Title: Methodology or Muse? A Response to the Commentaries

Wordcount: 2472

I'm grateful to the authors for their comments, and for the opportunity to respond. To clarify my approach, I sought to discuss the centrality of 'set and setting' at descriptive and normative levels in the Psychedelic Renaissance by focusing on the language of containment prevalent during my ethnographic research. Inspired by the study of material culture, I took up certain questions that can be asked of any container – how they are constructed and maintained, how leaks and overflows are dealt with, the work done by their boundaries, and relationship of these containers to their contents. There are undoubtedly more questions but these are the ones I found fruitful to think with. My goal was to explore how the concerns over set and setting, and containment, are themselves remaking psychedelic practices and discourses, both inside and outside of clinical trials and in relation to both therapeutics and calls for social justice.

I agree with Earp and Yaden that the existence of a 'psychedelic community' needs careful unpacking, though am less convinced that the term as I have used it is incoherent. Of course, asking what a community is and how we might come to know it is commonplace in the social sciences. I do not know of attempts to map out the contemporary North American and/or UK psychedelic scenes, including the strength of the weak ties (Granovetter, 1973) connecting seemingly-disparate clusters such as overground research groups, local community networks and other demarcated constituencies in attendance at psychedelic events both offline and online. My use of the 'psychedelic community' is emic vocabulary, used by my interlocutors during the period of my research. I do not use the term to claim that the psychedelic community is clearly defined or uncontested (on the contrary; see also Noorani, 2020), but to refer, *inter alia*, to the movement of actors, knowledges, resources, and affections from the clinic to a wider underground and back again.

Earp and Yaden suggest distinguishing between a larger 'community of interest' (my summary) gathering at psychedelic events I attended, and a nested psychedelic research community. We might equally find 'community' precisely where there are fraught conversations amongst actors over whether they form a community or not, who is 'in' and who is (or should be) 'out', as well as the feeling of being betrayed by peers who are deemed to know better. During my fieldwork these contestations were prevalent and often organized around appeals by the overground research vanguard to the wider audience to 'stay the course' and support medicalization as a thin-end-of-the-wedge tactic. If we accept that its identity is contested, how much then is this 'psychedelic community' an imagined community (Anderson, 1983), perhaps one hinging on (unevenly) shared histories of countercultural activity, prohibition, stigma, and the desire for redemption? And could we tease out distinct sub-communities, as themselves 'communities of practice' (Wenger, 1988), 'communities of interest' and more? I think these remain good questions for social scientists interested in the Psychedelic Renaissance.

I appreciate the commentators are wary of the 'hermeneutics of suspicion' (Ricoeur, 2008[1970]) that shape critical thinking and can produce a kind of conspiratorial mode of thinking. But let's not throw the baby out with the bathwater. The characterization of scientific trials as where everything is controlled and contained, versus the local communities as where all hell could run loose, helps to understanding the predicament the research teams have faced (*cf*. Giffort, 2020). Yet when the scientists and participants live in proximity,

maintaining the perception of separation requires work. I was thrown by Earp and Yaden's claim that ""extra-clinical" connections are not concretely described". My examples in the section 'Leaks and Overflows' are organized into four categories: the enthusiasm of former participants who have become advocates for psychedelic therapy; the emergence of psychedelic societies with connections to the clinical research(ers); friendships and wider community networks built through and around the trials; the extra-curricular and sometimes-evangelical work of clinical researchers and therapists. I insist that these are phenomena that deserve attention.

While I would agree with Lewis that science can be "pretty good at figuring" these phenomena out, clinical trial research questions and data analyses – even those underpinning the long-term follow-up that Lewis cites – do not probe how participants are differently emplaced within community support structures, nor the role of the larger sense of purpose conferred through proselytizing in the name of psychedelic-assisted therapy (PAT). Indeed, given the privileged demographics of participants of PAT trials in general, and the shared (and according to my argument, consolidating) community bonds of those coming to trials through snowball sampling, much of the data generated from the revival of research is borne of participants with relatively generous resources for making sense out of their psychedelic experiences and their sequelae. A more polemical way of putting this is that greater attention needs to be given to psychedelics' 'side effect' profiles, leading to the generation of chemo-socialities (Shapiro and Kirksey, 2017) in more direct and rapid ways than (say) the use of SSRIs.

