

ANSWER TO REVIEWERS

First of all, we are grateful to the two referees for their highly constructive criticisms and the high quality of their questions. For these reasons authors acknowledge the two reviewers, their suggestions greatly improve this manuscript.

Below we present a point by point answer to each referee comment.

Anonymous Referee #1

General comments:

This paper revisits the faint young sun problem using a general circulation model. Most of the results are unsurprising and do little to change the picture obtained from 1-D radiative-convective analyses of the problem, but there is one result which is novel and potentially significant, which is that the longwave effects of polar clouds can act to keep the Earth out of a Snowball, even at moderate CO₂ values (but see my comment below regarding the treatment of methane in the paper). This result is likely to be very model dependent, and it is uncertain how robust it will prove. The particular GCM used has a highly simplified empirical model of cloud water content. It also has no sea ice dynamics or ocean heat transport, which have been shown to considerably affect the conditions for initiation of a Snowball (See the Voigt et al CPD paper for a recent discussion of these effects). Besides that, the values chosen for snow and ice albedo also have a huge effect on the snowball transition (see the Pierrehumbert et al Ann. Rev. article). Nonetheless, the mechanism is novel in the context of the FYS, and deserves to be documented.

This paper should be published subject to revisions that I choose to refer to as "major" so as to underscore that they need to be treated seriously, though (with the possible exception of one comment) I do not think the authors will have a very hard time meeting my requirements. This paper is a vast improvement over the highly questionable Kienert study with CLIMBER, and for that reason alone deserves to be published. My major points are:

Q: (1) The authors refer to adjusting a cloud lifetime parameter, and this is likely to be a crucial part of their mechanism. However, the FOAM model they use does not have a cloud microphysics module, and specifies cloud water as a function of temperature (via the vertical integrated precipitable water). Thus, I have no idea what they might be referring to. Perhaps there is something in cloud fraction that has a lifetime in it, or perhaps they are confusing the CAPE relaxation time in the Zhang-MacFarlane convection scheme, but this point really needs to be clarified.

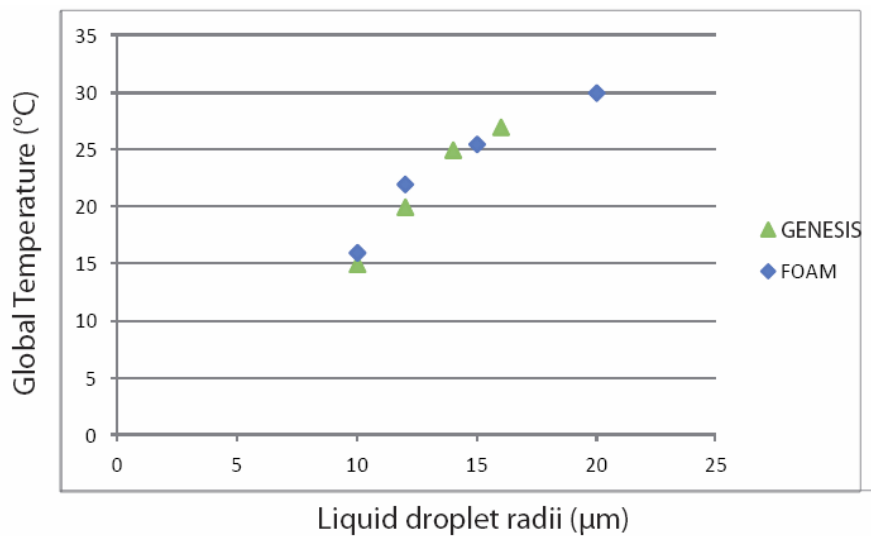
A: *The reviewer is right. This sentence is now removed. In absence of a cloud microphysics module, the clouds lifetime parameter cannot be adjusted. In the present study, only the increase in precipitation (precipitable water) has been modified according to the ratio proposed by Pollard and Kump, 2008. This change implies that rain is formed more rapidly when CCN concentrations are low (Andreae and Rosenfeld, 2008).*

To avoid any misleading interpretation, this point has been corrected in the revised manuscript (and the following reference describing clouds behavior is added (Hack 1998)).

Reference added:

Hack, J. J. (1998), Sensitivity of the simulated climate to a diagnostic formulation for cloud liquid water, J Climate, 11 (7), 1497–1515.

