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Interactive comment on "The regulation of the air: a hypothesis" *by* E. G. Nisbet et al.

D. Schwartzman

dschwartzman@gmail.com

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I can sum up my critique of this very stimulation paper: it has the Rubisco tail waging the long term carbon and oxygen biogeochemical cycle dog. The dog wags the tail, not vice-versa. First I note the very strong case for the long term carbon carbonate silicate cycle regulating the atmospheric/oceanic carbon dioxide level and ocean pH (Zeebe and Caldeira, 2008). Further, it is important to stress that Ca carbonate precipitation is mediated by life, but would occur in an abiotic regime once the sufficient supersaturation level is reached with the dominant source of both calcium ions and bicarbonate ending up as the calcium carbonate sink being chemical weathering on land. I submit there a good case for the carbon-dioxide concentrating mechanism being a long term adaptation to the declining carbon dioxide to oxygen ratio in the atmosphere for the last 2.4 billion years. This conclusion is supported by the variation in fine-tuning of Rubisco as a function of the local environment noted on p. 772. The evolution of

Rubisco has tracked the long term variation of the oxygen to carbon dioxide ratio in the surface environment over geological time. Our estimate of the atmospheric carbon level triggering cyanobacterial emergence was about 0.1 bar, 333 x the present atmospheric level (Schwartzman et al., 2008). This is consistent with the CCN mechanism raising the proximate carbon dioxide level on this order of magnitude owing to its long term atmospheric decline. The authors point out that inorganic processes have a longer time-scale. First the long term carbon cycle is biogeochemical not simply geochemical, with biotic enhancement of weathering and of course the organic carbon sink integral components. And just as anthropogenic C emissions have thrown the short term cycle out of steady-state, on the longer geologic time-scale the steady-state will be restored. Rubisco's influence on the carbon cycle may have similar perturbations, but does not regulate the climate on geological time-scales. Their argument for low carbon dioxide levels in the Archean is very problematic. Rosing et al.'s (2010) circular argument for low carbon dioxide levels (they assume low temperature hence the triple point of their phase diagram gives low levels, if thermophilic temperatures are assumed, the triple point corresponds to high levels). And this assumes magnetite in the BIFs is a primary precipitate, which has been seriously questioned (e.g., Craddock and Dauphas, 2011). Very recent support for thermophilic Archean temperatures independent from the oxygen isotope story is found in Fralick and Carter (2011). Finally the authors say: "That for any planet, chance trajectories of surface temperature which sustain clement conditions over geological aeons are very improbable, given the complex parameters of solar brightening, geological partitioning of C, O, N and S species between the planetary interior and the surface, exchange between ocean and air; and time. In the Earth's early history, inorganic processes may have produced clement conditions for a relatively brief period (105–107 yr) (Sleep et al., 2001), but this could not have been sustained over aeons. The implication is that it will be very difficult to create an inorganic feedback model that successfully sustains clement temperatures over the aeons. In contrast, the continually selective nature of biological evolution has inbuilt adaptation." (p. 782). I note that the relevance of the biotic enhancement of weathering on the long term carbon cycle is ignored in this paper, hence an abiotic scenario is not confronted, i.e., removing BEW (and the organic C sink) an abiotic Earth, with the same initial conditions as well as tectonic as well as solar luminosity history, would likely have been habitable since the first appearance of liquid water (surely an abiotic initial condition) but too hot for low temperature life, not too hot for thermophiles (Schwartzman, 1999, 2002). I argued that the pattern of biospheric evolution we record on our planet is likely a common one if this evolutionary process including coevolution of the biosphere and climate is roughly deterministic. Not "very improbable", rather quite probable if the initial conditions are similar. And a recent paper estimates that some one-third of FGK stars in our galaxy have terrestrial planets in the habitable zone (Traub, 2011) So by all means invoke Lovelock and Gaia, but I can't share the paper's support for the original hypothesis (optimal homeostasis by and for life), as stimulating the read of your paper was.

References

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