With regards to their description of snowball sampling, the crucial point that I am grateful to Earp and Yaden for (perhaps unwittingly) bringing to the fore is how scientific research practices code social acts as being purely about data extraction and knowledge generation. What Earp and Yaden miss, which the case of PAT research usefully showcases, is that snowball sampling with psychedelics may be apt to, well, snowball, in unexpected directions, because the experiences are so interesting, so seemingly transformative, and so stigmatized. Snowball sampling works through networks, and my argument with regards to recruitment for PAT trials was that as network links are remade, they are consolidated, with knock-on effects. Again, such overflows are not a bad thing – for anyone with an interest in mental healthcare, spirituality or drug (ab)use, drivers of a greater societal literacy with a diversity of states of consciousness may be welcome indeed. But depicting the use of snowball sampling as a mere technical matter for science repeats a more fundamental rhetorical move – one Lewis seems content to double down on – that 'science' and 'society' are somehow separate spheres.

In another example, Lewis suggests that it is simpler to say that serotonergic system stimulation has direct neurobiological effects on relative sensitivity to environmental stimuli, than it is to say that emphasis on the 'set and setting' of psychedelic use grounds claims as to how they should be used. I do not agree. These are explanations on different scales, and their compatibility is what has enabled the progression of psychedelic research into the era of pharmaceuticalization. Once again, I am not proposing some 'grand lie' narrative here. Consider the relative paucity of research investigating set and setting variables – shouldn't this suggest researchers remain agnostic on the use of psychedelics outside of the clinic? (And some do.) Compounding the problem, Lewis may be too narrowly construing the emphasis on a proper set and setting as something advocated for by psychedelic researchers alone, when media reporting, statements from regulators and politicians, and the declarations of drug development companies and other psychedelic industry players as to what is and is

not safe are all contributing to the emphasis on set and setting. This would have been useful to have clarified in my article.

Far from suggesting conspiracies in play, I argued that managing overflows and leaks has been a concern for clinical trials researchers, and understandably so given the sensitivity of the research field to critique and the possibility of a 'backlash' (see Rucker and Young, 2021 for a recent articulation of the concern with a backlash). Perception management is consistent with Giffort's (2020: 126-128; 145-151) descriptions of self- and other-policing within the psychedelic science research teams with which she conducted interviews. How should we interpret this? In light of the commentaries, I wish I had emphasized that the leaks and overflows I pointed to index not only alternative ways of viewing efficacy (which I do note) but the deep care that is inextricable from the work of the PAT researchers I have come to know and admire. They are navigating treacherous double standards, feeling profound responsibility towards – and perhaps even kinship with – research participants, while being beholden to ethical and legal stipulations that circumscribe their contact, as well as a more general climate of researching stigmatized drugs. We might even see them as "moral pioneers" (Rapp, 2000), trying to be conscientious in the face of bureaucratic demands borne of abstract bioethical principles that can sometimes get in the way.

Elsewhere, Lewis explains that "Noorani is telling a story about the motives for their actions that psychedelic researchers themselves may not recognize," but it is tempting to suggest that it is Lewis who sees too much intentionality. "Sinister" intentions at that. Take the CCTV camera – much in line with Lewis' commentary, I have heard researchers remark that they are not exactly delighted to have monitoring equipment in view but recognize the practical need for it. I did not intend to suggest Orwellian implications, but to consider its potential effects. We might add in relation to the dialectics of containment that the CCTV footage has value beyond its potential symbolic efficacy for participants. It offers a record of events in the room (as reliable and material witness), while itself exceeding the container, valuable as data to be coded, analyzed and even used in training. In short, faced with the options Lewis offers – "a frankly Orwellian impulse" or an "elaborate performance of medical and scientific legitimacy" – I would opt for the latter though with two caveats: a) not all valid analyses are reducible to actors' prior intentions, and b) performances have real effects.

