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Interactive comment on "The faint young Sun problem revisited with a 3-D climate-carbon model – Part 1" *by* G. Le Hir et al.

Anonymous Referee #2

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The authors use an atmospheric GCM coupled to a mixed-layer ocean model with thermodynamic sea ice to investigate greenhouse solutions to the faint young Sun paradox (FYSP). In particular they study the influence of larger cloud droplets (which have been hypothesized as a potential contributor to warming on early Earth) and find that such clouds could significantly warm higher latitudes.

This is an interesting study in line with recent attempts to move beyond the radiative-convective models with fixed albedo traditionally used to investigate the FYSP. Regrettably, the paper suffers from an unfortunate choice of boundary conditions and is not very well written. It merits publication in Climate of the Past, however, after major revision addressing several fundamental issues discussed below. The authors should view the rather long list of recommendations as helpful advice how the manuscript should

be improved.

Major comments

1. The paper has a strong focus on demonstrating that low CO_2 concentrations inferred from various geochemical estimates are sufficient to offset the faint young Sun. In all their simulations, however, they use 900 ppmv of CH_4 in addition to the CO_2. Very surprisingly this substantial amount of methane is completely ignored in all discussions throughout the paper, although it provides a considerable part of the warming in the simulations.

It should also be pointed out that 900 ppmv of methane is on the high end of the estimates of atmospheric methane during the Archean and a completely unrealistic value for the very early Archean (before the evolution of methanogens) and for the Proterozoic after the Great Oxidation Event. Furthermore, the experimental design with 900 ppmv of CO_2 and CH_4 is not ideal given that the CH_4/CO_2 ratio is beyond the limit of haze formation (as the authors correctly mention at some point).

These issues have to be discussed more prominently in the paper, in particular when comparing the results of this paper (which adds CH_4) to other studies (which do not).

2. The authors performed simulations for several time slices between 3.5 Ga and 1 Ga, varying some of the boundary conditions (solar luminosity, fraction of emerged land, continental configuration) but keeping the greenhouse-gas concentrations constant. In this sense their set of experiments represents a mix of realistic and idealized boundary conditions. This is not a problem in itself, but in several places the authors describe differences between the time slices in terms of changes in time ("evolution", "climatic transition") which is very misleading.

3. The papers strongly follows the argumentation in Rosing et al. (2010) in terms

of very low CO_2 values, atmospheric greenhouse-gas concentrations and the effects of larger cloud droplets. The Rosing et al. results are somewhat controversial, however, and the assumption of larger cloud droplets is rather speculative. Furthermore, the choice of greenhouse gas concentrations would imply the formation of a cooling organic haze layer as mentioned above. Since repeating the simulations with different settings would be an unreasonable demand, the least the authors should do is to be more specific about potential caveats. In particular, they have to be more specific whether the assumption of larger cloud droplets is justified or not.

4. The paper is not very well organized. Much of the text in section 2 is material for the introduction, while the experiment description at the end of that section is too detailed given the fact that there is a whole section on experiment design further below. Furthermore, discussion of uncertainties is presented in several places in section 4 whereas the conclusion section makes little mention of assumptions and caveats. This paper definitely requires a separate discussion section after section 4 and a more balanced summary of the results in the conclusions.

5. Finally the manuscript would definitely profit from more careful proofreading and language editing by a native speaker. A (by no means complete) list of technical corrections is provided towards the end of this review.

Specific comments

p 1510, l 15-16: This is not true, a significant part of the warming results from CH_4!

p 1510, l 16-17 (and p1522, l 1-2): I had to read this sentence twice before I could believe it: Do the authors seriously announce that one of their main conclusions will be shown to be invalid in a second paper which is not yet available? This would be very annoying for readers indeed! It is not a problem, of course, once the companion paper becomes available at least as a discussion paper.

p 1510, l 20: 25

p 1510, l 24: There is liquid water even below the ice on a snowball Earth, so it is liquid water at the surface which is important.

p 1511, l 8: The quoted value of 0.06 bar in Kienert et al. (2012) is not the "critical" partial pressure.

p 1511, l 13: The discussion of the differences between Rye et al. (1995) and Sheldon (2006) is not very accurate, the main issue is that the Rye et al. limit was derived from thermodynamics, whereas Sheldon's limit is derived from the kinetics of weathering.

