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Climate
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Discussions

Interactive comment on "Sea-ice dynamics strongly promote Snowball Earth initiation and destabilize tropical sea-ice margins" *by* A. Voigt and D. S. Abbot

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We thank the reviewers for their extensive and critical comments. Based on these we have substantially rewritten the manuscript to clarify our results and improve the comparison to previous studies. We hope that the revised manuscript answers their concerns. The major changes to the manuscript are:

1. Improved presentation and discussion of previous studies on sea-ice dynamics as requested by reviewer 1.
2. Clarified discussion of Jormungand hysteresis motivated by comments of reviewer 3.
3. Analysis of the sea-ice velocity tendencies predicted by the sea-ice dynamics scheme: Such an analysis has never been done before for Snowball Earth initiation. The analysis allows us to show that the equatorward sea-ice motion in low latitudes is mainly driven by the equatorward surface winds that are associated with the Hadley circulation. The destabilization of tropical sea-ice margins by sea-ice dynamics hence is fundamentally linked to the Hadley circulation.
4. New schematic figure that summarizes mechanisms that either stabilize or destabilze tropical sea-ice margins.

In the following we illustrate in detail how we take the reviewers' comments into account. Each reviewer is addressed individually, with the reviewers' comments in italic font, our answers in normal font.

1 Answer to reviewer 1

We thank the reviewer for pointing us to the previous studies by Lewis et al. (2003, 2004, 2007) and for requesting a more thorough discussion of these.

Reviewer comment: *Closely related papers discussing basically the same issue was published in recent years and the authors seem unaware of these.*

Response: We thank the reviewer for pointing out that the original manuscript did not sufficiently discuss the previous studies of Lewis et al. (2007) and Lewis et al. (2003) (Lewis et al. (2007) was shortly mentioned; Lewis et al. (2003) was not mentioned at all). We correct this great oversight of us and include both studies in the revised manuscript, referencing them first in the introduction section. However, our study substantially goes beyond the studies of Lewis et al. (2007) and Lewis et al. (2003). In particular, Lewis et al. (2007) were unable to conclude whether disabling sea-ice dynamics makes Snowball initiation more difficult or easier in terms of CO_2 because the effect on the critical CO_2 was sensitive to the prescribed wind forcing. Our study closes this gap. Also, Lewis et al. (2007) find that when sea-ice dynamics are included, sea-ice margins equatorward of 60N/S are unstable. This is in sharp contrast with coupled AOGCM simulations, which find stable sea-ice margins at 30N/S or even closer to the equator despite active sea-ice dynamics (Voigt and Marotzke, 2010; Voigt et al., 2011; Yang et al., 2012a,b). This difference between the model of Lewis et al. (2007), which uses an atmospheric energy-balance model with prescribed winds, and coupled AOGCMs, which resolve atmospheric dynamics, provides additional motivation to study sea-ice dynamics in a coupled AOGCM.

The reviewer also mentions the study of Lewis et al. (2004). We note that this study was not concerned with sea-ice dynamics but sea-ice thermodynamics. Nevertheless, we cite this study in revised manuscript in our discussion of sources of inter-model spread in Snowball Earth initiation.

Reviewer comment: *In addition, the "Jormungand" state is mentioned here: can the authors discuss if such a seasonally changing sea ice state is likely to occur with sea glaciers (thick ice) rather than relatively thin sea ice?*

Response: Several processes work in favor of thin sea ice in low latitudes, which is needed for the seasonal advance and retreat of the bare sea ice that is characteristic of the Jormungand state. First, the low albedo of bare sea ice allows large summer melting. Second, ocean heat transport leads to order of $10\,\mathrm{Wm}^{-2}$ heating of the low-latitude sea ice from below (Fig. 7b of Yang et al., 2012a). Third, sea-ice dynamics tend to spread the steadily-growing mid-latitude sea ice to lower latitudes, where it is melted. This spreading makes it harder for sea-ice glaciers to form in the mid-latitudes,

from where they possibly could invade the tropics. However, a final answer to the effect of sea glaciers on the Jormungand state requires a model that includes sea glaciers. Our model is limited in this respect because sea-ice thickness is restricted to about 8 m. These considerations are included in the revised discussion of the Jormungand state in the discussion section.

Reviewer comment: *Section titles: I wanted to suggest that sections 3, 4 and 5 are renamed so that the name reflects the objective of the run rather than the parameter values used in it. The first sentence of 4 and 5 may be a good place to start.*

Response: We adapt the section titles accordingly.

2 Answer to reviewer 2

We thank the reviewer for his support and helpful comments that lead us to clarify the manuscript.

