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What psychedelic research can, and cannot, tell us about consciousness

15–19 minutes



The short piece below first appeared in *Scientific American* ([Observations](#)) on October 26, 2018. It is a coauthored piece, led by me with contributions from Michael Schartner, Enzo Tagliazucchi, Suresh Muthukumaraswamy, Robin Carhart-Harris, and Adam Barrett. Since its appearance, both Dr. Kastrup and Prof. Kelly have responded. I attach links to their replies after our article, offering a few comments in further response (entirely my own point of view). These comments just offer additional clarifications – I stand fully by everything said in our *Sci Am* piece.

It's not easy to strike the right balance when taking new scientific findings to a wider audience. In a recent [opinion piece](#), Bernardo Kastrup and Edward F. Kelly point out that media reporting can fuel misleading interpretations through oversimplification, sometimes abetted by the scientists themselves. Media misinterpretations can be particularly contagious for research areas likely to pique public interest—such as the exciting new investigations of the brain basis of altered conscious experience induced by psychedelic drugs.

Unfortunately, Kastrup and Kelly fall foul of their own critique by misconstruing and oversimplifying the details of the studies they discuss. This leads them towards an anti-materialistic view of consciousness that has nothing to do with the details of the experimental studies—ours or others.

Take, for example, their discussion of our [recent study](#) reporting increased neuronal “signal diversity” in the psychedelic state. In this study, we used “Lempel-Ziv” complexity—a standard algorithm used to compress data files—to measure the diversity of brain signals recorded using magnetoencephalography (MEG). Diversity in this sense is related to, though not entirely equivalent to, “randomness.” The data showed widespread increased neuronal signal diversity for three different psychedelics (LSD, psilocybin and ketamine), when compared to a placebo baseline. This was a striking result since [previous studies](#) using this measure had only [reported reductions](#) in signal diversity, in global states generally thought to mark “decreases” in consciousness, such as (non-REM) sleep and anesthesia.

Media reporting of this finding led to headlines such as “First evidence found that LSD produces ‘higher’ levels of consciousness” (*The Independent*, April 19, 2017)—playing on

an ambiguity between cultural and scientific interpretations of “higher”—and generating just the kind of confusion that Kastrup and Kelly rightly identify as unhelpful.

Unfortunately, Kastrup and Kelly then depart from the details in misleading ways. They suggest that the changes in signal diversity we found are “small,” when it is not magnitude but statistical significance and effect size that matters. Moreover, even small changes to brain dynamics can have [large effects on consciousness](#). And when they compare the changes reported in psychedelic states with those found in sleep and anesthesia, they neglect the important fact that these analyses were conducted on different data types (intracranial data and scalp-level EEG respectively—compared to source-localized MEG for the psychedelic data)—making quantitative comparisons very difficult.

Having set up the notion that the changes we observed were “small,” they then say, “To suggest that brain activity randomness explains psychedelic experiences seems inconsistent with the fact that these experiences can be highly structured and meaningful.” However, neither we nor others claim that “brain activity randomness” *explains* psychedelic experiences. Our finding of increased signal diversity is part of a larger mission to account for aspects of conscious experience in terms of physiological processes. In our view, higher signal diversity indicates a larger repertoire of physical brain states that very plausibly underpin specific aspects of psychedelic experience, such as a blending of the senses, dissolution of the “ego,” and hyper-animated imagination. As standard functional networks dissolve and reorganize, so too might our perceptual structuring of the world and self.

“In short, a formidable chasm still yawns between the

extraordinary richness of psychedelic experiences and the modest alterations in brain activity patterns so far observed.” Here, their misrepresentations are again exposed. To call the alterations modest is to misread the statistics. To claim a “formidable chasm” is to misunderstand the incremental nature of consciousness research (and experimental research generally), to sideline the constraints and subtleties of the relevant analyses and to ignore the insights into psychedelic experience that such analyses provide.

Kastrup and Kelly’s final move is to take this presumed chasm as motivation for questioning “materialist” views, held by most neuroscientists, according to which conscious experiences—and mental states in general—are underpinned by brain states. Our study, like all other studies that explore relations between experiential states and brain states (whether about psychedelics or not), is entirely irrelevant to this metaphysical question.

These are not the only inaccuracies in the piece that deserve redress. For example, their suggestion that decreased “brain activity” is one of the more reliable findings of psychedelic research is incorrect. Aside from the well-known stimulatory effects of psychedelics on the excitatory glutamate system, early reports of decreased brain blood flow under psilocybin have not been well replicated: a subsequent study by the same team using a different protocol and drug kinetics (intravenous LSD) found only [modest increases in brain blood flow](#) confined to the visual cortex. In contrast, more informative dynamic measures have revealed more consistent findings, with network disintegration, increases in global connectivity and increased signal diversity/entropy appearing to be particularly reliable outcomes, replicated across studies and study teams.

Consciousness science remains a fragile business, poised precariously between grand ambition, conflicting philosophical worldviews, immediate personal relevance and the messy reality of empirical research. Psychedelic research in particular has its own awkward cultural and historical baggage. Against this background, it's important to take empirical advances for what they are: yardsticks of iterative, self-correcting progress.

