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Climate of the Past Discussions

Interactive comment on "Examining the role of varying surface pressure in the climate of early Earth" *by* Junyan Xiong and Jun Yang

Anonymous Referee #2

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Focusing on the role of atmospheric pressure, Xiong and Yang present one possible solution for the faint young sun paradox. Overall I think this contribution can move forward our understanding and I think it should eventually be published. However, I have several concerns and questions that I think should be addressed before publication (outlined below).

Major issues:

At page 2, line 21: First off, there is no geochemical proxy on atmospheric methane, so we simply don't know their upper or lower limit in the past. Second, Pavlov et al. 2001 on Archean kerogens didn't give an upper limit on methane concentration after their modelling exercise. Third, even if they did, many of the Archean kerogens are now believed to be contaminated by the oil drilling, therefore became an unreliable indicator

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for CO2/CH4 ratio.

At page 4, section 2.1: A question to the authors: the 1-D radiative transfer model also has Rayleigh scattering induced changes in planetary albedo, which then linked to the outgoing solar radiation. Did the albedo from 3-D model then coupled with the albedo parameter in the 1-D model? If not, why?

At page 15, table 2: Even if the authors can ignore the Archean high obliquity hypothesis, why is the obliquity is set to zero? Some justification is needed. Also, if ocean heat transport is a major parameter that differs from previous modeling work, what are the reasons the authors had in choosing their parameter space? Please provide more justification on the benefits of the utilized model and note how it compares to other models.

At page 5, line 14-15: even if pCH4 can be set as high as 1E-3 as a modelling exercise, I wonder why the authors didn't mention the concurrent hydrogen flux (or the lack thereof), which according to Kharecha et al., 2005 Geobiology paper, is quantitatively similar to the methane concentration (on a related note, the lead author from the same research group believe the methane estimate in their Kharecha et al. 2005 paper is more reliable than their Pavlov et al. 2001 paper, on top of my major issue 1) . Since this article is mainly about the effects of pressure, neglecting a major constitute in the Archean atmosphere seems a bit odd to me. Even if hydrogen eventually escape from the atmosphere, it is still a major constituent in the Archean atmosphere if outgassing is continuous. In addition, hydrogen serves as an indirect greenhouse gas that increases the lifetime of methane through scavenging radicals like OH.

Minor issues:

At page 1, line 22-27: One fundamental aspect about seawater temperature reconstructions the author didn't mention is that the delta 18O value in seawater can change overtime. Recent analysis on iron oxides, a temperature insensitive sedimentary proxy, shows that the seawater delta 18O value can increase by 15 permil since the Archean

(Galili et al., 2019 Science).

At page 2, line 7-13: in the texts above, the authors argued from multiple lines that the Archean seawater temperature was similar or higher than the modern value. If so, why do they argue the higher pCO2 was maintained by a low surface temperature? The authors argument based on silicate weathering feedback seemingly contradict with their own propositions on surface temperature and pCO2. It may be that this section just needs to be rewritten for clarity.

At page 2, line 24-25: it might be better to reference Pavlov and Kasting 2002 Astrobiology paper for Archean pO2. That paper was the original work that provided the most commonly cited upper limit on Archean pO2. Also, 1% PAL of O2 would contradict the modeling decision of not including oxygen and ozone in their bulk atmosphere composition, which also have pressure broadening effect on CO2 and H2O.

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