Moreover, we checked the validity of our clouds treatment (in order to correctly simulate the greenhouse warming due to larger cloud droplets).



Global and annual mean surface-air temperature with liquid cloud droplet radii reset to uniform values (changes affect both radiative properties and precipitation efficiency). Green triangles (modern simulations, GENESIS, Kump and Pollard, 2008), Blue diamonds (1Ga boundary conditions, FOAM present study).

We see that FOAM results are in good agreement with those obtained by GENESIS (Kump and Pollard, 2008). This sensitivity test suggests that our parameterization seems appropriated for estimating the warming provided by larger cloud droplets.

Q: (2) The paper suffers from a very superficial and uncritical review of the past literature. Papers are quoted without any critical discussion of the viability of the results. This includes the highly questionable CLIMBER results of Kienert et al, which are obviously unreliable because CLIMBER has neither the radiative transfer nor the dynamics needed to do an even vaguely informative attack on this problem; I view it as a failure of the review process that the reviewers did not spot the obvious problems with the CLIMBER calculations, and the vast disagreement of the FOAM results with CLIMBER only underscores how inadequate CLIMBER is for treating such problems.

A: A new section (section 4) is added wherein we discuss previous modeling studies

The initial version of the manuscript did not include a comparison with CLIMBER's results (Kienert et al. 2012) for several reasons. The first reason concerns the simulations, and differences in boundary conditions. CLIMBER runs (Kienert et al (2012) have been performed with present-day clouds and methane levels using a faster rotation rate, so it is difficult to make a direct comparison. Moreover this paper was submitted just few weeks before the present paper, which explains why we did not discuss this paper (just a short comment at the end of the section 3.5).

In details, if no other warming process is present, CLIMBER predicts a minimum of ~ 0.6 bar of CO_2 for a mean temperature $\sim 15^\circ\text{C}$. For reasons that seem unclear, Kienert et al (2012) assume that this rise of the CO_2 partial pressure is the consequence of the ice albedo feedback (enhanced by the faster Earth rotation rate). According to their paper, the radiative transfer is supposed valid (it was adjusted to be in agreement with Halevy et al 2009). Hence these

changes have to be found elsewhere. Regarding runs performed at 0.4bar of pCO₂ the pole-to-equator temperature gradient (ΔT) reaches 55°C. This large ΔT is attributed to a reduction of meridional heat transport associated to non-uniform decrease of the vertical lapse-rate. (Kienert et al, 2012).

However the ΔT reaches 30°C when the present-day rotation rate is used, other parameters being held constant. This result is at odds with GENESIS and FOAM results. In both cases, the GCMs predict that a faster rotation rate slightly increases the pole-to-equator temperature gradient (Figure 5b and Jenkins 1993). We may conclude that, in addition to clouds (see section 3.5), the atmospheric dynamics treatment in CLIMBER may overestimate the ice albedo feedback.

More importantly, the paper is written somewhat as if it is a vindication of Minik Rosing's equally questionable Nature paper on the FYS. That paper is referred to as "controversial," but let's face it, it was just plain wrong. It was wrong on the interpretation of the BIF record, as shown by Kasting's comment (Rosing et al do not really address that criticism in their response, but instead throw out a different argument which has yet to be evaluated and is probably equally wrong). It was wrong on the basis of clouds, as Goldblatt's comment shows. Le Hir's mechanism is not at all like the cloud mechanism in Rosing. Rosing fails to understand that clouds have a longwave as well as a shortwave effect, and his claim referred to reduction of albedo of low clouds alone. The mechanism in FOAM primarily involves the longwave effect of polar clouds.

A: *We preferred to use the term "controversial" to be more "diplomatic". We believe that the assumption (i.e clouds and CCN interplays) evoked by Rosing et al (2010) is still interesting even if their carbon dioxide constraint is now dismissed. This fact is now clearly mentioned in the revised text (section section 2.2 boundary condition and experimental design).*

The paper should also state the reasons to question the assumption that the Archaean had fewer CCN's. A vast variety of particles can serve as CCN's, including bacterial biogels which have been around since the beginning, and Charleson's CLAW idea, relying on DMS has been more or less rejected by data. For that matter the coccoliths that produce DMS did not evolve until past the Proterozoic, so they cannot have been the critical difference in the Archaean. It is worth documenting that a change in particle size can give a longwave effect that can help reduce the CO₂ needed to keep out of a Snowball, but the assumption that there should have been fewer CCN's is still very speculative.

