Review of: "Collective Pareidolia"

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This is a review of preprint V1, Collective Pareidolia, by Robert Bednarik

The paper describes and discusses a case where a director of a university college discovered thousands of rock art motifs, then engaged a large team of staff and students to make recordings of these petroglyphs that were, on later investigation by independent experts, found to be non-existent. The main strength of this paper is that it is novel and interesting because it proposes an instance of real-world face pareidolia in the context of the academic study of rock art. More often, pareidolia is discussed in the context of the non-academic, for example, people who believe in ghosts perceiving non-existent 'ghostly' faces or figures in ambiguous visual stimuli, or religious believers perceiving the face of their deity in random visual stimuli. Was the current case an example of what happens when you have academic non-experts on the one hand (the director?) and academic experts on the other hand (the three independent specialists brought in to verify the claimed petroglyphs)? The paper might be stronger if it explores the above theme.

Currently, I do not think the paper makes a persuasive case for its central claim that there is something surprising or qualitatively different about 'collective pareidolia' compared to individual pareidolia. We know that (for instance) photographs of ghostly pareidolias and religious pareidolias are widely shared online and appear to be similarly perceived by others (or they would not be shared), particularly when the viewer is told where to look and what to look for.

I feel that there are currently three main areas where further work would considerably strengthen the paper. First, the paper makes repeated reference to neuroscience; however, it is not clear why discussion of the neural mechanisms underlying pareidolia is relevant. Second, the paper does not provide evidence to support its conclusion that the operators are "experiencing the same sensory pattern without communicating about it" (page 7). Third, the paper contains other unsupported assertions about the methods and conclusions of the three independent specialists that are quite central to the integrity of its claims. I expand on these points below.

The paper states (page 2), "In individual pareidolia, we essentially recognise what our visual system causes us to expect to see". There is then discussion of visual pathways to conscious recognition, including "visual cortex, thalamic reticular nuclei, pretectum, superior colliculus and local LGN interneurons". The paper seems to be asserting that it is these pathways that cause individual expectancies that are then expressed through pareidolias/misperceptions. It is likely that most controlled laboratory-based studies of pareidolia are, for pragmatic reasons, studies of individual participants. However, in an uncontrolled real-world setting, such as in the case described in the paper, shared expectations and combined social and cultural influences cannot be excluded. Indeed, there is likely to be a power dynamic at play in this

setting, with the staff and students seeking to please the director who originally discovered the petroglyphs, with everyone aware that huge effort and hundreds of thousands of dollars had been spent on moving hundreds of large blocks of granite, and with all parties excited by the possibility of UNESCO World Heritage status. Individual expectations (of what a face looks like, of what a petroglyph looks like) are combined with social factors, some powerful and obvious such as those just described, and some more subtle (see my second point below).

Perhaps the paper's references to concepts from neuroscience are an attempt to bolster the argument that collective pareidolia is surprising, as the paper's argument appears to be based on the premise that the visual system causes individual pareidolia (page 2) but "Shared pareidolia...is more difficult to account for." (page 2). This reviewer feels that the Introduction's emphasis on neuroscience is a distraction, and the paper would be stronger if the Introduction dropped this in favour of consideration of social, cultural, and psychological factors likely to influence expectations and therefore the shared perception of pareidolias. This could reference relevant research, such as that reviewed by Zhou & Meng (2020) in the *Journal of Pacific Rim Psychology*.

Second, to make the case that there exists a surprising collective pareidolia, where individual expectancies turn out to match one another in the form of shared perceptions of non-existent faces, the paper asserts that there is "no verbal communication" (page 5) between the two operators who are jointly creating a 'recording' of a petroglyph. In the Conclusion, this becomes "without communicating" (page 7). This claimed lack of communication seems to be key to the paper's assumption that 'collective pareidolia' is different from individual pareidolia. However, clearly the 'empirical evidence' (page 2) described in the paper (page 5, and as seen in the photograph on page 6) shows that the two operators are working together at the same time. So they can see one another's work, and of course already have shared expectancies of what a face looks like. It is also not clear that the earlier recordings (made by the operators when no experts were observing them) were made *without* any verbal communication by the operators, and were made without having seen the petroglyph images previously identified by the director of the research centre. One way to rule out these shared non-verbal communications and prior shared experiences, and thereby to support the claim that it is "astonishing" that the operators "experienced similar, if not identical, designs" (page 6), would be to have one operator record one half of a new rock (one that had not previously been identified by the director as containing a petroglyph), then have another operator record the other half of the rock without seeing the recording made by the first operator and without having any contact with the first operator or an observer of the first operator. In other words, "masked" or "double-blind" methods would be needed.

The third way in which the paper could be strengthened is to provide support for the assertions that are made about the observations of the three specialists who were invited to confirm the existence and age of the petroglyphs. These include (my parentheses):

"The specialists agreed that it represented an extraordinary find" (page 3)

"despite all efforts, they (the specialists) could not detect any of the dozens of motifs they were shown" (page 4)

The specialists' examination found "it was entirely clear that their (the operators') lines were not following any real grooves

or depressions on the rock" (page 5)

A number of questions come to mind: Who were these specialists? Why are they not co-authors on the paper? How can we know that the depiction in this paper of their methods and conclusions is accurate? Did the specialists write or publish a report of their work, and if so, why is it not referenced? Linked to this point, the author interestingly describes experiencing these non-existent petroglyph grooves (page 6). This comes late in the paper and is a surprise to the reader. What was the author's role in this project? If the author is one of the specialists who could find no petroglyphs, it would be interesting to hear more about his experience and why the three specialists were more ‘immune’ to the pareidolias than the original team.