



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

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Received and published: 20 December 2011

Nisbet et al. present an interesting hypothesis of co-evolution of rubisco (and nitroge-nase) and atmospheric composition through earth history. The paper is well written and a pleasure to read, and the arguments presented are a pleasure to consider. However, I find the hypothesis only permissive, as the authors indicate, but not compelling at this stage of development, for the following reasons:

1) Virtually all N in the biosphere (including ocean and atmosphere) is in the atmo-sphere. Thus I don't find it compelling to argue that the N₂ content of the atmosphere is under biological control. Indeed, one might argue that atmospheric regulation has failed. More compelling is the regulation of ocean fixed nitrogen content by a balance between nitrogen fixation and denitrification.

2) I think that the issues of time scale of feedbacks is very important here, and this is

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why numerical modeling is so important. For example, Caldeira and Kasting demonstrated some time ago that because the spread of sea ice is so fast compared to the feedbacks that control CO₂, snowball Earth is hard to prevent with feedbacks, such as reduced productivity in the ocean. Diagrams like Fig. 3 are informative but can be misleading because of similar issues of timescale of adjustment.

3) In framing the arguments the authors tend to ignore important processes, making statements that are incorrect. For example, on line 7, p. 778, the authors state that photosynthesis controls CO₂ and global temperature on annual to millennium scales, ignoring atmosphere-ocean equilibration that can have marked effects on CO₂ levels. They go on to claim that a C₄ dominated biota would freeze the ocean, ignoring other feedbacks that would prevent such a catastrophe.

4) I'm not convinced that rubisco evolution doesn't just evolve in response to changing atmospheric CO₂ and O₂ contents that themselves are regulated by other feedbacks. This would explain the modern coincidence of their atmospheric ratio and that predicted by eq. 2. If, for example, the carbonate-silicate cycle controls atmospheric CO₂ (as has been argued by geochemists for decades) and controls on organic carbon burial determine oxygen levels, then these evolve independently and selection on rubisco, as the authors describe, keeps pace.

5) I'm not at all convinced by the N arguments for reason number 1 above and because the link between N:P in today's ocean and plankton is only because the ocean is largely oxic; anoxic oceans of the past likely had N:P ratios that differed significantly from typical plankton because of the increased importance of nitrogen fixation.

6) Drawdown of CO₂ can only occur by organic carbon burial; the biosphere cannot store a significant quantity of C on multimillennial timescales. So, the arguments listed on p. 780, line 20 about changing rubisco and its effects on atmospheric ratios won't work unless the changes in rates of primary production lead to changes in burial rate.

7) As stated above, I don't think that one can use Fig. 3 to argue that rubisco speci-

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ficity and its evolution can "manage surface temperature". In other words, the authors have not convinced me that the carbonate-silicate cycle controls CO₂ and climate on geologic timescales; they need to adopt a quantitative approach.

8) If this mechanism works (rubisco co-evolution with atmospheric composition), how does one explain the Archaean, when O₂ levels were essentially zero? The CO₂:O₂ ratio approached infinity! The authors could sharpen their focus on the Phanerozoic, but that would be just putting the issue of the Archaean under the rug. . .

9) In my opinion, the tests of the hypotheses listed on p. 782 have already largely been made and fail in terms of the proposed hypothesis. Inorganic controls (not just on carbonate precipitation, but on silicate weathering) can explain CO₂ levels. And they explain the levels pretty well (and other geologic controls explain O₂ pretty well too), such that Berner shows that O₂ and CO₂ vary inversely through the Phanerozoic, contrary to the general prediction that they should vary in concert with a relatively constant CO₂:O₂ ratio. Atmospheric nitrogen and atmospheric pressure, for the reasons given above, is demonstrably not under biological control, and it is easy, not difficult to create an inorganic feedback model that successfully sustains clement temperatures over the aeons.

So I think this paper could be improved by being a bit more cautious in its wording, bringing in the caveats I present above, and in the best of all worlds (but not necessary at this stage) developing this into a quantitative model. The discussion is an important one, and in fairness to the authors, despite decades of work, we really don't have a compelling mechanism for regulating the O₂ content of the atmosphere. CO₂ is a bit further along in this regard, thanks in large part to the modeling efforts of Berner, but one might argue that Berner never included evolving rubisco specificity in his modeling. Perhaps the authors would like to entrain Berner into such an exercise rather than building their own comprehensive quantitative model of the CO₂/O₂ controls.

More minor points:

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Line 5 on page 776: the references cited are all about the consequences of increasing solar luminosity, not references about increasing solar luminosity per se. More appropriate here would be a reference to Gough et al. (1981; "Solar Interior Structure and Luminosity Variations". Solar Physics 74 (1): 21–34.)

Line 26, p. 776: missing the subscript 2 on the first CO₂

Line 15, page 777: None of the controls discussed are related to thermodynamic equilibria, and biology certainly cannot affect this; biology can affect kinetics, though.

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Interactive comment on Solid Earth Discuss., 3, 769, 2011.

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