Review of: "Growing Confidence and Remaining Uncertainty About Animal Consciousness"

Alvaro Julian Moreno Bergareche[1]

1 Universidad del País Vasco

Potential competing interests: No potential competing interests to declare.

REVIEW OF THE PAPER:

Growing Confidence and Remaining Uncertainty About

Animal Consciousness

This paper is well written and shows that the author knows the literature about the question well. The aim of the paper is also well defined and interesting.

Below, I will review some of the points that, in my opinion, present problems.

1. The author says explicitly that he agrees with the 4E approach (p 4), but he focuses only on the structure of the nervous system (analyzing and comparing the types of brains of different animal taxa) and ignores all along the paper the relation between the brain and the body in each case. What can be inferred from this is that for him, this relationship is not relevant to explain the emergence and evolution of consciousness.

In p 8, for example, the author says that “Different theories of consciousness propose different mechanisms for raising the primary NCCs to conscious awareness (…) All three were proposed in the context of mammalian (and largely human) neuroanatomy, but their characteristics are generalizable enough to be applicable to *any animal* with a sufficiently complex nervous system” (the italics are mine). This is, in my view, a very hasty conclusion because it neglects the highly specific relation between the brain and body of the mammal class (and probably the whole vertebrate phylum), focusing exclusively on the neural/brain structure. And this is surprising, given that the author himself seems to claim an embodied view of the brain in p 4. It is significant that the author seems to reduce the neurophysiological domain to “the bioelectrical and neurochemical dynamics of the relevant circuits” (p 9). Now, if one thing is clear from what we know of human and mammal C, it is that it involves (and requires) a continuous, fine-tuned, and highly integrative control of the body.

1. In p 5, the author defends the idea that “consciousness is an evolved product of complex brains in complex bodies, so it is an emergent feature of a complex physical system (…) animals with sensory hierarchies can be conscious if they have four or more levels of neurons projecting to (and including) the highest processing area, or in some cases, just three levels (because in this type of hierarchical structure) … neural circuits combine diverse details and types of sensory input into one unified experience”. Yet, this argument sounds vague: how does this hierarchy work to generate a globally unified sense of self? How is this process related to the physiology of the animal? Which –if any— is the role

of the radical architectural difference between vertebrate and invertebrate brains and –also- the different ways these brains are embodied? And in the different taxa that share the same number of neural levels, which –if any—is the role of the enormous differences in the size and the type of organization of the brain? None of these important questions is addressed.

2. In p 6, the author says that “Subsequent evolution relocated details of sensory perception and motor control to higher brain centers, now considered a neuroanatomical necessity for animals that perceive qualia at a high level of resolution and manage motion with fine motor control”. Yet, there is not a detailed explanation of what is specifically meant by a “high” sensorimotor control center. The majority of nervous systems have some form of sensorimotor control center, so terms like “high”, when presented as a fundamental substrate for consciousness (henceforth, C), need to be carefully explained.
3. Another problem is how the author approaches the evolutionary dimension of C in animals. If in different animal taxa the evolutionary history of C has been so different in terms of complexification and diversification of C, why not discuss and analyze this question? For example, the evolutionary history of amniotes has shown a very rich diversification in the forms and degrees of complexity of C, whereas that of coleoid cephalopods has shown a comparatively poor diversification and complexification. After all, comparing the diverse evolutionary paths of complexification and diversification of C in the different taxa is easy, and it is reasonable to hypothesize that there should be a relation between a specific form of neurophysiological structure and its evolutionary unfolding. If during hundreds of millions of years nothing qualitatively relevant happens in terms of complexification of C in a given phylum, while in another phylum the evolutionary history shows a powerful process of complexification of C, one can suspect that some important neurophysiological difference exists ab initio. And this testable fact could be a cue that relevant neurophysiological differences concerning C in the different taxa or phyla. Although the author deals specifically with the question of the evolution of C in pp 6-7 (SECTION: “The Evolutionary Imperative”), he does not address this important point. In fact, what he does is just to mention the different authors that claim the existence of C in certain groups of arthropods, cephalopods, and lower vertebrates.
4. It is surprising that, after having presented a whole set of arguments to show that there is a growing consensus in identifying a certain neurological structure as the basis of C, the author admits that much more work is necessary to clarify this question. As he says, “the gap between the phenomenological experience of consciousness and its neurological correlates remains controversial.” If so, how can it be said that “but plausible explanations relating mechanism to phenomenology are arising as the neuroscientific study of consciousness matures”? These different statements seem somewhat contradictory.
5. Another important question that the author seems to neglect is the different meanings of the term C in the literature of animal C. Actually, there is an increasing association of a minimal form of “sentience” with the existence of a “subjective experience”. Hence, it is important to discuss the distinction between C as a cognitive representational phenomenon, and (a much simpler concept of) C, as just a feeling of pain. As said, i*n* the recent literature about animal C, there is an increasing tendency to use the term “sentience” for referring to the most basic form of C (see, for example, Damasio 1999; Denton et al. 2009; Godfrey-Smith 2016, Birch et al. 2021, to cite but a few). An example of basic sentience would be what Denton and colleagues (op cit) call "primordial emotions" – bodily feelings which

