A problem with detecting problem-solving outside the natural sciences

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Abstract. In this paper, I draw attention to an obstacle to determining to what extent the portrait of normal science as a problem-solving activity applies outside the natural sciences. I give two examples from social anthropology, one from the heyday of British structural-functionalism and one from recent British anthropology, "responding" to Marilyn Strathern's problem of the feminist fieldworker.

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There is a well-known portrait of the history of a natural science discipline (Kuhn 1996). According to this portrait, there are two kinds of periods in this history: revolutionary periods and periods of normal science. In a period of normal science, a research community works on solving well-defined problems – puzzles. Community members agree on what a good solution would look like, having in their mind an example of problem-solving which they regard as ideal. They share assumptions and solve problems, but some problems are unsolved and data is gathered which gives rise to new problems. Unsolved problems build up. Eventually a revolutionary period is initiated, in which some scientists search for a new framework for normal science.

To what extent does this portrait apply outside the natural sciences, specifically to the social sciences? I shall focus on normal science. Are there periods of normal science in the social sciences? Certainly there is problem-solving, but it seems to me that there is an obstacle to

tracking problem-solving here. Sometimes a social scientist writes material that is obviously relevant for addressing a certain problem and it is likely that they are aware of that problem, but they do not explicitly refer to it. They do not say something along the lines of, "Here is the problem I am addressing… Here is my proposed solution…" I give two cases from British anthropology below.

Lévy-Bruhl, Evans-Pritchard, and causation. The French thinker Lucian Lévy-Bruhl is associated in British anthropology with bold claims about the way of thinking in primitive societies. He claims that the individual in such a society is brought up to think "mystically." Consequently, if an individual consciously attends to any entity, he perceives it as involved in mystical relationships. For example, his shadow is perceived as his soul (see Evans-Pritchard 1965: 85). And he does not walk out in the midday sun, because of what will happen to his shadow. A problem the anthropologist is left with is how, if mystical thinking plays such a large role in some societies and mystical thinking is false, do these societies manage to survive at all? One can formulate the problem as a dilemma. Either:

(a) Deny that mystical thinking plays a significant role in the societies studied, a move which promises to explain how these societies survive but seems inaccurate about these societies;

or

(b) Say that mystical thinking plays a significant role, but then one has to accept Lévy-Bruhl's extreme portrait of thinking in primitive societies and is stuck with the problem of how these societies manage to survive.

E. Evans-Pritchard's *Witchcraft, Oracles, and Magic Among the Azande* appears to address the problem, though he does not explicitly identify it as his focus (see Jarvie 1967: 236).

What Evans-Pritchard does is give a precise account of when members of the Azande people – who would have been classed as primitive at the time – engage in mystical thinking. As he portrays the society, various events are described in causal terms familiar to us. Where a mystical concept is employed, specifically the notion of witchcraft, it is (often) where we would employ the notion of an unfortunate coincidence. In a much-quoted passage, the British anthropologist writes:

> In Zandeland sometimes an old granary collapses. There is nothing remarkable in this. Every Zande knows that termites eat the supports in course of time and that even the hardest woods decay after years of service. Now a granary is the summerhouse of a Zande homestead and people sit beneath it in the heat of the day and chat or play some African hole-game or work at some craft. Consequently it may happen that there are people sitting beneath the granary when it collapses and they are injured, for it is a heavy structure made of beams and clay and may be stored with eleusine as well. Now why should these people be sitting under this particular granary at the moment when it collapsed? (1976 [1937]: 22)

Soon Evans-Pritchard supplies the Zande answer: it was because of witchcraft that they were sitting there at precisely that moment. Thus he portrays an option overlooked by the dilemma above. The option locates mystical thinking in place of our "unfortunate coincidences," rather than in place of much of our folk science. (Unless "eleusine" is a mystical notion!)

But he does not actually refer to Lévy-Bruhl. Probably every British anthropologist at the time would have regarded him as responding to the French armchair anthropologist, but to the best of my knowledge he does not explicitly engage with the French anthropologist within the book. Without the appropriate background, one might read the passage and not realize what problem Evans-Pritchard is engaging with. In a later work, Evans-Pritchard explicitly engages with Lévy-Bruhl (1965), but not in the earlier Azande work.

Marilyn Strathern, Sarah Green, and the feminist fieldworker. There is a problem of how the anthropologist's moral commitments apply in the field, where they may well study groups with customs that go severely against some of these commitments. A more specific version of this general problem, or at least what appears to be a more specific version, was formulated by Marilyn Strathern: the problem of the feminist anthropological fieldworker (1987: 288-289). It too can be presented as a dilemma. The feminist anthropological fieldworker, tasked with reporting about a specific group, either

(a) Mixes with males, in which case she betrays her feminist commitments, which tell her not to;

or

(b) Does not mix with males, in which case she betrays her anthropological commitments, which tell her that she has to mix with them in order to produce an adequate description of the society or culture under study.

In the next decade, anthropologist Sarah Green conducted a fieldwork study on an all female community of radical and revolutionary feminists in London. That gives us a solution worth

considering: "I can be a feminist anthropologist fieldworker by doing fieldwork in an all-female community." Now Green refers to some works by Marilyn Strathern, though not one presenting the problem, and thanks her in the acknowledgements. And radical feminism, or a version of it, is actually Strathern's example of feminism (see Boddy 1991: 126). I think some readers acquainted with this research community would see Green's monograph in terms of the problem Strathern has raised (e.g. Visweswaran 1988; Boddy 1991). But Green does not say, "This is the problem and this is the solution I am exploring." Plausibly – 50% chance or higher, given what we know – that is what she is doing.

Appendix A: a good solution?

Is doing fieldwork in an all-female community a good solution to the problem of the feminist fieldworker? I anticipate someone's saying: "There is such a thing as the masculine mode and the feminine mode, but both modes are available to female human beings and to male human beings. And any community must feature some people who use the masculine mode – both modes are necessary for the functioning of a community. One's feminist prescriptions will then tell one not to interact with female human beings who are operating in the masculine mode." I shall not assess this proposal here.

Appendix B: multiple interpretations

It may be proposed that it is against the interests of the anthropologist to explicitly specify which problem they are engaging with in a fieldwork monograph, because they do not wish to close off other interpretations of their work: other framings, to use a metaphor. The aim

of this paper is not to recommend a change. But I shall comment on this proposal. It is not obvious that a framing in terms of a problem will always determine how readers receive the work. One can engage with the background philosophy of science within a child psychology article, say (see Carey 1988; Edward 2022a, 2022b). In practice, some research communities may find that one fieldworker frames their material so that it is about an explicit problem very well, but the descriptive data is impoverished – this person should be in the philosophy department, or cognitive science perhaps – while the other fieldworker provides rich data, but the framing which relates the data to a problem is not great. One just does not attract people who do both, or do both as one would like; or in some cases do both *for you*.

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