

Review: The Oxygen Valve on Hydrogen Escape Since the Great Oxidation Event - by Cooke et al.

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1 The review

In this manuscript, Cooke et al. investigate how variations in atmospheric oxygen (O_2) mixing ratio since the Great Oxidation Event influence the total hydrogen (H) mixing ratio at the homopause, and consequently the diffusion-limited H escape rate over the past 2.4 Gyr. To address this question, the authors employ WACCM6, a three-dimensional chemistry–climate configuration of the Community Earth System Model (CESM). They perform eight simulations spanning a wide range of atmospheric O_2 levels, from 0.1 % to 150 % of the present atmospheric level (PAL). The main result is that although the diffusion-limited escape rate of H varies with O_2 abundance, it remains within a factor of about 5 of the present-day value across all scenarios. These escape rates are sufficiently small to be negligible on geological timescales, supporting the conclusion that substantial H loss did not occur after the onset of the Proterozoic. The authors further show that the stratospheric H mixing ratio responds nonlinearly to O_2 mixing ratio, with maximum H escape predicted for O_2 mixing ratio between 5 to 10 % PAL. This behaviour is attributed to changes in ozone (O_3) mixing ratio in the tropical tropopause layer, which modulate UV heating, local temperatures, and the efficiency of the cold trap. Overall, the study highlights the role of atmospheric oxygen as a nonlinear “valve” regulating H escape and motivates the necessity of three-dimensional chemistry–climate models to accurately capture the processes controlling Earth’s H loss through time.

I believe this manuscript is suitable for publication after minor revisions. I particularly appreciated the well-structured and thoughtful introduction, which clearly outlines the key concepts and mechanisms that are subsequently developed throughout the paper. The main results are clearly presented, well illustrated, and adequately justified. However, more can be done (see comments below) to further improve the manuscript.

1.1 Major comments

1) My main comment relates to the interpretation of the results presented in Table 2. While a modest increase in the hydrogen escape rate is observed at 5 and 10 % PAL, the overall trend suggests a substantial decrease in escape rate with decreasing O_2 mixing ratio. Based on these results alone, one might reasonably expect this trend to continue toward even lower O_2 levels, which are more representative of pre-GOE conditions. This appears somewhat at odds with the broader conclusion that hydrogen escape rates should have been significantly higher prior to the GOE.

It would therefore be very helpful to clarify this apparent discrepancy. In particular, an additional simulation at much lower O_2 levels (e.g., 10^{-3} – 10^{-4} % PAL), representative of pre-

GOE conditions, could help resolve what I refer to here as a low-O₂–low-H-escape paradox. Would it be feasible to add such a simulation to the experimental set?

2) A second major comment concerns the description of the model setup and the assumptions underlying the simulations. In particular, it is not entirely clear which orbital configuration is used in this study (presumably modern). Similarly, while the use of contemporary ocean and land settings is mentioned (line 101), it would be useful to clarify whether adopting a modern continental configuration is common practice in deep-time atmospheric studies, and how this choice compares with previous work.

I strongly encourage the authors to clearly and explicitly specify these aspects in the model description section, and possibly to reiterate them in the conclusions, especially given that the importance of such parameters is acknowledged later in the discussion (lines 271–273). Additional information on the ocean configuration (slab versus fully coupled, resolution), prescribed pCO₂, and solar constant would also improve transparency and reproducibility.

While I agree with the statement in line 290 that a full reconstruction of Proterozoic conditions is not required, it should nevertheless be made explicit which aspects of the climate system represent Proterozoic conditions and which do not. If all parameters except O₂ are kept at or close to 1850 conditions, it becomes less clear how directly these simulations can be used to infer hydrogen escape over the past 2.4 Gyr. In that case, a more developed justification, beyond what is currently provided in lines 290–291, would strengthen the interpretation of the results.

3) My final major comment relates to the Discussion section. Overall, I found it to be rather long and primarily focused on outlining potential future research directions. While this is useful, the discussion would benefit from a stronger emphasis on interpreting the results presented in this study.

In particular, a more explicit comparison with previous modelling or theoretical studies, as well as a clearer link to available geological constraints, would help to place the results in a broader context. A dedicated subsection (or paragraph) on model limitations would also be valuable. Moreover, some key results, such as the role of clouds or the behaviour of the tropical tape recorder, are not revisited in the discussion.

The Discussion could also be used to further explore the physical mechanisms underlying the nonlinear dependence of hydrogen escape on O₂ mixing ratio. For example, why does the O₃ abundance itself respond nonlinearly to changes in O₂? The discussion could also be used to resolve the low-O₂–low-H-escape paradox mentioned above.

Finally, the two subsections devoted to future work might be merged and condensed to reduce the introduction of new material at this stage of the manuscript, thereby allowing more space for a deeper discussion of the study's key results and their implications.

1.2 Minor comments

1) Caption Table 1. In the sentence: "The mixing ratios or fluxes of other gases specified at the surface we kept constant as O₂ was altered.", "we" should be replaced by "were". Please check and correct.

2) Line 102. Consider rephrasing to "70 atmospheric layers along the vertical" rather than "70 vertical atmospheric layers".

3) Line 112. To avoid potential confusion, I suggest rephrasing as: "were computed using implicit numerical schemes" and "were computed using explicit numerical schemes".

- 4) Line 144. Consider replacing "at pressures greater (altitudes below) 10 hPa" with "at pressures greater than (altitudes below) 10 hPa".
- 5) Line 173. Please specify whether the longitude range is expressed in °E or °W.
- 6) Figure 4. The yellow–orange–pink contour levels should be consistent between the two panels. As currently presented, the differing scales are misleading and hinder comparison. Adding additional contour levels and extending the colour gradient would greatly improve the interpretability of the figure. The same comment applies to Figure 5.
- 7) Figure 5. Please check the small white gap at the extreme right of each panel, which gives the impression that the final month may not be correctly displayed. Same for Figure 3.
- 8) Figure 6. In the first panel, consider replacing "Warmest" with "Highest" in the label of the vertical axis and in the caption. Please also check the text, as this was also at lines 201, 203, and 211. In addition, in the final sentence of the caption, should the range be $\pm 24^\circ$ rather than $\pm 20^\circ$? If this is intentional, a brief explanation of why the tropical range differs here would be helpful.
- 9) Tropical tape recorder Results section. Is the Tropical tape recorder essential to the core message of the paper? Alternatively, the focus could be placed on the persistence of seasonal cycles and their phase lag of about 6 to 12 months relative to surface conditions, rather than on the "tape recorder" metaphor. I also wonder about the robustness of these seasonal signals, given that many parameters are held constant. For example, changes in continental configuration and astronomical forcing through time are expected to significantly affect seasonality. Some justification of why these seasonal-cycle results remain relevant over the last 2.4 Gyr would strengthen this part of the analysis.
- 10) Code availability and data availability. References to the model version, the ExoCESM GitHub repository, and the Dryad data repository should be accompanied by a DOI and/or a direct URL.

Again, I would like to congratulate the authors on their manuscript. I found the manuscript enjoyable to read and, in the end, had the clear impression of learning something useful.