This research is providing a unique window onto mappings between mechanism and phenomenology, but we are just beginning to scratch the surface. At the same time—and perhaps more importantly—psychedelic research is demonstrating an exciting potential for clinical use, for example in [alleviating depression](#), though larger and more rigorous studies are needed to confirm and contextualize the promising early findings.

Kastrup and Kelly are right to guard against overplaying empirical findings by the media. But by misrepresenting the explanatory reach of our findings in order to motivate metaphysical discussions irrelevant to our study, they risk undermining the hard-won legitimacy of a [neuroscience of consciousness](#). Empirical consciousness science, based firmly on materialistic assumptions, is doing just fine. And unlike alternative perspectives that place themselves “[beyond physicalism](#),” it will continue to shed light on one of our deepest mysteries through rigorous application of the scientific method.

You can read Dr. Kastrup's response [here](#), and Prof. Kelly's [here](#). In the spirit of constructive clarification I will offer a few additional comments on the parts of the work I was involved in: the signal diversity study and the general interpretation of how empirical work on the brain basis of psychedelic research

speaks to metaphysical debates about the nature of consciousness. These comments relate mainly to Prof. Kelly's critique.

(With respect to [Dr. Kastrup's comments](#) I will simply offer, as he no doubt knows, that relating fMRI BOLD to neural activity, in terms of global baseline and regionally differentiated metabolics, functional neuronal connectivity, and so on – remains an area of extremely active research and rapid methodological innovation.)

1. Prof Kelly notes that we do not provide 'exact N's for the data segments we used to compute measures of signal diversity.

This is because they varied substantially between drug condition, participant, and analysis method. We do however clearly state that “[a]nalyses were performed using non-overlapping segments of length 2 sec for a total length between 2 min and 10 min of MEG recording per participant and state”

([Schartner et al](#) 2017, p.5)” These numbers indeed lead to a total number of segments ranging from ~3,500 to ~27,000 per participant and per state (since we have 90 channels/sources per segment). These large numbers provide stable statistical inference (e.g., by the central limit theorem). Also, as we mentioned (above) the absolute scores on the diversity scale are not as meaningful as effect size and statistical significance. I'd also like to add that in our paper we go to great lengths to establish that our reported diversity changes do not trivially follow from well-known spectral changes in the drug conditions – this is part of the unavoidable computational sophistication of the method, when done properly.

2. When Prof. Kelly says that “relatively simple neuroimaging methods can easily distinguish between wakeful and drowsy states and other commonplace conditions” I do not disagree at all. Our paper was specifically interested in signal diversity as a

metric of brain dynamics (and as mentioned above we take care to de-confound our diversity results from spectral changes). Also, we do not claim these diversity changes fully explain the extraordinary phenomenology of psychedelic states. However, I do believe that they contribute helpfully to the incremental empirical project of mapping, in explanatorily satisfying ways, between mechanism and phenomenology. I defend the general approach in this 2016 *Aeon* article: [‘the “real” problem of consciousness’](#).

3. I also agree the measures of signal diversity we apply are only part of the story when mapping between experiential richness and brain dynamics. My lab (and others too) have have worked hard on developing empirically adequate measures of ‘neural complexity’, ‘causal density’, and ‘integrated information’ which are theoretically richer – but unfortunately, at least so far, not very robust when applied to actual data – and are substantially more computationally sophisticated. See here for a recent [preprint](#). We have to do what we can with the measures we have, while always striving to generate and validate better measures.

4. I do not buy the claim that near-death-experiences provide an empirical challenge to physicalist neuroscience (as argued by Prof. Kelly). See my previous blog post on this issue ([‘the brain’s last hurrah’](#)).

5. No need to impute me with a bias towards physicalism! I explicitly and happily adopt physicalism as a pragmatic metaphysics for pursuing a (neuro)science of consciousness. I can do this while remaining agnostic about the actual ontological status of consciousness. The problem with many alternative metaphysics – in my view – is that [they do not lead to testable propositions](#). Dr Kastrup and Prof Kelly are of course entirely

entitled to their own metaphysics. I was merely objecting to their usage of our psychedelic research in support of their metaphysics, because I think it is entirely irrelevant. I simply do not accept that there are any “evident tensions between physicalist expectations and the experimental results [from psychedelic neuroimaging]”.

6. Finally, we can hopefully all agree on the importance of forestalling, as far as possible, media misinterpretations. This is true whatever one’s metaphysics. And it’s why, when our diversity paper first appeared, I felt compelled to pen an immediate corrective right here in this blog ([‘Evidence for a higher state of consciousness? Sort of’](#)).

After posting, I realized I had not specifically responded to Bernardo’s [initial reaction](#) to our *Sci Am* piece. There is some overlap with the above points, but please anyway allow me to correct this oversight here.

1. *Clearing the semantic fog.* I hope I have made clear my intended distinction between ‘fully explain’ and ‘incrementally account for.’ Again my [Aeon piece](#) elaborates the strategy of refining explanatory mappings between mechanism and phenomenology.

2. *Metaphysical claims.* Our work is consistent with materialism and is motivated by it, but empirical studies like this are not suited to arbitrate between competing metaphysical positions (unless such positions state that there are no relations at all between brains and conscious experiences). Empirical studies like ours try to account for phenomenological properties in terms of mechanisms – but in doing so there is no need to make claims that one is addressing the (metaphysical) ‘hard problem’ of consciousness. [Kastrup and Kelly have written](#) that “the