Cooke et al. employ a 3-D atmospheric chemistry–climate model (WACCM6) to investigate how changes in atmospheric O_2 following the Great Oxidation Event influence the diffusion-limited hydrogen escape rate. Their strategy of isolating the effect of pO_2 is well justified, and the mechanistic framework linking O_2 to hydrogen escape through O_3 production and its impact on tropopause height and temperature is clearly articulated. This study will be a valuable contribution to both the Precambrian-Earth and Exoplanet communities.

One general comment I would like to raise here is that at lower pO_2, Rayleigh scattering in the Schumann–Runge bands (175–192 nm) becomes increasingly important because solar UV radiation penetrates deeper into the lower atmosphere. For the same reason, absorption by H_2O and CO_2 in the Schumann–Runge bands also becomes critical under lower pO_2 conditions. I recall seeing paper showing that neglecting these effects could lead to overestimation of OH production by orders of magnitude at low pO_2 (Ji et al., 2023 *JGR*). I note an earlier paper by the lead author (Cooke et al., 2024 *Planetary Science Journal*) mentions that absorption in the Schumann–Runge bands has been newly implemented in the WACCM6, which is great. However, it would be helpful to clarify whether Rayleigh scattering in this wavelength range is included in this study.

Most of my remaining comments are either minor or technical and are discussed in detail below.

Lines 134–135:
I am a little concerned about holding other gases, particularly CH_4 and H_2, fixed at a given surface mixing ratios may not fully isolate the effect of changing O_2 alone. Altering pO_2 inevitably changes the abundances of OH and $O(^1D)$, which in turn affects the lifetimes of CH_4 and H_2. Moreover, the model would automatically adjust the upward flux (or number density) of CH_4 and H_2 at the surface, in order to maintain those surface mixing ratios fixed at 0.8 ppmv and 0.5 ppmv. For the sake of argument, let's assume a biological methane source of ~300 Tg yr^{-1} yields a surface mixing ratio of 0.8 ppmv under 1 PAL of O_2, but at lower pO_2 the flux required to maintain the same surface CH_4 abundance could be substantially different. So my question is, to what extent might the results in Table 1 reflect combined changes in O_2 and methane source strength, rather than O_2 alone?

Lines 168–169:
I did not see a citation for the binary diffusion coefficients used here, which have different temperature dependence. Please clarify the source of these coefficients, or how they were derived.

Lines 202–205:
This is an interesting result. I recall the classic O_2–O_3 calculations by Ratner and Walker (1972, *Journal of the Atmospheric Sciences*), who also found that ozone mixing ratios peak near ~10% PAL O_2. In that study, however, the vertical temperature structure was held fixed. It would be helpful to further discuss why the ozone mixing ratio at the tropopause reaches a maximum near ~10% PAL, given that this behavior may be driven by processes different from what currently being discussed here (the temperature/greenhouse effects because of peak ozone).

Line 351:
Could the authors clarify whether the stated "20× modern iodine concentration" refers to oceanic iodine, atmospheric iodine, or both?