register important metabolic conditions such as thirst, the need for salt, or the feeling of not having enough air; or pain, which for Godfrey-Smith (op cit) is perhaps the best case for motivating a divide between a broad sense of subjective experience and full-blown C. This last sense corresponds essentially to the concept of C presented in the list of seven characteristics that, according to Brofman and colleagues (2016), reflect the most accepted use of the term C among neurologists, cognitive psychologists, and philosophers. The sense used by the author in the 1-5 features of the abstract and in pp 2-4. This difference in the meaning of the term “subjective experience” is important because Brofman and colleagues and the author’s sense of C implies a cognitively more complex dimension than that of basic sentience. For example, Edelman’s (and Dehaene’s) concept of a “scene” implies a representational dimension that is lacking in Denton’s concept of basic sentience. Hence, this second sense of C is more basic and likely evolutionarily more primitive than the former.

Now, this distinction is relevant for the discussion of what is the neurophysiological substrate of C. With the exception of coleoid cephalopods, a review of the literature shows that the more evolutionarily removed from mammals the examples are, the more imprecise and speculative the neurophysiological data supporting evidence for subjective experience (if this last term is understood in a full-blown sense). This is probably one of the reasons why *the term “sentience” is increasingly used when looking for minimal forms of C (in lower vertebrates and/or certain invertebrates)* where it is much more difficult to assess objectively the presence of any form of “representational” subjective experience than in animals evolutionarily closer to us. In any case, this search would probably be less difficult if we understand the concept of minimal C in the less demanding sense of Denton, Godfrey-Smith, and Birch, namely, as synonymous with sentience. As for the case of coleoid cephalopods, the number and detail of studies (both at the neurophysiological and at the behavioral level) supports a growing certainty that these animals can have some kind of subjective experience. But researchers are quite divided about what type of “consciousness” they can have, and more specifically, to what extent they can have a "unified self" (i.e., Carls-Diamante 2022). In fact, the author himself mentions this question.

In sum, it is much more difficult to argue for the existence of this strong sense of C in evolutionarily distant animals (and even worse if they have very small brains).

1. Another surprising fact is the use of phrases at the end of the section that relativize and weaken the main argument developed by the author himself. For example, in p. 9, at the end of the section, the author says that “the applicability of currently proposed NCCs to animals with neural architectures very different from those on which most models are based will remain in question until mechanisms compatible with the diversity of nervous systems over a broader range of the animal kingdom are explored.” If so, why has he said that “their characteristics (of these models) are generalizable enough to be applicable to *any animal* with a sufficiently complex nervous system” just a few paragraphs before (p. 8)? It is really contradictory. And this contradiction concerns his main message (namely, that there is a growing consensus about the empirical distribution of C).

In sum,

Even acknowledging that the article is well written and shows that the author knows the literature, it does not really offer a conclusive argument in support of the message contained in the abstract. In several places, the author takes back with

the one hand what he has given with the other. Hence, I don’t see that it makes a significant contribution to the debate on this important issue. My recommendation is not publication.