Response to RC1 by Aiko Voigt

The authors study the location of the Snowball Earth bifurcation in terms of atmospheric CO2 as a function of insolation in the range of 1361-1034 Wm-2. As the sun becomes stronger over time, the insolation range covers the time from today to 3600 Ma before present, meaning that the work studies the bifurcation as a function of time. The authors apply a model of intermediate complexity with a simplified atmosphere model in aquaplanet setup, which allows them to sweep through a broad range of insolation and CO2 values. Their two main findings are i) that for lower insolation values the critical CO2 decreases logarithmically as insolation increases but drops faster for higher insolation values, and ii) that the nature of critical states (defined as states before the runaway icealbedo feedback sets in) is different between low and high insolation. For low insolation values, the critical ice edge is located in the midlatitudes (termed the Ferrel state by the authors), whereas for higher insolation values it is located in the subtropics (the Hadley state). The authors ascribe this difference in critical states to the meridional gradient in insolation and wind-driven sea-ice transport. The text is well written and the graphics are of high quality (except for two minor questions, see below).

My main criticisms is the following. From reading the text it seems the authors suggest that critical states with a sea ice cover around 50% or with a sea ice edge equatorward of 30 deg were not possible. Yet, there are several studies that have found such states. The conclusion of the change in critical state dynamics thus seems not as robust as described by the authors. I also found that in some cases the comparison with previous studies seems a bit lopsided. I elaborate this below as part of my main comments.

Overall, however, this is a well conducted and well presented paper that addresses a question that was so far not studied. I am confident the authors can address my concerns and recommend minor revisions.

First of all we would like to thank Aiko Voigt for his very constructive as well as helpful comments and the overall positive evaluation of our manuscript. We really appreciate the effort!

Just for clarification, we do not suggest that states with around 50% global sea ice cover are not possible since our Hadley states fall into this category. The existence of states with a sea ice edge closer to the equator is strongly model dependent, and has also been questioned in a recent paper by Braun et al. (2022) on which the reviewer is one of the co-authors. Our model does not exhibit these states, but we agree that the (still unresolved) question of the existence of these states has to be discussed in the manuscript. By the way, most of the cases where the comparison seems “lopsided” appear to be due to simple misunderstandings, see our detailed response to the individual comments below. As far as these misunderstandings are due to the wording of our manuscript, we are going to improve the text accordingly, again see our replies below.

Main comments:

1. In the conclusion section (L350ff) the authors argue that critical states with a sea ice cover around ∼40% are not possible (the exact numbers are model dependent). The argument is made based on the Ferrel vs. Hadley states, and is allegedly supported by comparison to the work of Yang et al. However, when checking the figures in Yang et al. (2012a) I believe I found some inconsistencies with the authors' arguments. Specifically, Fig. 2 of Yang shows that there are stable states with a sea ice fraction of 50%, contradicting the statement that "... there are no stable states with global sea-ice fractions between ∼ 40% and ∼ 60% for a present-day continental configuration." Probably even more severe, Fig. 16b of Yang et al. (2012a) shows that there is a stable state with 70% sea ice cover for 90% insolation. In my understanding such a state contradicts the Ferrel-Hadley-state argument of the authors. There might be other inconsistencies with the Yang et al results.

Thank you for asking these critical questions. Concerning the first one, we think that it is unclear whether the state with about 50% global sea-ice fraction in Figure 2 of Yang et al. (2012a) will remain stable due to the rather short integration time of this particular model simulation. In fact, the quoted statement is derived from a sentence from Yang et al. (2012a, page 2719, left column) where they write: *In other words, it is likely that there are no stable states between ∼ 40% and 60% sea ice coverage during the initiation of the Snowball Earth; this phenomenon is further confirmed in the simulations with CCSM4 (Yang and Peltier 2012).* Thus there is certainly no inconsistency here.

The second comment is more interesting because Figure 16 in Yang et al. (2012a) refers to the dependence on initial states of the Yang et al. (2012a) results which we were not aware of. We will discuss and clarify this in the revised version of the paper. We do point out, however, that our statement holds for initialisation from warm climate states corresponding to the procedure used in our model simulations.

