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Climate of the Past
Discussions

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 #1

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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:

(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.

(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.

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. LeHir'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.

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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 CO_2 needed to keep out of a Snowball, but the assumption that there should have been fewer CCN's is still very speculative.

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.

(3) Probably cuing off of Rosing, the simulations are done with 900ppm of CO_2 and 900ppm of CH_4. The methane values are unsupportable, since you would get thick organic hazes at that ratio of CO_2 to CH_4. Further, it is not likely that the ccm3 radiation code in FOAM is valid at such high levels of CH_4 (it probably is OK up to 100ppm). The inclusion of unrealistically high CH_4 gives a misleading impression of how low CO_2 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 CO_2 or CH_4. Thus, the authors can just quote the equivalent CO_2 value based on the ccm3 radiation code itself, avoid the issue of unrealistic methane behavior, and state that the CO_2 could be brought down somewhat by substituting CH_4 (or better, H_2, see the Wordsworth Science paper) for some of the CO_2. My own estimate is that the equivalent CO_2 is something like 10000ppm, but the authors should check using their own calculations.

Sequential comments:

p2: Mention the Wordsworth Science paper on the H_2 greenhouse effect as part of the discussion of possible other GHG's that can play a role.

p3:The Kienart 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

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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.

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.

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.

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.

p8: Kiehl (spelling)

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.

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.

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.

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.

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?

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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.

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.

Interactive comment on Clim. Past Discuss., 9, 1509, 2013.