Review of: "Phylogenetic Evidence for the Early Origin of the Homeostatic Influence of the Biota on Planetary-Scale Geophysical Processes"

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Potential competing interests: No potential competing interests to declare.

I read the manuscript "Phylogenetic Evidence for the Early Origin of the Homeostatic Influence of the Biota on Planetary-Scale Geophysical Processes" by Mark Leggett, David A. Ball, on the development of James Lovelock's Gaia concept and found it very unusual. Its structure is far from standard, but most incongruous is the absence of a reference list, although the text of the manuscript abounds with references. This is even more ridiculous because the authors themselves write in the section "Data Availability": "All data underlying the study are available at the references cited."

This is the main but not the only major problem with this manuscript.

First, the title "Phylogenetic Evidence for the Early Origin of the Homeostatic Influence of theBiota on Planetary-Scale Geophysical Processes" does not reflect its content, as there is not a single clear evidence in the text. It contains only general reasoning and comparison of different points of view. As for geophysical processes, they are not discussed at all and are not even included in the keywords.

In defining the aims (rather than goals) of their study, the authors state that they are seeking evidence for "i. in an individual modern organism of at least one example of the Gaia core attribute of a control system; and ii. in individual modern organisms, of at least one example of a group of organisms experiencing common joint action and carrying out such joint action that leads to beneficial results for the group". Aim "i" looks very strange. How can an individual organism be an example of an attribute of Gaia in the conventional, planetary understanding of Gaia? Admittedly, the authors do not define what they mean by the word "Gaia", and it is quite difficult to understand this, as I will show below. The aim “ii” looks no less strange. The phenomenon of common joint actions of living organisms, leading to useful results for the group, has been known for more than a hundred years and has been called symbiosis.

The authors further state that " this study does not purport to present *individual* empirical results or concepts that have not previously been reported. What we seek from the pool of such previously reported empirical results is a *set* of empirical results, that set not having previously been reported as a set, to provide evidence relating to a particular hypothesis." In other words, they characterize their manuscript as a review of published work. Not to mention that a review without a list of references looks more than strange, the authors contradict themselves when describing their methods in section 2: "we have presented empirical evidence for the existence of separate specific genes contributing to the functioning of examples of each of the two proposed core attributes of Gaia." They do not present any "empirical evidence", referring to other research’s data without any references, forcing the reader to take their word for it.

The central idea of the manuscript is to identify the main attributes of "Gaia," whatever the authors mean by that. The first of these they define as "the existence of a control system". It is difficult to envision any "control system" for "Gaia" as Lovelock originally defined it. Perhaps the term "self-management system" would be a better term for a planetary-scale biological system. However, the manuscript authors do not agree with the planetary attribute for Gaia, even though their Table 1 shows this attribute is the most common attribute in scientific publications for the term Gaia. Moreover, one can read in the Concluding Remarks that "the first successful cell can be seen as a Gaia itself " or "multicellular organisms – has been a combination of Gaias ". In other words, here the authors identify "Gaia" with "cell" without giving any arguments in favor of this strange identification.

The list of such major problems could be continued. In addition, the manuscript contains many minor problems and typos.

However, there is no point in discussing them because the above-mentioned fundamental flaws are in my opinion more than enough to reject the submitted manuscript. Even if the authors decide to redo everything, it will be a completely new text with a new title, and it will have to be reviewed again.