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A life in books

T.M. Luhrmann: On finding findings

I arrived in Cambridge to study anthropology in 1981. For me, Cambridge was a lot like Hogwarts. By late October, the streets went dark before late afternoon and the leaves skittered across the flagstones in the wind. It was damp; it was always damp in Cambridge, and it was easy to believe that there were ancient secrets in the old stone walls. In their shadows, anthropology seemed like something very new. In fact, early founders of the discipline still lunched in college. Meyer Fortes, Edmund Leach, and Audrey Richards sometimes came to the department's Friday seminar. Evans-Pritchard's niece sold textiles at the end of the street. Where anthropology had seemed like one major among others at my undergraduate American university, at Cambridge, it seemed like a young, brash discipline which challenged tradition with mud-splashed truths.

In that heady world, the queen of the social sciences was philosophy, not economics, as people sometimes said back home. Down the corridors where John Maynard Keynes had walked, the ghosts that seemed to matter to the anthropology department were philosophical ones. I read through Wittgenstein in my second year in graduate school the way young American anthropologists now read Agamben, with the sense that these were the texts that serious students mastered. I attended Elizabeth Anscombe's last lectures as if they were glimpses of the grail. She was perhaps Wittgenstein's best student, and she had compiled his book *On certainty* from his notes. One day in class she was struggling to ferret out the meaning of a sentence in the text. Why, she asked us, had Wittgenstein used the direct article for this noun? There was a long pause in which she looked thoughtfully at her notes. Then she said suddenly: 'Ah! I mistranslated that sentence'. Another day, a timid young scholar poked his head into the room and asked us whether this was a class on the philosophy of knowledge. Anscombe gave him a long, measured look. 'You could say that', she responded. 'But that is not what you mean'.

There was a clear sense that the philosophers were the smart, respected cultural insiders and that anthropologists were slightly scrubby outsiders. The sharpest anthropology students went to talks by Bernard Williams and Quentin Skinner and they read Quine and Kripke. The department turned out almost to a person when Richard Rorty came to town. When Jack Goody retired, the university hired a man first trained as a philosopher, Ernest Gellner, to take his place. He became my adviser. My generation – Pascal Boyer, James Laidlaw, Simon Coleman, Henrietta Moore – all started out writing as if our goal was to persuade analytic philosophers to think differently about belief, just as Evans-Pritchard had done.

The problem, of course, was that while the philosophers loved having a direct source of 'sublunary Martians', as Clifford Geertz (1986: 117) had so splendidly observed, they weren't very interested in anthropology. They no longer read books by anthropologists. And the anthropologists were not, in general, much good at philosophical argument. That was not just because philosophers use language in very special ways, honed by years of training. It was because the kind of thing that the philosophers were doing – their basic philosophical project – was not anthropological. (Here a comment by Wittgenstein comes to mind: 'I am sitting with a philosopher in the garden; he says again and again "I know that that's a tree", pointing to a tree that is near us. Someone else arrives and hears this, and I tell him: "This fellow isn't insane. We are only doing philosophy"' [1969: 120 n. 467].) The philosophers were ultimately interested in the language game of coming up with a compelling

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description of words like 'belief'; the anthropologists were trying to explain what was going on when their field subjects sacrificed a cow. As a result, the anthropological work on belief was becoming increasingly frustrating, culminating in Rodney Needham's 1972 book on the subject, which managed to argue that no one believed anything at all.