A: *We added several sentences arguing this hypothesis (see 2nd paragraph section 2.2).*

To be honest, nobody knows the CCN concentration. In the same way that Goldblatt and Zahnle (2011) we assume that inorganic and organic fluxes have changed the CCN's availability over the whole duration of the Archean.

New reference:

M.O. Andreae, and D. Rosenfeld, Aerosol–cloud–precipitation interactions. Part 1. The nature and sources of cloud-active aerosols, Earth-Science Reviews 89 (2008) 13–41, doi:10.1016/j.earscirev.2008.03.001.

As a more minor point, I believe Gregory Jenkins did some GENESIS GCM simulations of the faint young sun back in the 80's or 90's. If I'm recalling correctly, these shouldn't be hard to find, and should be mentioned in the literature review.

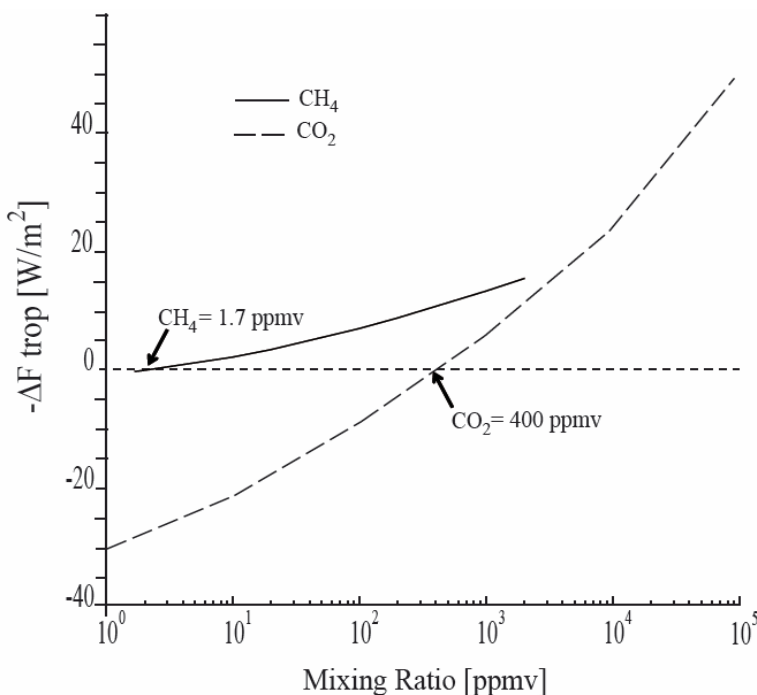
References added.

(3) Probably cuing off of Rosing, the simulations are done with 900ppm of CO₂ and 900ppm of CH₄. The methane values are unsupportable, since you would get thick organic hazes at that ratio of CO₂ to CH₄. Further, it is not likely that the ccm3 radiation code in FOAM is valid at such high levels of CH₄ (it probably is OK up to 100ppm). The inclusion of unrealistically high CH₄ gives a misleading impression of how low CO₂ can be kept without falling into a Snowball – methane is doing a lot of the heavy lifting. The simulations don't really need to be re-done, since Hansen's efficacy paper shows it makes little difference whether radiative forcing comes from CO₂ or CH₄. Thus, the authors can just quote the equivalent CO₂ value based on the ccm3 radiation code itself, avoid the issue of unrealistic methane behavior, and state that the CO₂ could be brought down somewhat by substituting CH₄ (or better, H₂, see the Wordsworth Science paper) for some of the CO₂. My own estimate is that the equivalent CO₂ is something like 10000ppm, but the authors should check using their own calculations.

A: *In the first version of the manuscript we didn't discuss this point because Rosing et al. (2010) have already given their conversion: "an increase of 7 p.p.m.v. units of CO₂ corresponds to a decrease of 1 p.p.m.v. unit of CH₄". Since ClimT (RCM used by Rosing et al 2010) and FOAM share the same radiative code (NCAR ccm3), we thought that this problem was not essential.*

To answer to reviewer, the new figure (figure 1) now shows the radiative forcing provided by CH₄ and CO₂ (see Le Hir et al. Climate Dynamics 2010 for details and methodology). This figure replaces the conversion by Rosing et al (2010) which seems clearly unrealistic.