Reviewer comment: *In the discussion section, the authors mention that their (2ppmv,85%) state is compatible with the covering of the continents by glaciers. They suggest that continents fully covered with ice might be compatible with the existence of a free water between 20N-20S. But 2 ppmv is so unrealistically low that such a state would most probably never have occurred.*

Response: We agree that because of volcanic outgassing atmospheric CO_2 is unlikely to fall to such a low level. However, the two main purposes here are first to show that a stable state with sea-ice margin at 10N/S and 85% sea-ice cover can exist in ECHAM5/MPI-OM once sea-ice dynamics are disabled, and second to demonstrate that disabling sea-ice dynamics lowers the critical CO_2 by a factor of 100 in ECHAM5/MPI-OM. In our opinion one should put less emphasis on the absolute CO_2 value that is needed to find a state with 85% sea-ice cover. More broadly speaking, the critical CO_2 that one finds in Snowball model simulations is sensitive to the choice of boundary conditions (exact value of solar constant, i.e. 95% or 96%, land surface albedo, land distribution, aerosols, non-CO_2 greenhouse gases etc.) and model physics (strength of sea-ice dynamics, cloud-radiative effects, exact value of sea-ice albedo, i.e. 0.45 or 0.5, etc.). The uncertainties in these factors translate to uncertainty in the critical CO_2, so the state with 85% sea-ice cover could also be realized for higher (and hence arguably more realistic) CO_2 values. However, the relative effect of sea-ice dynamics on the critical CO_2 is independent of the uncertainty in the absolute critical CO_2. That said, our manuscript is most interested in the relative importance of sea-ice albedo, sea-ice dynamics and ocean dynamics on the critical CO_2, while less emphasis should be put on the absolute CO_2 values. We have adapted the introduction and discussion of the revised manuscript along these lines.

Reviewer comment: *There are several typo errors in the discussion section.*

Response: Thanks. The typos are corrected.

Reviewer comment: *What happens if a high sea-ice albedo is prescribed in a simulation without sea-ice dynamics? Does the snowball CO2 threshold rises significantly? If not, this would definitely confirm the key role played by sea-ice dynamics.*

Response: The reviewer raises an interesting issue that tackles the question of whether the effect of changing the sea-ice albedo and the effect of disabling sea-ice dynamics add up linearly in terms of the critical CO_2. For disabled sea-ice dynamics, it is clear that the critical CO_2 is larger for a high bare sea-ice albedo than for a low bare sea-ice albedo. Moreover, we expect that disabling sea-ice dynamics also decreases the critical CO_2 when sea-ice albedo is high. Consequently, the critical CO_2 for a setup

with high albedo and disabled sea-ice dynamics will be in between the critical CO_2 values for a setup with high albedo and enabled sea-ice dynamics ($\simeq$ 500 ppmv) and a setup with a low albedo and disabled sea-ice dynamics ($\simeq$ 2 ppmv). If the effects of decreasing the sea-ice albedo and disabling sea-ice dynamics added up linearly, the critical CO_2 would be around 5 ppmv, but it is unclear to what extent the assumption of linearity is correct. Answering this question would require us to repeat several thousands of years of coupled AOGCM simulations with high bare sea-ice albedo and disabled sea-ice dynamics. This would represent a serious computational effort. Since a low bare sea-ice albedo is more appropriate than a high bare sea-ice albedo (see the discussion section), we feel that the question of linearity is interesting but not essential to the Snowball debate. That said, investigating the (non-)linearity is worth doing but beyond the scope of this study. We therefore leave this issue for future work, but discuss it in the revised manuscript.

3 Answer to reviewer 3

We thank the reviewer for his extensive and critical comments. His comments have motivated us to clarify the presentation of our results and conclusions. We hope to thereby avoid any misconceptions or misunderstandings of our results, in particular concerning the (lack of) Jormungand hysteresis.

Reviewer comment: *The authors revisit in this paper an additional mechanism for hysteresis that was previously suggested to exist in the ECHAM atmosphere only model which was referred to as involving a so-called Jormungand bifurcation that depended upon the difference between the albedo of bare sea ice and that of snow-covered sea-ice. In the current paper the new analyses demonstrate that the previously found* *hysteresis was an artifact of the exclusion of oceanic and sea-ice dynamical processes. The current paper then includes special pleading to the effect that the same source of hysteresis might be recovered as a consequence of processes not explicitly included in their model. From the perspective of this referee, the paper would be improved by entirely removing these poorly motivated speculations.*