Meanwhile the problem with Wittgenstein – and with Foucault, Agamben, and the other philosophers young anthropologists read today – was that their questions were so big that no empirical research could answer them. These authors invite us to reflect upon what we know already about human life, and to think about it differently, rather than setting us empirical puzzles we can solve. They ask 'What is the nature of human understanding?' rather than 'Why do the English bury their dead and the Zoroastians leave the corpses to be picked clean by vultures?' This was also the era when Clifford Geertz (1973) was arguing that anthropology was more like literary criticism than it was like science. That suggested that the details of what fieldworkers discovered weren't particularly important as discoveries (although one could say that this was not quite what Geertz had meant). After all, the task of a literary critic is to interpret novels readers have already read, and to help those readers into a different understanding of them. Literary critics deliver new understandings, not new data. Then Geertz's postmodern critics took him to task for his arrogance in claiming to represent the Other. This new intellectual environment – the crisis in representation, the postcolonial critique, the comparison to literary criticism – led many anthropologists to think of ethnography as being more like interpretation than like research in which one discovered something. To be sure, we all understood that our goal was to have a question that fieldwork could answer. But the climate invited us to imagine that our goal was to evoke and to challenge rather than what other social scientists did, which was to find out something new and explain why it mattered.

In fact, back in the day, as a young graduate student emerging into an intellectual world shaped by the postmodern and postcolonial critique and by Geertz's lush prose, I imagined that the goal of the book of the research – the ethnography as published – was to provide an account of another world that would capture that world perfectly in a literary way and as elegantly as Geertz had done. One's goal was surely to write a book that would live forever, just as the books of E.E. Evans Pritchard and Meyer Fortes had done. It would have to be a humble book, acute in its understanding of the limits of observation, because that was how the critique had schooled us. And because it was humble, it would explain something particular – that other world – but not something more general. This was the chastening effect of critique. The consequence was to shift one's ambitions away from empirical argument (what *was* the social effect of literacy?) to imagining the ideal book as a gem: compelling, precise, complete. Such a book would stand on its own to challenge the reader's assumptions. It would make the strange familiar and the familiar strange; it would present a complete picture of its ethnographic topic; and nothing more needed to be said.

Of course, this way of imagining the ethnographic book sets the bar impossibly high. If the goal was to astonish, the job had already been done. Once we have the Azande (Evans-Pritchard 1937), how many more sublunary Martians do we need? If the goal was to write a perfect gem, it was always easy to see the need for a sharper cut. In many ways, the intellectual climate of the day set up young ethnographers to feel like failures.

It wasn't until I spent time with an interdisciplinary group at the University of Chicago that I began to think of my own intellectual task differently. The Chicago Templeton Network mostly comprised academic psychologists and biomedical researchers – scientists whose papers were often very short and centred on tables and graphs. They simply didn't believe me when I told them what I had seen from my ethnographic research: that some people were better at prayer practice than others, and that prayer shaped the way that they experienced their world. The group wanted different kinds of evidence to support the claim. This annoyed me. After all, I had spent many years collecting those ethnographic observations. It made me so irritated that I set out to prove my point. I did some more structured research and found that the outcome supported my ethnographic observations. When I gave my presentation, beginning with my years of ethnographic research and leading up to my first quantitative finding, they listened patiently until the first scatterplot went up on the screen. Then someone smiled at me and said: 'Data!'

This also annoyed me.

Yet the work liberated me, because it shifted my focus from perfection to puzzles. The specific attempt to compare people to one another systematically raised as many questions as it answered. I had demonstrated that people who differed in ways measured by a standardized scale were more likely to enjoy praying and more likely to experience what were supposed to be the fruits of prayer: a real relationship with God; a sense of a back-and-forth conversation with him; even a sense that God was sensorially present. I had been able to show that some people were better at prayer than others, and that these differences seemed to change the way they experienced their world in the most concrete way.

I had done this by using a scale that measures something called 'absorption', which asks people whether certain statements are true for them: whether

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they like watching clouds change shape in the sky; whether they sometimes experience things the way they did as a child; or whether they can change noise into music just by the way they think about it (Tellegen & Atkinson 1974). The scale seems to probe the manner in which people experienced their inner and outer senses. It seems to ask: do you enjoy getting caught up in your imagination? Do you like to take time in the morning to drink in the sky? The work suggested that the way people were oriented towards their mental experience was quite important to their experience of prayer.

