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To cite this article: Eija Vinnari (2021): Animals, Activists and Accounting: On Confronting an Intellectual Dead End, Social and Environmental Accountability Journal, DOI: [10.1080/0969160X.2021.1889387](https://doi.org/10.1080/0969160X.2021.1889387)

To link to this article: <https://doi.org/10.1080/0969160X.2021.1889387>



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Published online: 22 Feb 2021.



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Animals, Activists and Accounting: On Confronting an Intellectual Dead End

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ABSTRACT

The purpose of this commentary is to encourage academics to be more open about the intellectual challenges involved in undertaking research. To that end, I will tell you my story of confronting an intellectual dead end and reflect on the more general lessons that could be drawn from this experience.



KEYWORDS

Research challenges;
personal reflection; animals

Introduction

The origins of this commentary go back to the Centre for Social and Environmental Accounting Research (CSEAR) congress held in St. Andrews towards the end of August 2019. This annual CSEAR gathering constitutes the bright spot of my academic year, something I look forward to well in advance and then reminisce during the long winter months. However, in 2019 my elation was replaced by a more sombre mood because I would have to present a paper that, bluntly speaking, was going nowhere. A couple of weeks before the conference my co-author and I had decided to give the presentation a new sub-title: 'The story of an intellectual dead end'. The editors of *SEAJ*, who were among the conference audience, later asked me to expand that presentation into a commentary to initiate a broader discussion on this rarely discussed theme. Conference presentations are usually constructed to project an image of a study having progressed in a smooth, linear fashion from research idea through choice of theory and methods to meaningful findings and implications. Such presentations can be viewed as academic equivalents of social media posts depicting perfect meals, stylish homes and happy families. Just like exposure to such posts may evoke anxiety and envy, witnessing a streamlined presentation may result in members of the conference audience, especially junior scholars, feeling stressed and incompetent if they confront 'a hitch, a snag, a catch, or a hiccup' (Latour, 1999, 191) on their research journey.

The purpose of this commentary, then, is to liberate us anxious academics from the urge to manage our colleagues' impressions and to encourage us to be more open about the challenges involved in undertaking research. To that end, I will tell you my story of confronting that intellectual dead end and reflect on the more general lessons that could be drawn

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from this experience. Needless to say, my recollections constitute just one representation of how the events unfolded and all mistakes in it remain solely mine.

The never-ending story of accounting, activism and ontological politics

Prologue

Back in 2013, my colleague and I were chatting in a cab on our way to the airport, heading back from another CSEAR conference. I told him I wanted to undertake research that reflects my values, to somehow support the cause of animal rights activists. He came up with the idea that we could connect the topic to the emerging literature on counter-accounts. During the hour that it took to drive from St. Andrews to Edinburgh, we already got quite far in planning the study, and back home, we eagerly began collecting empirical material. Ultimately, the data proved so plentiful that we were able to divide our analysis into two separate studies, which, after the usual rounds of data analysis, writing, seminar presentations and journal reviews, were both published in the same year. One of them (Vinnari & Laine, 2017) is a multi-modal discourse analysis of how animal rights activists' counter-accounts (i.e. websites containing videos from animal farms) attempt to morally engage their audiences, while the other (Laine & Vinnari, 2017) is a Laclauian discourse analysis focusing on how the activists' campaign was perceived by the broader society. Writing these studies was fairly easy compared to what was about to happen with their sequel.

Act I

Despite publishing the two studies, we felt the topic of animal rights activists' counter-accounts was not yet sufficiently discussed, and we still had plenty of unused data, so we began to consider how to approach it from a new angle. We trawled the journals of related social sciences, finding fascinating studies about social movements, environmental conflicts, and ontological multiplicity. While this commentary is not intended as a theoretical piece, I will dwell on the latter concept for just a little while as it plays a key role in this story. In a nutshell, the notion of ontological multiplicity refers to the philosophical idea that instead of a single truth or object on which individuals have different perspectives (perspectival ontology), there are several co-existing truths or objects generated in different practices. Since reality is actively made in practices, 'its character is both open and contested' (Mol, 1999, 75). This is claimed to open up the opportunity for ontological politics, in other words, making political claims about which version of reality should be allowed to exist. To give a concrete example, Alzheimer's disease is enacted in various locations and practices such as scientific conferences, laboratory testing, and care homes, resulting in different versions of the disease and how it should be treated. Ontological politics steps in when for instance biomedical and pharmaceutical enactments of the disease are allowed to dominate, while denying versions of the disease that can be treated with innovative institutional care instead of medication (Moser, 2008).