Response: Our study shows that ECHAM5/MPI-OM exhibits Jormungand-like states once sea-ice dynamics are disabled. This is important because we for the first time demonstrate that the Jormungand mechanism also operates in a coupled AOGCM with continents, albeit only when sea-ice dynamics are disabled. Furthermore, our study shows that these Jormungand-like states however are not true Jormungand states because there is no hysteresis associated with them. Moreover, we would like to clarify the reviewer's comment. Our simulations do not prove that the Jormungand hysteresis is an artifact of the exclusion of oceanic and sea-ice dynamical processes in Abbot et al. (2011). Such a statement is not made anywhere in the manuscript, and we also do not intend to suggest such an interpretation of our simulations. In contrast, we show that there is no Jormungand hysteresis in ECHAM5/MPI-OM also when ocean dynamics and sea-ice dynamics are disabled, which implies that ocean dynamics and sea-ice dynamics do not cause the lack of the Jormungand hysteresis in ECHAM5/MPI-OM (Sect. 6). There are several possible reasons for the lack of Jormungand hysteresis in ECHAM5/MPI-OM. First, we used continents while Abbot et al. (2011) used aquaplanet simulations. Second, ECHAM5 showed Jormungand hysteresis in Abbot et al. (2011), but because ECHAM5 in stand-alone mode (i.e., not coupled to an ocean GCM as in the present study) does not track snow on sea ice, Abbot et al. (2011) had to artificially specify areas equatorward of 20N/S as snow-free. This is not the case in the present study. Moreover, ECHAM5 in stand-alone mode does not allow partially sea-ice covered grid boxes while ECHAM5/MPI-OM does. Both issues might explain why ECHAM5 in Abbot et al. (2011) showed Jormungand hysteresis but ECHAM5/MPI-OM does not. Third, it is still possible that other coupled AOGCMs exhibit Jormungand hysteresis, which would mean that the Jormungand

hysteresis is model-dependent. Motivated by the reviewer's comment the revised manuscript contains a substantially rewritten and clarified paragraph about the Jormungand hysteresis in the discussion section.

Reviewer comment: *The paper would also be significantly further improved by listing the specific differences between the co-ordinates of the bifurcation point found by Yang et al for both CCSM3 and CCSM4 under conditions of modern continental configuration and by including results from ECHAM/MPI-OM for the modern distribution of the continents with the reduced value for the bare sea ice albedo so that direct comparison with the Yang et al results is possible. This would greatly help in sharpening-up the remaining issues in the literature and would lead to a much stronger publication.*

Response: The revised discussion section will include a more explicit comparison between the ECHAM5/MPI-OM results and the CCSM3/4 results of Yang et al. (2012a) and Yang et al. (2012b). We would like to note that comparing the results of ECHAM5/MPI-OM and CCSM3/4 is somewhat hampered by the use of different continents (Marinoan for ECHAM5/MPI-OM; modern for CCSM3/4), which is also mentioned in the manuscript. We agree that having the models run with the same continental configuration is desirable. CCSM3/4 runs with Marinoan continents are planned for the near future (Jun Yang, personal communication), which will enable a cleaner model comparison and allow us to better understand sources of reported model differences. This will be made more explicit in the revised manuscript. However, we beg to differ with the reviewer's request to repeat our simulations with modern continents and low albedo of bare sea-ice. Continents have been shown to be important for Snowball Earth initiation (Voigt et al., 2011). Modern continents are mainly located in high Northern latitudes and hence are not appropriate when modeling the Marinoan Snowball Earth periods that were characterized by low-latitude continents. That said, we are looking forward to the upcoming CCSM3/CCSM4 simulations with Marinoan continents.

Reviewer comment: *Page 2470, Figure 5 Bottom: The figure caption is not consistent with the figure legends. From the figure caption, it is clear that the red line is supposed to indicate the influence of sea-ice dynamics, whereas in the figure legend the red line indicates the influence of melting and conversion. Confusions also exist for concerning the black and blue lines.*

Response: Corrected.

Reviewer comment: *Page 2453, section 3: In Yang et al. (2012a, Figure 7), the dynamical and thermodynamical sea-ice thickness tendencies in CCSM3 had been analyzed in detail, i.e. the sea-ice momentum budget has been fully analyzed. The language in the text at this point suggests that this has not previously been done for any model. In low- and mid-latitudes, the results of Yang et al. and in this paper are consistent. But for high latitudes of both hemispheres, Yang et al. demonstrated that the thermodynamic contribution is positive whereas the dynamical contribution is negative, meaning sea-ice is growing in these regions and is being transported equatorward by dynamics. The results of this paper (Fig. 4) suggest that both the thermodynamic and the dynamic contributions are positive. Since the results of Yang et al. are more reasonable on physical grounds this issue needs to be fully addressed prior to publication. Might the inconsistency be due to the cut-off imposed upon sea-ice thickness, that it be constrained never to exceed 8 m in ECHAM5/MPI-OM?*