Let me pause here. My decision to use a scale might seem peculiar to many anthropologists. Scales probably seem like dead tools that reduce the complexity of human experience. And, of course, they do reduce that complexity. But in an anthropologist's hands, they also help to open up the work. Scales, after all, are just tools to help us to compare: to say that members of this group, compared to members of that group, own more land, or respect their teachers more, or spend more time in meditation. Anthropologists create scales – structured interview protocols – all the time without using the term. A structured list of questions also helps you to see differences between experts and non-experts. These people seem more interested in narrative, more focused on detail, than those, and maybe that difference is important.

When we ask systematically about the differences, it helps to build an argument. It gives us confidence in what we see. Standardized scales are created by other people, who have used them in many settings. I had an intuition; I used a standardized scale, and it supported the intuition; and so I felt bolder when I gave a talk, because I was more confident in what I had to say. In anthropology, these tools are usually called 'mixed methods', and they were far more part of the early ethnographies than many of us now remember. Margaret Mead collected an extraordinary amount of data of many different kinds, using all sorts of methods. We might remember her for the Samoan girls (1928) she wrote about, but her work on the Arapesh (1971) is a dense compendium of data.

Why was this liberating? Because I was no longer aiming for the definitive account, the perfect gem. I had a puzzle, and it was clear that many people would chew over it and shake it back and forth like a dog with a rope toy. What, after all, *was* absorption? Was it an individual trait, somehow encoded in a body? Or a cultural invitation, encouraged by different social practices? I knew what the scale's authors thought – but I wasn't sure I agreed with them. Now what I was doing was more like a detective story, not like literary criticism. Now the details mattered, because it was suddenly clear that no one actually knew them. No one knew whether the absorption scale would pick up similar religious phenomena in other countries, and what it would mean if it did or it did not. No one knew what it meant that people high in absorption had funny little hallucination-like experiences, and nor, for that matter, whether it followed that they were like people with schizophrenia or not. This was fun. It was not because I had added a quantitative dimension to my work. It was because I had a clear sense that there are real puzzles and empirical research can help to solve them.

To be clear, I still believe that the basic research method of anthropology is ethnography. I still spend long hours with people, sitting in the park with people who hear voices that taunt them, going to weekends with people who talk with the dead. But I have now begun to think about the goal of ethnography differently from the way I did in the days before my interdisciplinary encounter. I think differently about how and what I want to explain. These days, I think of myself as having findings.

Findings are empirical observations which call out for explanation by generalization. The generalization is a hypothesis, which later findings will support or challenge. Findings are news in the way that the general idea that Moroccans, say, are Moroccan is not. They offer puzzles that need to be solved. Here is an example. I have begun to spend time in psychiatric settings outside the United States, chatting to patients and doctors, learning about how people find their ways into an in-patient ward. What I do these days that I did not do when I was younger is to ask those inpatients systematic questions about what they hear. That means that I can compare them to what other patients say elsewhere.

In the Accra General Psychiatric Hospital, people who meet criteria for schizophrenia often report that the voices speaking to them come from God. On average, they report that their voices are more positive than those reported by a comparable group of Americans. In Chennai, similar subjects more often said that they heard their kin. They more often said that they heard the disembodied voices of persons they already know in the flesh. Not one person in my American sample told me that their dominant voice-hearing experience was positive, and only three Americans out of twenty told me that they heard the voice of someone they actually knew (Luhrmann, Padmavati, Tharoor & Osei 2015*a*; 2015*b*). Why? I thought it might have something to do with the social worlds in which they begin to hear those voices – more or less religious, more or less interdependent, more or less in worlds in which the mind is imagined as bounded and hearing voices means you are crazy. That's important, because how harsh the voices seem has something to do with how well people recover – and if the content of the voices can be shaped by culture, it suggests that medication ought not to be the only way our clinicians intervene.

