and is articulated across sites, in India.

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