Response: We now more thoroughly discuss and compare our analysis of the sea-ice thickness tendencies with that in Yang et al. (2012a). Our findings (Fig. 4) are consistent with those in Yang et al. (2012a) in low- and mid-latitudes. The difference in high latitudes is likely caused by the restriction of sea-ice thickness to 8 m in ECHAM5/MPI-OM. The revised manuscript discusses this point. Moreover, the revised manuscript includes an analysis of the sea-ice velocity tendencies generated

by the sea-ice dynamics scheme (i.e., the sea-ice momentum budget). Such an analysis has never been done before (in contrast to the reviewer's comment this was not done in Yang et al. (2012a)) and represents a major extension of the manuscript. We show that the equatorward sea-ice motion in low latitudes is mainly driven by the equatorward surface winds that are associated with the Hadley circulation. The destabilization of tropical sea-ice margins by sea-ice dynamics hence is fundamentally linked to the Hadley circulation. Importantly, this destabilizing effect competes with the stabilizing Jormungand mechanism that also relies on the Hadley circulation. Moreover, our analysis of the sea-ice velocity tendencies shows that the equatorward sea-ice motion is supported by viscous spreading of thicker mid-latitude sea ice towards thinner low-latitude sea ice. We discuss that the magnitude of this viscous spreading likely differs from model to model because of different tuning parameters in the sea-ice dynamics schemes. This motivates us to discuss how model differences in sea-ice dynamics can lead to strong differences in the models' Snowball Earth initiation behavior.

Reviewer comment: *Page 2456, section 5: This section purports to demonstrate that ocean heat transport has a much smaller effect on the critical* CO_2 *level and the critical sea-ice cover relative to that of sea-ice dynamics. However, in the zero ocean heat transport experiments, sea-ice dynamics has already been disabled. A possible explanation for the weak effect of ocean heat transport is that disabled sea-ice dynamics has already driven the critical sea-ice cover significantly equatorward to the very high degree (85%), so that above this point the climate system enters a very unstable region; further reduced ocean heat transport therefore has nearly no effect on the critical sea-ice cover. If a group of experiments with zero ocean heat transport and active sea-ice dynamics were performed, the effect of ocean heat transport on the critical sea-ice cover would be directly revealed. These experiments should also be performed prior to publication, further strengthening what could be a strong contribution.*

Response: To clarify the reviewer's statement we find that including ocean heat transport has no effect on the critical sea-ice margin but halves the critical CO_2. In general, our result on ocean heat transport is consistent with earlier of Poulsen et al. (2001) and Poulsen and Jacob (2004) that used the AOGCM FOAM. These studies showed that ocean heat transport makes Snowball initiation more difficult. Poulsen et al. (2001) and Poulsen and Jacob (2004) did not determine the magnitude of that CO_2 lowering. To our knowledge, our study is the first that quantifies the effect of ocean heat transport on the critical CO_2 in an AOGCM. The halving of the critical CO_2 might seem small in magnitude, but this is not inconsistent with what we indirectly infer from Poulsen and Jacob (2004). From Tab. 1 of Poulsen and Jacob (2004), one can infer that ocean heat transport decrease of the critical total solar irradiance (for fixed CO_2) by at least 2%. Assuming a planetary albedo of 0.3, this corresponds to a radiative forcing of $4.8\,\mathrm{Wm^{-2}}$, which is approximately the radiative forcing associated with a halving of CO_2 (assuming the modern logarithmic dependence of the radiative forcing of CO_2 still holds when CO_2 is around 2-4 ppmv).

Our new analysis of the sea-ice velocity tendencies shows that ocean dynamics not only create ocean heat transport but also create substantial poleward drag on the low-latitude sea ice. By this drag ocean dynamics work against Snowball initiation. This effect has not been acknowledged before and is now described in the revised manuscript. Also, this effect must clearly distinguished from the influence of ocean dynamics via ocean heat transport. In order to isolate the effect of ocean heat transport on the critical CO_2 and sea-ice cover it is hence necessary to first disable sea-ice dynamics. We also note that the previous studies Poulsen et al. (2001) and Poulsen and Jacob (2004) did not include sea-ice dynamics, so they focused on the effect of ocean heat transport in the same way as our study does.

Reviewer comment: *Page 2458, section 6: In two groups of experiments with and without ocean dynamics, the authors found that there is no sign of the Jormungand*

bifurcation. Then, the authors concluded that the lack of the Jormungand bifurcation in ECHAM5/MPI-OM is not caused by ocean dynamics and the associated ocean heat transport. Abbot et al. (2011) concluded that the Jormungand bifurcation is due to the high albedo contrast between bare and snow-covered sea-ice, which also exists in the simulations of ECHAM5/MPI-OM of this paper. Do the results of this paper suggest that the high albedo contrast is not the reason for the Jormungand bifurcation, or whether the high albedo contrast can generate the Jormungand bifurcation or not depends on the model employed?

Response: We rephrase the according paragraph according to our answer to the first comment of the reviewer.

Reviewer comment: *Minor issues*

Response: We have fixed the mentioned sentences. We also discuss more thoroughly the studies of Yang et al. (2012a,b).

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