In details, when the pCH₄ is set to 1.7ppmv the pCO₂ should be close to 7000 ppmv.



Above 100ppmv of pCH₄ our ΔF_{CH₄} remains close to values estimated by Kiehl and Dickinson model (1987), and underestimates by 10% the ΔF_{CH₄} predicted by Haqq-Mishra (2008) but seems in agreement with Halevy et al. (2009).

We sorry for the H₂ effect. It is clearly a novelty but it is clearly above the scope of the present study.

Reference added:

Halevy, I., R. T. Pierrehumbert, and D. P. Schrag (2009), *Radiative transfer in CO₂-rich paleoatmospheres*, *J. Geophys. Res.*, 114, D18112, doi:10.1029/2009JD011915.

Sequential comments:

p2: Mention the Wordsworth Science paper on the H₂ greenhouse effect as part of the discussion of possible other GHG's that can play a role.

Reference added

p3: The Kienert study is unreliable, as it was done with CLIMBER, which can't reliably represent atmospheric dynamics, least of all effects of rotation rate. Further, the radiative transfer model is highly simplified, and the lapse rate feedback is not reliably modeled either. The caveats should be mentioned here. Note also it's not entirely sensible that a faster rotation rate should favor glaciation, since less heat loss from the tropics means higher temperature gradient and hence easier to keep the tropics unfrozen.

The text has been changed (paragraph 2 section 1). Concerning the CLIMBER's radiative transfer module, the authors assume that their radiative forcing is correct (see paragraph 9, section 3 of Kienert et al. 2012). Unfortunately any figure demonstrates the accuracy of this sentence.

p4: Cite the papers showing the flaws in Lindzen's IRIS paper. There are many, but the BAMS response by Hartmann and others is a good starting point.

References added:

Hartmann, D. L., and M. L. Michelsen, 2002: *No evidence for iris*. *Bull. Amer. Meteor. Soc.*, 83, 249–254.

Reply to comment by Hartmann, D. L., and M. L. Michelsen, 2002 Bull. Amer. Meteor. Soc., 83, 1349–1352.

p5, line 25: It is not correct to say that 1D radiative-convective models cannot reproduce the ice albedo bifurcation. They can reproduce the bifurcation easily through incorporation of a temperature-dependent albedo, as in the EBM used for the Neoproterozoic in the Pierrehumbert et al Ann Rev. Neoproterozoic review, or in Chapter 3 of the textbook Principles of Planetary Climate. What the GCM brings to the discussion is the ability to remove some arbitrariness regarding the representation of horizontal heat transport.

Indeed this sentence is misleading. Budyko and Sellers have published description of their EBMs in 1969, and shown the possibility of alternative stable climatic states for the Earth due to the albedo. Our initial sentence refers to the recent studies (notably Rosing et al, 2010) where the ice-albedo feedback was not included. This sentence is rewritten (paragraph 4 section 1)

References added:

Pierrehumbert, R.T., Abbot D.S., Voigt., A. and Koll D. *Climate of the Neoproterozoic Annual Review of Earth and Planetary Sciences Vol. 39: 417-460 (Volume publication date May 2011) DOI: 10.1146/annurev-earth-040809-152447*

Pierrehumbert RT 2010: *Principles of Planetary Climate. Cambridge University Press, 652pp*

p7: Better to say "precludes the formation of a stratospheric temperature inversion." There's still a stably stratified region aloft which could reasonably be called "a stratosphere." In any event, to call this all "troposphere" is clearly incorrect.

This is right. This is corrected in the revised manuscript

p8: Kiehl (spelling) – *corrected*

I don't understand how cloud lifetime is implemented in this calculation. FOAM has a diagnostic cloud water scheme, which ties cloud water to temperature. Unless this scheme has been replaced, the lifetime isn't one of the parameters.

See section "general comments"

p 9, Faster rotation does not necessarily make the Earth more vulnerable to a Snowball. That depends on whether formation of polar ice leads to runaway ice growth. Weaker heat transport actually makes it easier for the tropics to stay warm, since they lose less heat to cold regions.

