

Clim. Past Discuss., author comment AC1
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Reply on RC1

Adam Yassin Jaziri et al.

Author comment on "Dynamics of the Great Oxidation Event from a 3D photochemical–climate model" by Adam Yassin Jaziri et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-150-AC1>, 2022

This very constructive comment raises several misunderstandings. Mainly, the redox balance of the early atmosphere and the effect of surface temperature on the biosphere are discussed. We try with this answer to better clarify our approach which is in agreement with the comments.

In the atmospheric model, the redox balance of the early atmosphere is ensured by atmospheric chemistry and a surface flux that combines the different contributions mentioned ("organic carbon burial and the input of reductants"). The objective of the atmospheric model is to quantify the atmospheric contribution which establishes a dynamic equilibrium with all the fluxes at the surface. This does not seem to be correctly underlined in the article. This is therefore the subject of a modification in part 2 "Model", aiming to clarify the meaning of the surface flux, corresponding mainly to the organic carbon burial and the input of reductants. Thereafter, all the contributions are well separated for the study of the temporal evolution.

We also agree that atmospheric instability did not "trigger" the GOE. The evolution of the organic carbon burial or other fluxes must be the cause. The atmospheric contribution is only at the origin of an instability that allows this rapid evolution from a low oxidized state to a high oxidized state, which controls the oxygen levels without being the trigger of the GOE. This also seems unclear in the article and may be subject to some linguistic adjustments.

These different misunderstandings are now better explained. Also, there seems to be confusion about Figure 15. The results of the 1D and 3D atmospheric simulations in terms of atmospheric loss flux are largely similar. Figure 15, as the legend indicates, then compares the result of 1D simulations with the theoretical model of Goldblatt et al. (2006). The interest in the decrease of the bistability zone does not come from the 3D model but only from atmospheric models (whose 1D approximation seems to be sufficient). Moreover, Figure 16 shows that despite this the dynamics remain similar. But it is true that we could insist more on this new result to emphasize the easier return from a high oxidized state to a low oxidized state.

We answer in the following to the detailed comments for which we are in agreement and of which the most part come from a confusion which will be corrected by clarifications in the article.

Thank you for your comments,

Authors

Answers to the detailed comments:

1-5 will be updated in the preprint regarding the comments.

8. We agree on the GOE trigger. The confusion can be corrected in the preprint and the reference added.

9. The comment about nitrogen is relevant. Its chemistry is not constrained but not negligible, this will be corrected.

11. It is indeed undeniable that the surface temperature will impact the biological activity at the surface. Nevertheless the approach here does not contradict these facts. It tries to put forward the effect only on the atmosphere which has not been put forward until now, whereas the effect on the biological activity is better known. This methodological confusion in the article can also be clarified following this comment.