2. I am missing a discussion about the fact that critical states with sea ice margins quite close to the equator have been found in models, e.g., Voigt and Abbot (2012), Abbot et al. (2011, http://dx.doi.org/10.1029/2011JD015927) and Braun et al. (2022, https://doi.org/10.1038/s41561-022-00950-1). Overall, this makes me think that the changes in the critical state dynamics - although operating in the Climber model used here - are not as robust and fundamental as described by the authors.

As explained above, we are somewhat skeptical regarding the existence of these states. That being said, we agree with the reviewer that the issue should be discussed in the manuscript. We will do so in the revised version. The changes in critical state dynamics should at least be relevant for the majority of models not exhibiting stable waterbelt states; for models with waterbelt states, the Ferrel and Hadley states could be stable states at higher CO_2 concentrations (depending on solar luminosity), similar to the situation at higher solar luminosities where both Ferrel and Hadley states can be stable.

Other comments:

L10 and L145: Is the change in the CO2-insolation function related to the change in the critical state dynamics? This is not clear to me.

No, the change in the function is not related to the shift in critical state dynamics. As can be seen from Figure 2, for example, the shift occurs at about 90% of the present-day solar constant, whereas the downturn in Figure 1 is most pronounced beyond 95% of today's solar luminosity.

L27: It is unclear to me what you mean by "for even lower solar luminosities". What does "even" refer to.

Yes, we understand that the wording could be confusing. We will reword the sentence to make this clear.

L80: Pierrehumbert et al., 2011 (doi:10.1146/annurev-earth-040809-152447) compared Snowball initiation in three AGCMs in aquaplanet setup (their Fig. 4). These models did not include ocean and sea ice dynamics, but used the same coordinated setup. Also, Hoerner et al, JAMES, 2022 (https://doi.org/10.1029/2021MS002734) used an aquaplanet setup to study the impact of sea ice thermodynamics on Snowball initiation. Maybe these are interesting references?

We will include and discuss these additional references in the revised version of the paper.

L101: Some more discussions on the atmosphere model, its limitation and the impacts of its limitations would be desirable. For example, are the Hadley and Ferrel cell boundaries fixed in time, or can they move with the seasonal cycle? How does this impact the P-E patterns and hence snow on sea ice and surface albedo? Do the authors think that this matters? This would also be helpful for the wind argument made around L262 in the result section.

This is an important point. While the annual mean width of the Hadley cells in our simplified atmosphere model is fixed (as we had described it maybe somewhat too briefly in the manuscript), the boundary between the Hadley cells moves with the thermal equator, with a corresponding, but smaller shift in the boundaries between the Hadley and the Ferrel cells, see Petoukhov et al. (2000, Section 3.2). Thus the overall changes of the large-scale circulation with the seasonal cycle are represented in the model in principle. We will add this important information to the description in the revised version of the paper.

L111: The agreement with the Liu et al (2013) work seems cherry picking and in my view is a weak argument. There are other studies for which the agreement would be much lower, as in fact can be seen from Fig. 1 of the paper.

We agree. What we wanted to convey is that our model gives comparable results for the glaciation threshold to a more sophisticated model with similar cryosphere albedos. However, the sentence in this form was written before the full synthesis presented in Figure 1 was available. We will change this in the revised version of the paper.

Table 1: I would find it helpful if the S/S0 ratio could be included in the table, as the ratio is used in Figs. 1 and 2.

This is a good idea, we will add an additional column with the ratio to Table 1.

L140 and L193: The 0ppm CO2 value for today's insolation is consistent with Voigt and Marotzke, 2010, who found that removing all CO2 would lead to a Snowball in the coupled ECHAM5/MPI-ESM model (using present-day continents).

Many thanks for the hint, we will add this to the discussion in the revised version of the paper.

L147ff: I do not understand what the authors mean by baseline warming from water vapor. I also wonder how clouds are treated in Climber.

By "baseline warming" we refer to the effect that even in the rather cold, but not fully ice covered states there is evaporation and thus some greenhouse warming due to atmospheric water vapour. We will check whether we can reword the sentence to make this clearer.

The cloud module of CLIMBER-3α uses a two-layer cloud scheme (stratus plus cumulus) with the cloud fractions depending on humidity and vertical velocity, see Petoukhov et al. (2000, Section 3.4).