We think that our sentence is correct. In detail, we do not say that a faster Earth rotation rate makes the Earth more vulnerable to a snowball Earth. We say that in this case the Earth's becomes more sensitive to the ice albedo feedback due to the cooling occurring in high latitudes.

p10: The initial condition was never specified, and the procedure is unclear from the text. Given multiple states, this is important. I believe the simulations were started from the bright modern Sun and walked backwards (a "warm start") but this should be made clear in the text.

This is corrected in the revised manuscript. We added the sentence explaining initial conditions and how we performed our set of simulations (paragraph 1 section 3).

p11: Again, the problem is not the use of a 1D radiative-convective model, but the lack of inclusion of ice-albedo feedback, as noted previously.

This is corrected in the revised manuscript. We referred to the contribution of Rosing et al (2010).

p12: But the elimination of clouds doesn't change the position of the Snowball bifurcation, despite the considerably warmer non-Snowball climate. Why is that?

This apparent similarity in the snowball Earth bifurcation is an artifact. Theoretically the onset of the glaciation does not occur in the same time.

This point is now discussed (see paragraph 1 section 3.2)

p 13: The strong cloud greenhouse effect in high latitudes (presumably over open water) is somewhat surprising, but may be due to the relative insensitivity of cloud emissivity to droplet size, as compared to cloud albedo. (See Ch. 5, Principles of planetary climate). Smaller droplets allow the clouds to live longer and have more water content, but do not reduce the cloud emissivity much. But the authors need to say how they have gotten the lifetime effect into the FOAM cloud model.

See general comment.

Since FOAM does not include a cloud microphysics module, the lifetime cannot be invoked to explain this behavior. This cloud greenhouse effect is caused by interplays between air temperature, condensed water and clouds albedo. By their reduced albedo, clouds induce a warming and conduct to increase the water content into the atmosphere (at 1Ga, the specific humidity is two times higher with low CCN clouds than case with modern clouds). This mechanism enhances the cloud formation and conducts to a positive feedback.

p 16: Again, it's not surprising that Kienert got the wrong answer for rotation effects, given the manifest inadequacies of CLIMBER regarding dynamics. The authors should not be shy about saying so.

See section "general comments"

Anonymous Referee #2

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. Regret-tably, 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 CO₂ concentrations inferred from various geochemical estimates are sufficient to offset the faint young Sun. In all their simulations, however, they use 900 ppmv of CH₄ in addition to the CO₂. 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 CO₂ and CH₄ is not ideal given that the CH₄/CO₂ 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 CH₄) 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 paper strongly follows the argumentation in Rosing et al. (2010) in terms of very low CO₂ 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.

A. The manuscript has been rewritten and addresses all points mentioned by the reviewer. Here is the list of updates:

- to solve the haze formation, CO₂ and CH₄ radiative forcings are given (figure 1).*
- a discussion (section 4) wherein the substantial warming provided by methane levels is calculated. The table 2 summarizes alternative solutions to the FYSP with pCH₄ fixed to its present-day value (same thing for clouds).*

- table 1a, table 1b and table 2 summarizing boundary conditions used.*

Below are the answers to each specific points (the text has been changed to include corrections).

Specific comments

p 1510, l 15-16: This is not true, a significant part of the warming results from CH₄!

This is corrected in the revised manuscript (see section 4 and simulations performed, table 2).

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.

We agree with the reviewer, this sentence may appear inappropriate. Our initial idea was to present climate and carbon modeling results in the same paper. This paper being too long to be easily readable, it has been splitted in 2 parts (climate results for the 1st part and carbon results for the 2nd part). Carbon-climate simulations clearly suggest that an early Archean atmosphere poorly enriched in CO₂ is an unlikely solution whatever geological constraints tested. If the reviewer thinks that we have to remove this sentence, we will do it.

p 1510, l 20: 25
30% has been replaced by 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.

This sentence implicitly refers to the surface water. To avoid misleading this sentence is now changed (paragraph 1, section 1).

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

The word "critical" is removed (paragraph 2, section 1).

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.

Indeed these two papers use different methods to constrain the $p\text{CO}_2$. Rye et al (1995) use a thermodynamics approach and assume that the initial mineral was the greenalite ($\text{Fe}_3\text{Si}_2\text{O}_5(\text{OH})_4$). Sheldon thinks that berthierine was the precursor, not the greenalite (a mineral never observed in studied paleosols). Moreover Sheldon prefers to constrain the $p\text{CO}_2$ using the mobile/immobile elements during the weathering. Associated to hypothesis (kinetics of weathering, soil thickness, duration of soil formation,...), he quantifies the $p\text{CO}_2$ by a mass-balance approximation. We have let the text unchanged but if the reviewer thinks that is necessary, we can modify the text.