p 1511, l 26-28: "However" is confusing because Driese et al. (2011) do not support the results by Rosing et al. (2010).

p 1512, l 1-4: I disagree that the CO_2 constraints "challenge our understanding". First, it is very likely that CH_4 has contributed to warming during the time periods for which we do have empirical constraints. Secondly, other greenhouse gases or pressure broadening or some other effects could have contributed to climatic warming.

p 1512, l 5 - p 1514, l 16: This discussion of the possible implications of cloud properties for the FYSP is material for the introduction rather than a separate section. Furthermore, the heading "How to solve the faint young Sun problem?" is not appropriate since it remains unclear what contribution clouds have in solving the FYSP.

p 1512, l 20-24: The discussion should be more critical. At the very least, some of the many studies criticizing the Rondanelli and Lindzen (2010) papers should be cited.

p 1513, l 14-17: The critical comment by Goldblatt Zahnle (2011) on the Rosing paper should be discussed.

p 1514, l 7-14: It should be pointed out already here that the methane to carbon dioxide mixing ratio is beyond the limit of haze formation.

p 1514, l 14-60 and p 1522, l 4 - p 1523, l 8: It is unclear to the reader whether this second set of simulations is done with or without CH_4 in the atmosphere. It is never mentioned, so that one would assume these are done without CH_4, but in the caveats (p 1522, l 26 to p 1523, l 1) haze formation at CH_4/CO_2 = 0.5 is mentioned, so I guess they are done with CH_4. If so, the authors should be very careful when comparing to other studies without CH_4 since 900 ppmv will considerably contribute to the warming.

p 1514, l 19-15: There is no description of the sea-ice model which is an essential module for this type of study. The authors should point out that sea-ice dynamics are not included in this model which could affect their conclusions. Also, the sea-ice albedo values are critical parameters for climate states close to the snowball-Earth instability, they should be moved from the caption of Figure 2 to the model description section.

p 1514, l 23-25: Do the authors adjust the parametrization of the heat transport in the mixed-layer ocean to reflect Archean boundary conditions or do they use the present-day diffusion rate?

p 1514, l 25 - p 1515, l 2: How well is the FOAM radiative transfer scheme calibrated for the very high CH_4 concentrations used in this study? Furthermore, there is considerable uncertainty with respect to the continuum absorption of CO_2 at high CO_2 levels (Halevy et al. 2009). This does not apply to the relatively low CO_2 levels derived in this study, but since in reality CH_4 levels were probably much lower and CO_2 levels much higher, it would be good to know how the radiative transfer scheme used here relates to the parametrizations in Halevy et al., in particular with respect to sensitivity experiments without CH_4 (see below).

p 1515, l 3-16: The limitations due to the lack of an ocean GCM and sea-ice dynamics should be noted here.

p 1515, l 7-9: Even if differences in cloud schemes between GCMs were fully "understood" (which I doubt) that does not mean that we know which one is correct. Furthermore, I doubt that differences in clouds are only significant for snowball Earth climates.

p 1518, l 18 - p 1518, l 3: A table summarizing the various experiments and their boundary conditions would be useful.

p 1515, l 21: As mentioned above, it should be discussed how (un)realistic 900 ppmv of methane are for the different time slices.

p 1515, l 22: "orbital parameters are set at their present-day values" Please specify what "present-day" means in this context.

p 1516, l 18-19: The reconstructions from Pesonen et al. (2003) represent time periods from 2.45 Ga to 1 Ga. The authors should explain how these are extrapolated for the earlier time slices considered in this paper. They should also briefly explain the method by which Pesonen et al. derived these and discuss how uncertainties in the reconstructions could affect their conclusions.

p 1516, l 20-26: The validity of the assumption of larger cloud droplets should be discussed at some point, preferably in a discussion section at the end of the paper.

p 1516, l 26 - 1517, l 1: The description of how the shorter cloud lifetime is implemented in FOAM is confusing. More importantly, the dependence of the precipitation efficiency P_e on droplet size r_e is highly uncertain. In their supplementary online material, Kump Pollard (2008) state that it ranges from $P_e \sim r_e$ to $P_e \sim r_e^{5.37}$. This has to be discussed in the paper.

p 1517, l 16-17: Is the diffusion constant for the heat transport in the mixed-layer ocean adjusted for the new rotation rate or not?

p 1518, l 6-8: How are the experiments initialized?