L162: Voigt et al., 2011, Climate of the Past showed that moving continents to the tropics cools the climate and facilitates Snowball initation. This is in line with the argument made by the authors and maybe worth including.

We will add this to the discussion in the revised version of our manuscript.

L175: I agree with the statement that sea ice dynamics was found to facilitate Snowball initiation. Yet I do not agree that previous studies robustly found that simplified oceans make Snowball initiation more difficult. There are at least three counter examples. Poulsen and Jacob (2004, doi:10.1029/2004PA001056) stated that ”The wind-driven ocean circulation transports heat to the sea-ice margin, stabilizing the sea-ice margin.”. Rose (2015, https://doi.org/10.1002/2014JD022659) also found a stabilizing role of ocean heat transport. This relates to the argument made in L215 regarding the lack of a full ocean. Voigt and Abbot (2012, https://doi.org/10.5194/cp-8-2079-2012) show explicitly that setting ocean heat transport to zero makes Snowball initiation easier, and they argue that this is related to the subtropical wind-driven ocean cells (see their Figs. 12 and 13).

You are absolutely correct, of course, and we should and will describe the respective effects of ocean and sea-ice dynamics separately and in more detail in the revised version, see also below.

L180: The study of Pierrehumbert et al., 2011 (see above) tested for albedo values in 3 models, showing that ice albedo differences are key.

We will add this study to the discussion of the impact of cryosphere albedos.

L198: I believe Lewis et al., 2003 used prescribed surface winds, because of which they could not make robust statements of the impact of sea ice dynamcics. See the discussion of the Lewis work in Voigt and Abbot (2012; page 3 left column).

We are aware that the model used by Lewis et al. (2003) uses prescribed surface winds and will add this to the discussion during revision of the manuscript.

L261: Is the fuzzy transition a result of seasonal averaging over fully ice covered grid boxes or does the model allow for partially ice covered boxes?

Our sea-ice model allows for partially ice covered grid cells. We will add this information to the model description in the paper.

L274: I am wondering about the role of the wind-driven subtropical ocean cells below the Hadley cells. These cells should be represented by the ocean model and are expected to work towards Snowball initiation (see my comment regarding L175).

This is a good point. We fully agree that ocean heat transport makes Snowball initiation more difficult, but preliminary analysis suggests that this effect cannot fully counteract the destabilising effects of sea-ice dynamics. We will expand on this in the revised version of the manuscript.

Fig. 1: I appreciate the very nice summary of previous modeling work in the figure. Some relevant studies seem to be missing, however. I suggest adding the results of Pierrehumbert et al. (2011), Voigt and Abbot (2012), Hoerner et al. (2022, https://doi.org/10.1029/2021MS002734) and Braun et al. (2022, https://doi.org/10.1038/s41561-022-00950-1). I apologize that these are all studies that I co-authored, I am listing them here since they are missing and I know of them. There might be additional relevant work.

Many thanks for these hints. We had decided against including the Pierrehumbert et al. (2011) results due to the lack of ocean and sea-ice dynamics, and we had not been aware of the papers published in 2022 at the time of submission. The ressults of these studies and the one by Voigt & Abbot (2012) will be included in the revised version of Figure 1.

Are Figs. 5 and 6 needed given the zonal symmetry and the zonal-mean plots in Fig. 7?

Yes, Figures 5 and 6 are somewhat redundant with Figure 7. Our intention was to illustrate the general climate states with maps, which are less abstract than the more aggregated Figure 7. And besides the meridional temperature distribution, these maps also show that the model indeed exhibits the zonal uniformity that is to be expected for an aquaplanet. We will explore different ways to combine the maps with Figure 7 or use Figures 5 and 6 to display seasonal variations.

Fig. 8: I do not understand the meaning of the legend in panel a and the color coding of the lines.

The one-dimensional energy balance equation can be used to attribute changes in surface temperature between different equilibrium states, in other words surface temperature differences of two climate states. In Figure 8, we compare the surface temperature differences between the Hadley states for the different time slices and the one at 900 Ma. Thus “1800 Ma − 900 Ma” in the legend is to be read as “1800 Ma *minus* 900 Ma”. We tried to explain this in the caption of the Figure, but will attempt to make this clearer during revision.