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

This is corrected in the revised manuscript

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.

This sentence has been removed. The methane effect is discussed (section 4 and Figure 1 for the radiative forcing).

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.

The introduction has been extended (part 1 and 2 are now associated), and title of the section has been modified

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.

References added (see comment reviewer 1)

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

The revised estimate by Goldblatt and Zahnle (Nature comment 2011) is now added (paragraph 3, section 1)

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.

Our initial sentence is removed; this point is discussed later (section 4).

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 CH₄ in the atmosphere. It is never mentioned, so that one would assume these are done without CH₄, but in the caveats (p 1522, l 26 to p 1523, l 1) haze formation at CH₄/CO₂= 0.5 is mentioned, so I guess they are done with CH₄. If so, the authors should be very careful when comparing to other studies without CH₄ since 900 ppmv will considerably contribute to the warming.

We added tables (table 1a, 1b and table 2) showing the pCH₄ used.

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.

The description is updated (paragraph 1 section 2.1)

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.

Values added (paragraph 1 section 2.1)

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?

We used the present-day value (point now mentioned).

We preferred to keep the diffusion rate at its present-day value, because we don't have enough constraints to adapt the heat transport (see paragraph 1 section 4).

p 1514, l 25 - p 1515, l 2: How well is the FOAM radiative transfer scheme calibrated for the very high CH₄ concentrations used in this study?

See answer to reviewer 1.

Furthermore, there is considerable uncertainty with respect to the continuum absorption of CO₂ at high CO₂ levels (Halevy et al. 2009). This does not apply to the relatively low CO₂ levels derived in this study, but since in reality CH₄ levels were probably much lower and CO₂ 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 CH₄ (see below).

To be honest we cannot answer to this question (a real quantification would take too long to be included in a review process, see Halevy et al. 2009 or Haqq-Mishra 2008). Regarding past studies, the treatment of CH₄ by the ccm3 radiative module (Figure 1) seems to be in agreement with Halevy et al. (2009) and Kiehl and Dickinson (1987). Here we suppose that the methane is not enough concentrated to absorb a part of the incident solar energy.

Reference added:

Halevy, I., R. T. Pierrehumbert, and D. P. Schrag (2009), Radiative transfer in CO₂-rich paleoatmospheres, J. Geophys. Res., 114, D18112, doi:10.1029/2009JD011915.

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

This is corrected in the revised manuscript and discussed in the section 4.

Reference added:

Voigt, A. and Abbot, D. S.: Sea-ice dynamics strongly promote Snowball Earth initiation and destabilize tropical sea-ice margins, Clim. Past, 8, 2079-2092, doi:10.5194/cp-8-2079-2012, 2012

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. Further-more, I doubt that differences in clouds are only significant for snowball Earth climates.

We agree with the reviewer, this sentence appears clearly too affirmative. Stevens and Bony study (2013) demonstrates these uncertainties. This sentence has been rewritten in the revised manuscript.

Reference added:

Stevens, B.; Bony, S. What Are Climate Models Missing? (2013) Science, Vol. 340, No. 6136. pp. 1053-1054, doi:10.1126/science.1237554

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

Done. See table 1a, table 1b and table 2.

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

The point is now mentioned.

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

Values added (paragraph 1 section 2.2)

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.

Pesonen's methodology is now described (paragraph 2 section 2.2)

From 3.5 to 2.75Ga, we assumed a theoretical paleogeography with two continents located to low and mid latitudes. Their respective surface evolves to respect the continental surface imposed by Rosing et al. 2010. In term of climate, this assumption is clearly marginal, the surface albedo being driven by the sea ice extend (point mentioned section 3.3).

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.

See answer to reviewer 1. Based on Andrea and Rosenfeld (2008) we now explain why CCN particles should be less abundant during the Archean (paragraph 3 section 2.2).

p 1516, l 26 - 1517, l 1: The description of how the shorter cloud lifetime is implemented in FOAM is confusing.

See answer to reviewer 1

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.

Due to number of processes already investigated we decided, in the initial paper, to use the same factor that Kump and Pollard (2008), i.e an intermediate value/mid-strength feedback. This choice is now discussed (paragraph 3 section 2.2).