I think if anthropologists went back to empirical comparison, the field would feel liberated, just as I did.

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If we as a field did empirical comparison with the aim of observing different patterns of behaviour and developing theories that explain specific differences – findings – in such a way that we can say whether those theories are better or worse, we would have so much to talk about, and to so many people. Empirical comparison does not need to be an assertion of arrogance, as the postmodern critique sometimes assumed, but can be a concession of humility. Claims based on findings are necessarily limited. They are partial attempts to explain a puzzle. Once presented as such, the ensuing debate keeps one humble still. They are contributions to a conversation in which one is one of many players.

And findings matter. If I can show that the voices heard by people with psychosis are different in different countries, I am able to argue that voice-hearing responds to learning. That paves the way to think differently about what we should do for those who want to experience their voices differently. Explicit comparison enables us to make claims about the way specific cultural features may have specific consequences. And that, as Margaret Mead urged long ago, is how anthropology could change the world.

I know that the pieces in this section are really supposed to be about books. I have mostly been talking about my relationships to books – to E.E. Evans-Pritchard's *Witchcraft, oracles and magic among the Azande* (1937), which set the terms of my work; to my adviser Ernest Gellner's *Legitimation of belief* (1974), which used to frustrate me because it was so brief and now has become a kind of lodestone; to Ludgwig Wittgenstein's *On certainty* (1969), which I read so carefully when I was young; to Margaret Mead's *The mountain Arapesh* (1971), which Gilbert Lewis held up one afternoon in seminar and described as amazing; to Meyer Fortes' *Oedipus and Job in West African religion* (1959), which everyone read. Let me end with a mad book by Julian Jaynes which has fascinated me for decades, and which I now think is brilliant, if still mad.

The origin of consciousness in the breakdown of the bicameral mind (1976) is one of those fertile, over-ambitious books that gets many things wrong, but in such an interesting way that readers, on finishing it, find that they think about the world quite differently. At least, I did, although I think the book crept up on me year by year until I suddenly decided that this odd book I'd read in college had a fundamental insight – and had set me the puzzle that became my life's work.

Jaynes taught psychology at Princeton, back in the days before psychologists had walled themselves off from literature, when he noticed that in the Homeric epics, the gods took the place of the human mind. In the *Iliad*, we do not see Achilles thinking. Achilles acts, and in moments of strong emotion, he acts as the gods instruct him. When Agamemnon steals his mistress and Achilles seethes with anger, Athena shows up, grabs him by the hair, and holds him back. Jaynes argued that Athena popped up in this way because humans in archaic Greece had no words for inner speech. So when they felt compelled by this strong internal force, they attributed that sensation to the gods. 'The gods take the place of consciousness', Jaynes wrote (1976: 72). Moreover, Jaynes thought that in these moments they heard the god speak with their ears. He thought that the inability to name the sensation as internal altered it so that in moments of powerful feeling, moments when we feel pushed from within by our own overwhelming rage or joy, they heard the cognitive trace of that emotion audibly, and as if it was coming from outside.

Jaynes asked:

> Who then were these gods that pushed men about like robots and sang epics through their lips? They were voices whose speech and direction could be as distinctly heard by the Iliadic heroes as voices are heard by certain epileptic and schizophrenic patients, or just as Joan of Arc heard her voices (1976: 73-4).

Well, maybe yes and maybe no. To me the point was that the way we pay attention to inner sensation changes the nature of the sensation, sometimes profoundly. The way we recognize mental events and deem them significant, the way we reach for what we take to be real – those differences shape what we know of gods and madness.

The book begins: 'O, what a world of unseen visions and heard silences, this insubstantial country of the mind!' (Jaynes 1976: 1). I feel drab in Jaynes' company. I just want to get the facts right. But this was another of his lessons. He taught me that data can sing.

NOTE

This piece by Tanya Luhrmann, reflecting on her reading and thinking, began as an interview with Dolores Martinez, Reviews Editor.

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