p 1518, l 10 - p 1519, l 13: When describing the different time slices, the authors should avoid wording which suggests real climate changes in time, e.g., "evolution", "climatic transition" etc. They should further point out that the greenhouse-gas concentrations are held fixed and that this is unrealistic.

p 1518, l 15-16 and Figure 2: A stable state at a global temperature of $-20°$C is rather surprising and considerably colder than what is typically discussed in the literature on snowball-Earth transitions. The authors should explore possible reasons for this stability. Furthermore, the simulations without clouds are considerably warmer (and have a significantly lower planetary albedo) than the present-day cloud simulations outside the snowball-Earth regime, yet they fall into the snowball state at the same point. Why?

p 1518, l 24: The authors note the non-linear change in global temperature despite almost linear changes in solar luminosity. This is not really surprising given the nature of the climate system (and changes in other boundary conditions like the continental configuration).

p 1519, l 8-10: "Hence the solar constant evolution and its interplay with the ice-albedo feedback are the predominant factor governing the Earth's climate." This is a bold statement given the fact that the authors keep greenhouse-gas concentrations constant. They could either add "for fixed greenhouse-gas concentrations" or drop this rather meaningless statement.

p 1519, l 10-13: The authors should be more careful here, there is a huge amount of literature on the snowball-Earth instability, to a large degree performed with models simper than GCMs (by parametrizing albedo in terms of temperature, for example)!

p 1520, l 6-19: Again, the wording in some places appears to suggest evolution in time whereas the experiments are actually idealized.

p 1522, l 1-2: This has been discussed in the literature before, the appropriate references should be cited.

p 1522, l 4-7: Mention the CH_4 concentration in the simulations.

p 1522, l 7-9: Here, a more detailed comparison with previous studies is missing. Furthermore, the uncertainties need to be explored. What happens with smaller cloud

droplets? How much CO_2 is needed without CH_4? What is the sensitivity to sea-ice albedo parameters? The authors should run a few dedicated sensitivity experiments to explore these uncertainties.

p 1523, l 10-16: Mention whether methane is included in these simulations. Furthermore, it should be pointed out that a mixed-layer ocean with prescribed (present-day?) heat transport is used which could affect the results.

p 1524, l 2-3: The authors state that for present-day boundary conditions high latitudes are cooled at higher rotation rates whereas Figure A1 shows a warming in the entire southern hemisphere. Why? This is in contradiction to Jenkins (1996).

p 1524, l 15-20: A comparison is very complicated indeed due to the different model designs and choices of boundary conditions. The low-CCN are indeed likely to contribute to the difference, but also the mixed-layer ocean or the lack of sea-ice dynamics cold explain part of it. "Overestimate" would imply a firm knowledge that Archean clouds were indeed characterized by large droplets, but this is just a hypothesis. Finally, when comparing CO_2 partial pressures to other studies without CH_4 the authors should keep in mind that they add substantial amounts of CH_4 to the atmosphere.

p 1524, l 22 - p 1525, l 11: The paper definitely needs a more detailed discussion section which more comprehensively summarizes the results from the many experiments performed for this study together with a fair discussion of all the assumptions and possible caveats.

p 1524, l 22-26: Again, the role of CH_4 needs to be discussed, otherwise this sentence is very misleading.

p 1525, l 1-2: Again, the authors have to discuss here how plausible the assumption of large cloud droplets is in reality.

Technical corrections

p 1510, l 16-17 and p 1522, l 1-2: It is confusing to talk about the second part of "this paper", maybe better write "second paper" or "companion paper" or something like that.

p 1510, l 26: "peculiarly" is not the right word here.

p 1511, l 10-11: "in the mid nineties" appears twice in this sentence.

p 1512, l 4: motivates

p 1516, l 5: Kiehl

p 1516, l 18: Pesonen

p 1517, l 11 (and p 1524, l 18): I guess "nebulosity" is not quite the right word here.

p 1519, l 28: insignificant

p 1520, l 1-3: I suggest to rewrite this sentence because it is very difficult to understand.

p 1522, l 7: Progressively

p 1523, l 24: in

p 1524, l 2: Due to the reduced...

p 1525, l 5-6: It is not really the "spatial resolution" (i.e. the question how fine the model grid is) which is important here.

p 1531, Figure 2: The albedo values should be moved to the model description section.

p 1532, Figure 3: The blue squares are not described in the caption.

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