We checked the validity of our clouds treatment (see answer to reviewer 1).

Unfortunately, we did not run additional simulations where we separated droplet size and cloud-conversion rate (as done in Goldblatt and Zahnle (2011)).

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?

The diffusion rate is held constant in all simulations (point mentioned in the revised manuscript)

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

We added a sentence explaining the initialization phase (paragraph 1, section 3)

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.

We now used the term "bifurcation", a term which refers to climatic states

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.

This point is incorrect. Indeed several GCM have a similar behavior (see Yang, et al, 2012 or Pollard and Kasting 2004). Several first order factors explain this variability (1) the sea ice/snow albedo, (2) heat transport. Second order factors are: geography (and its feedback on atmospheric/ocean dynamics) and topography.

Reference Yang, J., W. R. Peltier, and Y. Hu, 2012: The initiation of modern "soft Snowball" and "hard Snowball" climates in CCSM3. Part II: climate dynamic feedbacks. J. Climate, 25, 2737-2754, doi:10.1175/JCLI-D-11-00190.1

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?

The snowball Earth bifurcation seems to be an artifact. We have changed the text to explain this important point omitted in the initial version (see paragraph 1 section 3.2)

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).

We agree

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 state-ment 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.

This is corrected in the revised manuscript

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 simpler than GCMs (by parametrizing albedo in terms of temperature, for example)!

This is corrected in the revised manuscript. Indeed this sentence is misleading. Budyko and Sellers have published description of their EBMs in 1969, and shown the possibility of alternative stable climatic states for the Earth due to the albedo. Our initial sentence refers to the recent studies (notably Rosing et al, 2010) where the ice-albedo feedback was not included. This sentence is rewritten (paragraph 4 section 1)

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

We now used the term “bifurcation”, a term which refers to climatic states

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

References added

Walker et al. 1981 and Godd ris and Veizer 2000

p 1522, l 4-7: Mention the CH₄ concentration in the simulations.

Done

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 CO₂ is needed without CH₄? What is the sensitivity to sea-ice albedo parameters? The authors should run a few dedicated sensitivity experiments to explore these uncertainties.

A large set of new simulations have been performed, the table 2 summarizes the most interesting ones. We focused on warming induced by methane and clouds (see section 4). We also investigated the case of a low ice albedo (Run 7, table 2) to provide more details.

Without other warming factors, a reduced albedo could maintain the Earth unfrozen at 3.5Ga with 0.025bar of carbon dioxide instead of 0.056bar for a classical albedo. Hence, a change in ice albedo has an important influence in the surface temperatures and can limit the triggering of a snowball Earth. Unfortunately an ice albedo lower than 0.6 appears unlikely as shown by Warren et al. (2002). Moreover the global mean temperature (-10.5 C), associated to 0.025bar of carbon dioxide, cannot explain the temperate climate for the Archean Earth, and cannot maintain the long-term carbon cycle at its equilibrium.

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

Information added.

A reduced diffusion rate should increase the pole-equator gradient and limit the cooling in tropics. Up to now, the only one study investigating the FYSP with an ocean dynamics is CLIMBER (Kienert et al 2012). Unfortunately several problems seem associated to the use of CLIMBER (see reviewer 1).

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).

The color bar is misleading. The figure 5 shows temperature anomalies [standard run] minus [run with modified LOD or/and salinity]. For the figure A1, the southern hemisphere is colder (but the anomaly is positive so in red) when a faster rotation rate is imposed.

Figure 5 has been completely redone (now a red color corresponds to a warming)

p 1524, l 15-20: A comparison is very complicated indeed due to the different model de-signs 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 could 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 CO₂ partial pressures to other studies without CH₄ the authors should keep in mind that they add substantial amounts of CH₄ to the atmosphere.

We hope that changes in the revised manuscript are sufficient to answer to this comment. The section 4 deciphers respective impact of CCN clouds, methane and carbon dioxide

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.

Following the reviewer 2, we have changed the text and added a general discussion (section 4). Caveats and other studies results are now included in this section.

p 1524, l 22-26: Again, the role of CH₄ needs to be discussed, otherwise this sentence is very misleading.

Done

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

This is corrected in the revised manuscript

Technical corrections

The paper has been revised to remove grammatical and typo errors.

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.