We do need to be thinking harder about the spaces we create and condone for PAT. This underpinned my all-too-short exploration of the history of the living room-like setting. This may have been ill-judged, and I confess I was drawn to adding a little historical color when referring to a previous incarnation of the living room as the 'death room' of Victorian times. The commentators seem irked by my abbreviated history, which was not intended to create a "grim" picture but to encourage denaturalizing this staple phrase within the emerging psychedelic research doxa. Despite this, it is unclear to me why the idiosyncratic history of the living room-like setting is separate from the cultural competence issues that Earp and Yaden suggest are of greater concern. Who identifies the living room with comfort, safety and quietude? Whom might the language of 'living room', or the furniture and decorations used, alienate? For whom is the living room primarily a site of servitude or even conflict? Anticipating Lewis' charge of apophenia, I understand that the term living room-like setting may function as a code for researchers to be communicating, in the language that many of them share, the kind of environments that they are looking to create for their participants and patients, but again, would remind Lewis that the idea of the living room-like setting is part of public-facing account of PAT, the burgeoning retreat industry, and beyond.

Earp and Yaden end by pointing out that anthropologists can play a crucial role in cross-cultural research (and we are safe to assume they are not referring to the cultures of different labs!). I agree that the value of social scientific inquiry needs to be better understood in relation to the Psychedelic Renaissance. I remember a conversation with a prominent psychedelic researcher in 2015, who encouraged me to think of ethnographic research in terms of generating powerful descriptions of PAT for use as media soundbites. More's the pity. Alongside the classic role of studying the cultural 'other' to 'the West' or 'science' (whatever that means), there are growing opportunities for the ethnographer to be hired as a kind of technician to be placed in charge of the 'culture' variable in interdisciplinary collaboration. In my experience, this can require assimilating to the overarching discourses produced by the psychedelic researchers, while tweaking medical or pharmacological knowledge claims here and there.

Yet for four decades anthropology has also been offering important contributions to science studies (Latour 1993), challenging the assumptions that the scientific lab is a culture-neutral place. I see my article attempting to contribute something here, by articulating the cares and concerns of the Psychedelic Renaissance in such a way that another conceptual constellation comes to the fore, capable of reframing what is at stake. It is understandable that Earp and Yaden would identify this as "poetic" insofar as it crosses modalities and jumps scales. We might say that one methodology is another's muse. For instance, Earp and Yaden note issues of cultural bias and the need for diverse stakeholders, but this is too quickly framed in terms of user choice and the issue of "cultural expectations" in the roll-out of PAT. I would encourage them to expand their purview to include the facts that are being produced and the kinds of science and knowledge that is considered legitimate. It was with the whiteness of PAT in mind that I sought to highlight protection, exclusion, and refusal as important aspects of the session rooms.

As Lewis rightly points out, the terrain is fast-changing. With growing optimism and confidence surrounding the legitimation of psychedelic use, there is more openness of connections traversing clinical and community sites, alongside more admissions of personal psychedelic use. Careful (and care-filled) interdisciplinary inquiry into the dynamics of disavowal at these sensitive intersections could teach us about the stigma and taboo, the relationship between the effacement of community connections and critiques of appropriation, and the experiential sequelae of prohibition and the war on drugs. We might ask how such connections and admissions are folded into science-as-normal, for example under the rubric of sampling techniques, or the way that disclosing personal experience of psychedelics can actually legitimise rather than compromise the expertise of privileged actors (for a recent example, see Lieberman, 2021). The irony here, which is hard to overstate, is that these researchers themselves provide one source of expertise (often described as 'lived experience') for these investigations based on their collective analyses (for example, see Garcia-Romeu, 2016). I look forward to such interdisciplinary dialogue, both with and about psychedelic science, while accepting that even as the psychedelic industry is rapidly growing into a multi-billion dollar sector, some psychedelic scientists maintain that it remains too risky to speak about such topics.

Ultimately I agree with Lewis that the search for meaning here be guided by its "fruits". For Lewis these are "practical handles for explanation and therapeutic intervention". I would broaden the scope to include interventions at social, cultural, political and regulatory levels, and also aesthetic criteria – those meanings that are interesting or inspire further consideration. For this we may need to build new 'communities of inquiry' (Lipman, 1991)

invested in both disciplinary *and* interdisciplinary research. I hope that this exchange contains some considerations towards that end.

Acknowledgements: Thanks to Abou Farman, Rachel Jane Liebert, Thomas Osborne, Emilia Sanabria and Bob Simpson for conversations that shaped this response.

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