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Who needs methods?

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In a recent presentation of my research at a conference I left out any mention of the methodology I had applied. I was struck by the complacency of the audience, interested in discussing the findings, but with little concern for how I had come up with them after all. This triggered a reflexive process about communication in academia, and my own motivations for engaging in such a process.

Transparency or persuasion?

In June 2007 I participated in an international conference organised by the European Society of Ecological Economics where I was one of four participants on a panel. Each of us gave a twelve-minute presentation followed by eight minutes of discussion. With such a short time to present, I planned to leave out the methods I am using, but make clear my willingness to discuss them afterwards. As it happened, I was carried away by my passion for the research theme (exploring illegal actions as a response to static and illegimate institutions) and forgot to mention them at all.

My findings could have been the result of widely different methods – archival analysis, surveys, selective interviews, participant observation, etc. – which do not necessarily have the same underlying epistemologies. If I had been in the audience I would have asked myself whether the presenter considered that combining methods based on conflicting epistemologies could lead to great difficulty in making sense of the findings - or even impede any meaningful conclusions. It would be a fair question and one that would be important in order to clarify how the research was done, thus helping to lend more or less significance to what was being presented.

Nevertheless, there were no questions about epistemology, methodology or methods. Instead, the audience was eager to know more details about the case and elaborate further on the issues I had highlighted - the question time developed into an exciting dialogue.

The nearest the audience got to inquiring about methodology was when a person asked for details of my relationship with the participants – and it is worth mentioning that it was put just like this, openly, as opposed to my relationship *as a researcher* with the participants. I asked whether she could be more specific, remarking that "relationship" could range from how we behaved in interviews to friendship. The answer can be summarised as "in general". It seemed to me then that the question had more to do with ethics than methodology, with a hint of doubt as to how much of my account could be regarded as impartial or whether it was activism disguised as research. Thus, in my reply I talked about how the current stage of my research was the result of years of work with, broadly speaking, the same group of particpants, and stressed how I have strived to avoid negative interference between the personal and professional aspects of my relationship with the participants. I then enquired as to whether this was a satisfactory response, and it was.

A thorough assessment of methodological rigour requires knowledge of several aspects of research[1] which are not easily reduced to a check list or recipe, especially in the case of social sciences. We need to know which perspective informs the choice of theoretical/conceptual framework, how the methods and tools are applied and how the information gathered is analysed and communicated. It is a complex task, made more difficult by the fact that there are many paths which can be taken by the researcher, each with its own logic and coherence. Again, this is probably felt more strongly in social sciences, as the ubiquitous application of cartesian scientific method in natural sciences provides a universal standard and makes the task much easier – although also more prone to become self-fulfilling.

I would argue that this is one of the reasons that a multidisciplinary audience[2] be persuaded by a convincing presentation rather than a comprehensive exposition of methodology – any evaluation has a high risk of being equivocal. Or, perhaps more to the point, no one feels capable of voicing criticism or assessment. From my experience in contexts where researchers from different disciplines worked collectively, I have seen that considerable effort is required for people to assess something they are not totally familiar with – common reasons mentioned by colleagues for this being the chance of sounding superficial, or for the criticism to backfire. As a consequence, methodological rigour is assessed "by proxy", through a combination of belief in the trustworthiness of the communicator and a hope that others well versed in the area will intervene. In other words, if it sounds correct, and no one else around seems to think otherwise, it must be alright.

It may sound obvious to affirm that the group people belong to has a noticeable effect on the way they behave, but I think it is worth highlighting for two reasons. Firstly, working in groups in which people come from several backgrounds and disciplines has become commonplace in academia. Thus, it is important to recognise that in addition to the positive effect of having diverse perspectives integrated, there is also a challenge which may well never be uncovered. It might be easier to avoid the difficult task of promoting transparency and accountability, as this might lead to confrontation and hamper teamwork. Secondly, it could serve to raise awareness of the fact that the good practice of reliable scientific communication can be easily replaced by sophisticated public relations. As we have seen in politics, both recently and in the past, it is possible to replace policies with empty publicity stunts and gain the approval of even the most enlightened audiences.

Reflecting about reflecting

This account could be taken as a confession, whereby I acknowledge my mistake and seek some sort of relief by exploring its causes and consequences. However, I do not wish to expiate, as I do not see any wrongdoing here. What I am attempting instead is to understand my position in a field which is new to me, where the rules and norms are unknown, as are the consequences of following or breaking them.[3] As someone who is changing course, moving from the clear and structured channels of natural sciences to the unpredictable waters of sociology, I need to find new ways to guide myself in this journey: new language, listening and understanding are necessary, new behaviours are developed, values are reassessed.

1 See Punch (2005) for a "pragmatic" approach to evaluative criteria and Denzin and Lincoln (2000), section V, for a comprehensive discussion of evaluation in qualitative research.

2 I could identify ecologists, economists, sociologists, engineers, anthropologists and biologists. This was done either in conversations directly with those who asked questions or by identifying people from their presentations.

3 Skeggs (2002) drew my attention to the need to move from "telling and confession to practice and positioning" (p. 369)

My training in natural sciences, which followed a strictly positivist perspective, was clear about the paramount importance of presenting methods straightforwardly and in the finest detail possible. Therefore, it was with some reluctance that I decided to keep methodology out of the sequence of slides, as if ghosts from my former training would come to haunt me at the presentation, shouting reprimands for my lack of rigour. Nonetheless, I also felt reassured by the tolerance that social sciences generally shows towards variations in the presentation of information, particularly in the qualitative research approach which I follow - at least when compared with the rigid codes of conduct of natural sciences, where the presentation sequence of *introduction/material and methods/results/discussion* is seldom challenged.

Furthermore, it is possible to go a step further and take a critical look at how we play our roles as researchers. Taking this paper as an example, I can barely conceive anything like it happening in a natural sciences environment – elaborating on the implications of the abscence of methods in a text or presentation would probably be considered a waste of time (I can imagine "a typical social sciences fudge" being said in a corridor chat), certainly not worth another piece of work in its own right.

Even though the "hard sciences" are effective at laying down directives for proper behaviour, which is particularly useful when ethics are the objective of such directives, the researcher - as someone with feeling and memory as opposed to an idealised, objective and impartial observer - is seldom the focus of critical appraisal. However, it should be noted that reflexivity is not without its caveats. Skeggs (2002) sums up the need for a critical stance on this issue, making the distinction between "[...] *accruing* reflexivity [...] as a cultural resource to authorise the self and *doing* reflexivity: building sensitivitry into research design and paying attention to practice, power and process." (p. 368, emphasis is mine). With this in mind I have tried here to achieve a balance between my account of an experience, which is the result of a reflexive exercise and could contribute to a debate aimed at the practice of more accountable research, and a reflection on the initiative itself, seeking both to deepen the reflexive process and open up new paths of discussion.

References

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