Review of: "Evidence for the Early Origin of Genes Leading to the Development of Biogeochemical Homeostasis at Planetary Scale"

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Potential competing interests: No potential competing interests to declare.

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General remarks about Gaia etc:

This review establishes that the genetic apparatus for various forms of homeostasis has been present throughout the history of the biosphere and plausibly may have had impacts at different spatial and temporal scales. In my view this is an interesting point that is worth making. I am supportive of the broad intention that the authors have in connecting this evidence to the Gaia hypothesis, but I am afraid that some of the initial premises are faulty in this regard.

Specifically, the idea that Gaia can be sub-planetary is not something I can agree with, because it leaves the definition of Gaia so vague as to be indistinguishable from something like “there exists a general tendency for homeostatic feedbacks to sometimes emerge”. Perhaps others might disagree with this position and suggest that homeostasis at the planetary scale is most parsimoniously explained by the spread of homeostasis from sub-planetary scales. There are problems with this, but perhaps they can be dealt with, I’m not sure. But either way, the place to argue that manifestation at the planetary scale is a contingent and not necessary feature of Gaia is in a focused model on this exact subject, not

a review of other issues.

I suggest that the authors restructure this work to be something like “Review of the widespread presence and influence of genetic toolkits for homeostasis across the tree of life”, or similar, leaving the Gaia bit to the end, and making it much more tentative and conditional in nature. Due to this issue with focus/structure I will have to give this draft a low rating. However, I am confident that I should be able to give the final draft a much higher rating if these changes are made. I look forward to taking a look at the next draft.

GENERAL COMMENTS ON GAIA AND NATURAL SELECTION

The problems (or at least the most significant problems) that evolutionary biologists have with Gaia are that:
(1) The planetary biosphere can’t be a unit of selection, nor can the biome, ecosystem or other larger entity without clear heredity.
(2) There is a fundamental separation of scales between the processes that affect organisms and those that affect planetary climatic/geochemical feedbacks, such that what is adaptive at the organism scale might easily have a longer-term or larger scale anti-homeostatic impact at the planetary scale, and this might not feed back on growth/adaptation until too late to avert a collapse of habitability.

The authors mention (1) briefly, but implicitly assume that (2) can be explained away by the fact that there are genes for homeostasis at various scales. Biologists are obviously well-aware that homeostasis and maintenance of chemical disequilibrium with the external

environment is an inherent property of living organisms. But such properties are attributed adaptation at the organism level (or arguably multi-level selection in the quorum sensing example). Thus, the presence of a genetic toolkit for homeostasis in early life provides evidence that you can't have life without organism-level homeostasis, which is uncontroversial. But it doesn't provide evidence for Gaia, in my opinion.

Such evidence is, arguably, provided by homeostasis manifesting at a (near) planetary scale that can't be a unit of selection, and the authors discuss their own interesting work in this regard. But I'm not convinced that homeostatic properties at the latter scale can be causally related to the genetic evidence at lower scales. This is a possibility, but remains an open one, of which more skeptical readers will remain unconvinced, I think best left for future work. There are mechanisms under discussion (by-products, emergence, constraints on adaptation, persistence-based selection on non-replicating entities) to deal with (2), but these issues remain a matter of debate, and I imagine many evolutionary biologists remain skeptical.

For example, suppose a population of organisms (to be generous) have a net homeostatic impact on their immediate external environment/ecosystem. Suppose further (to be more generous) that this external homeostatic effect feeds back positively on organism fitness, such that this organism-environment interaction is self-reinforcing. It is perfectly imaginable that this hypothetical organism-environment interaction has another effect (e.g. releasing a globally well-mixed by product) that has climatic consequences that destroy habitability, but via a feedback sequence that operates over a longer timescale than adaptation of the organisms concerned, or involves a threshold or "tipping point" etc.

SPECIFIC COMMENTS ON THE PAPER (in order of appearance):

Section 1.1 the quotations refer to the system as a “single” entity, so I would argue actually support the planetary scale being a necessary feature.

Table 1 I don’t think that frequency of search terms is a reliable guide to the distinction between necessary and contingent attributes. Also don’t think the aeroplane analogy works.

I also suggest a more detailed definition of group action in terms of control theory, particular if this can be related to specific genes. Classification of distinct control systems is interesting but needs to be more directly related to subsequent biological/genetic data.

1.2.3 see the two issues raised in my initial comment above, i.e., adaptation and separation of scales.

Paragraph beginning “recent conceptual advances” it is nice to have some of my own work quoted, but many would challenge whether this conclusively shows that can evolve by persistence selection. I would argue that it sketches a possible path, but others would dispute whether this is enough to solve the problem, and the discussion is ongoing.

Results, Case 1: “For signals from individuals to add in such a way that there is a signal observable at the global level, not simply noise – this globally aggregated signal must (i) arise out of a dominant proportion of the individuals of the global biosphere and (ii) be sufficiently preserved to be seen in the globally aggregated output. This is explainable by a dominant proportion of the individuals of the global biosphere working in concert, a notion that matches the Gaia core attribute of joint action.”

This is an interpretation, based on a number of untested assumptions, to the effect that a simple kind of averaging exercise is the explanation of putative homeostasis. I am not

convinced that this is valid. Also, I'm unclear as to what "in concert" means in this context, given that some physiological functional types may disproportionate contribute to homeostasis in the focal variable.

Cases 2 and 3 interesting stuff, but the variable being controlled is the organism's internal composition so I question the relevance to Gaia.

Cases 4 and 5 you seem to be arguing that because there is joint action at these scales there is joint action at an arbitrarily large (i.e. planetary climatic) scale, which seems odd. If this isn't what you are implying then I don't understand the relevance to Gaia.

Case 6 is very interesting but niche constructionists will argue the same thing as above.

Table 2 the "not applicable" bit, I would argue very applicable – i.e. a direct relationship between the properties you detect and the evolution of the genes connected is exactly what you are implicitly claiming to have found. How you get from one to the other is the essence of the Gaia and natural selection question.

Appendix 1 seems unnecessary, perhaps work into a text or turn into a table or similar.