Intrigued by the idea of ontological multiplicity, my co-author and I went back to reading social movement studies and realised that frame analysis, a ubiquitous theory

in social movement research, explicitly subscribes to a perspectival ontology. This observation formed the motivation for the first draft of what we referred to as 'Animal Paper 3'. In this draft, we set out to study how a shift from a perspectival ontology to ontological multiplicity could bring new insights to research on social movements and counter-accounts. We did not have a ready answer in mind but were convinced that it would emerge as we began to analyse our empirical material through this new lens. I remember being very proud of myself at the time as I was convinced that our questioning of the underpinnings of frame analysis was a prime example of problematization *à la* Alvesson and Sandberg (2011).

However, problems began to surface when we turned to our empirics. We identified multiple co-existing versions of an object called 'cow' in activists' counter-accounts and animal industry's corporate social responsibility reports. We realised this finding was only marginally different from saying that the activists and the animal industry hold very different perceptions of a cow. The dreaded 'So what?' question hung heavily in the air, and we were not able to find any justification for our intuitive claim that ontological multiplicity would bring additional insights to the study of social movements and their counter-accounts. We had thus arrived at our first dead end and decided to let the paper rest for a while.

Act II

Several months later as I was working on another manuscript, I needed to revisit a debate on whether Actor-Network Theory (ANT) enables the researcher to intervene in organisational practices s/he deems harmful. Briefly, Whittle and Spicer (2008) had posited that ANT was not up to the task, while Alcadipani and Hassard (2010) had replied that at least the 'ANT and After' literature, characterised by ontological multiplicity, could enable the scholar to take a political stance by defending a particular version of reality against others. An idea struck me: we could position Animal Paper 3 in relation to this debate, our motivation being that subscribing to this worldview would allow us to engage in critical performativity by undertaking ontological politics. We were thus keen to examine how ontological multiplicity would enable us as scholars to legitimately express a value-based opinion on animal farming.

As the reader may already guess, this new problem formulation led us to another epistemic quagmire. One of our obstacles related again to the empirics. Our main idea was to study how farmed animals are enacted in the reality-making practices of accounting and counter-accounting. However, the majority of studies informed by 'ANT and After' (or ontological multiplicity more broadly) are ethnographies. Neither of us had the resources or the will to undertake ethnographic research, so we ended up analysing a meat company's annual report and an animal rights activists' counter-account (website). We found that the activist version of farmed animal ('suffering individual') was radically different from the ones that we encountered in the meat company's annual report ('raw material', 'source of protein'). Yet, to anyone with at least a rudimentary knowledge of industrial animal production, there was nothing very original or surprising about our findings.

Another thing that really bothered us was the issue of how ontological politics could be undertaken in research practice. While there might be societal debates regarding the

proper way of treating diseases, as in the case of Alzheimer's mentioned above, all parties to the debates agree at least that diseases are detrimental to human health. In contrast, animal farming is a deeply contested issue where the parties engaged in the debate share very little common ground. We still did not understand how we as analysts could juxtapose the different versions of a cow and then state that we prefer one of them. When we discussed these issues in seminars preceding CSEAR 2019, some of our commentators were rather outspoken in their views. As there is an abundance of inherently critical theories, they said, what does it matter that 'ANT and After' is ambiguous in this respect? Why were we wasting time on conceptual imbroglios when the world was burning? These were very good questions and our inability to provide satisfactory answers constituted our second intellectual dead end. After some reflection, we decided to discard the paper for good (or so we thought).

Epilogue

To go back to where this commentary started, that is the CSEAR 2019 conference, it was quite a relief to notice that the discussion following my presentation took on a very collegial tone with audience members sympathising with our predicament and offering advice.¹ Moreover, quite a few people came to tell me afterwards how refreshing and relieving it was to hear senior scholars openly admit that they too sometimes fail. All in all, it seems that the paper resonated with the audience and we therefore did the right thing in not withdrawing it from the programme. Then, several months later, a colleague who had heard an earlier version of the paper being presented was keen to know how the study, which he had considered very promising, had progressed since then. Slightly embarrassed, we told him that the paper was lying in the figurative bottom drawer, after which he kindly offered to read what we had written so far. Having done so, he still seemed intrigued enough and, heartened by his interest, we decided to give the paper one more chance and invited him to join us as a co-author. At the time of writing (January 2021), the three of us are only in the process of exploring how to take the paper forward but I remain hopeful that one day our Animal Paper 3 will have a proper title and something meaningful to say about animals, activists and accounting.

Reflections

What are the more general lessons that might be drawn from the events described above? Well, first of all I think the case illustrates what it means to take your work very personally, to the extent that a failure engenders feelings of shame instead of mere embarrassment. To some degree, I think this is related to an individual's personality traits. For me, a shy introvert, academic achievements have always been the main way to get positive attention, and they are the yardstick against which I measure myself. My guess is that there are plenty of other academics out there with a similar mindset. However, while such a predisposition might be advantageous to our employers, it exposes us to the risk that we take a failure in our research work as a sign of personal deficiency. How could we reduce that risk, considering that one's personality is rather difficult to change? I can only offer a very practical piece of advice that I remember reading somewhere: make sure to diversify your interests to include more than just work or, in more colloquial terms, 'get a life'. I

have cautiously experimented with this counsel already, by reconnecting with an old friend, and am happy to state that it actually seems to work.

Second, I think the case illustrates the challenges involved in attempting to achieve different types of research aims. Trying to produce a paper that is simultaneously theoretically innovative, societally relevant and publishable, requires a certain degree of jugglery, especially as the three balls in the air are not of equal weight. For instance, I would argue that very often publishability concerns trump genuine intellectual curiosity. Problematising the underlying assumptions of a body of knowledge may well generate theoretically interesting research questions (Alvesson & Sandberg, 2011) but that alone is not enough. At least seasoned researchers know to anticipate the 'So what?' question from the part of colleagues, editors and reviewers looking for a statement of (incremental) contribution. In our case, this happened twice; asking ourselves that question we were not able to provide any other reason for our study except that it piqued our interest. But was that an intellectual dead end? Or had we just imbibed the publishability mentality so thoroughly that we blocked our own publication path before even embarking on it? I really do not know.

In addition to theoretical originality, we also wanted our study to be societally relevant. I am very much inspired by New Zealanders' idea of the university as a conscience and critic of society.² To me, it is an evocative expression of a scholar's duty to undertake research that matters (Flyvbjerg, 2001), for instance by drawing attention to an injustice or proposing ways to redress one. In this respect, part of the problem in the case at hand was that the main injustices I had wanted to highlight with respect to the animal industry had already been communicated in the two previous articles we had published on the topic. This resulted in the feeling that the societal relevance of Animal Paper 3 was not strong enough, that there was nothing new to communicate to either academic or extra-academic audiences (see Gendron, 2013; Burawoy, 2005). Moreover, producing a study with zero emancipatory effects was out of the question as it would have meant exploiting the animals' suffering and the activists' valiant efforts just for the sake of another publication. Many *SEAJ* readers, and critical scholars more broadly, have probably encountered similar problems.

Third, audience members' reactions of relief after my conference presentation appear to me as a reflection of the enormous pressures exerted by academic performance management systems. In many universities, it really is a question of publishing or perishing; shelving a paper and labelling it as a sunk cost is simply not an option. Moreover, it is so easy to jump on the bandwagon. Who of us has not publicly celebrated an academic performer (Gendron, 2008) efficiently churning out publication after publication? At least I am guilty as charged, although the irony is obvious. The more we bend over backwards to meet and exceed the performance goals imposed on us, the less time we have for meaningful reflection and development of ideas. This is not to say that slowing down automatically produces 'better' research, but it might increase the possibility of scholars coming up with original ideas, perhaps even yield solutions to the most pressing problems of the day (see Stengers, 2018).

On a related note, many scholars seem to be under the mistaken impression that having to change course during a study is an exception, a sign of intellectual inferiority that must be concealed from others. This is a testament to the necessity of (especially senior) academics occasionally sharing the myriad setbacks they sometimes encounter

during the research journey, exposing the messy backstage and not just the smooth, well-rehearsed frontstage (Goffman, 1959).

In terms of practical considerations, I fully acknowledge that my fortunate situation made it easier for me to accept the intellectual dead end. A permanent position as full professor is a soft cushion to fall back on. At the same time, I must also note that our university management 'encourages' everyone to publish an average of two high-quality papers per year. I have been able to live up to these expectations due to having several papers in various stages of the 'pipeline'. Accepting the dead end, at least for a while, freed up time and brain capacity to develop some of these other manuscripts into publications and thus to keep our management happy. Again, diversification offers itself as a strategy – getting stuck with a paper is less drastic if you have at least a couple of others to work on.

Hopefully this commentary has come at least halfway in achieving its aims and will serve to initiate discussion and debate on the topic of intellectual dead end. As a final thought, I would like to emphasise that articles are only a means to an end and focusing too much on publishability may get one sidetracked from the larger goal of engendering societal change. As Bruno Latour so aptly puts it: 'What should have been a means may become an end, at least for a while, or maybe a maze, in which we are lost forever' (Latour, 1999, 191).

Notes

1. This is probably due to the CSEAR community's foundational principles of inclusiveness and open-mindedness; the confessional might have been less sympathetically received elsewhere.
2. As defined in Section 162 of the Education Act (1989) of New Zealand. I thank Jesse Dillard for first bringing this to my attention.

Acknowledgements

The author would like to thank the SEAJ Editors, Michelle Rodrigue and Helen Tregidga, for inviting to write this piece and for posing questions to stimulate my thinking. The author would also like to thank co-author Matias Laine for encouragement and support. The author gratefully acknowledges the financial support received from the Academy of Finland, research grant 32421.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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