The authors investigated the evolution of climate and ocean circulation towards a modern snowball Earth in MIROC4m, and found some phenomena that are quite different from those in previous studies, especially when the Earth has entered a hard snowball. I like the study in that their model can be continued for thousands of years after a snowball Earth has been initiated, which was not possible in CCSM3; the model would just crash in most cases (Yang et al., 2012; Liu et al., 2013). It is unclear whether this was achievable in ECHAM5/MPI-OM (Voigt and Marotzke, 2010). Stable snowball simulations are certainly possible in ICON-ESM (Ramme and Marotzke, 2022), but it has not been used to investigate in detail the evolution of oceanic and atmospheric circulation. Therefore, there is an opportunity to find something new from the simulations done by the authors using MIROC4m. However, to my understanding, the important phenomena found by the authors so far are mostly artefacts due to inappropriate settings in the model. These problems will be listed in detail below and because of which, I think they will have to redo some of the experiments and the corresponding analyses.

1. They found that there would be a strong nearly hemispherically symmetric MOC (Fig. 5c) even if the thickness of sea ice is more than 200 m thick (Fig. 4). I think this is due to the unrealistically large wind stress felt at the ice-ocean interface. This wind stress will cause large poleward Ekman transport off the equator and thus strong upwelling at the equator, which will drive strong and deep MOC when vertical stratification is absent. This MOC is an enlarged version of the wind-driven subtropical cell (called STC), which is only ~500 m deep under normal conditions (e.g., Fig. 3d). The authors recognized the limitation in this stress but probably did not realize how much their results would be affected by this drawback. I do not know under how thick sea ice the ocean should not feel the wind stress anymore, but sea ice of 200 m thick (like ice shelf around Antarctica) will certainly not move with wind anymore. Thus, they should better re-do the TSI094 and TSI091 simulations by fixing this limitation, in order to provide to the readers realistic results.

   When fixing the limitation on wind stress at the ice-ocean interface, it is probably also necessary to make the sea ice stagnant. This is still unrealistic (thick sea ice moves slowly) but would be better than the spatial distribution of ice thickness shown in their Fig. 4a (TSI094); one would expect that ice is thicker over the high latitudes than over the low latitudes as in Ashkenazy et al. (2014).

2. Another peculiar phenomenon they found is the net precipitation at the equator as well as the annual mean Hadley circulation that rises at the equator (I think the

two are related and can be considered as one). This phenomenon is opposite to what was found in the previous study by Abbot et al. (2013). However, this is also an artefact in my opinion because they set the land surface to be glacier once the ocean is completely covered by ice. When they do this, the land surface will have smaller surface albedo (Fig. 4b) and thus higher surface temperature (not shown but can be inferred). Moreover, the land surface will become an infinite source of water vapor. This is why the land surface has a strong net evaporation (Fig. 14) while the ocean has a net precipitation. A reasonable guess is that this also causes the air to rise over land and sink over the ocean, the latter will produce a strong temperature inversion over the ocean. Therefore, I do not feel that this temperature inversion should be attributed to the turbulent coefficient in the atmospheric boundary layer, and indeed, their test with a different coefficient could not remove the inversion (Fig. 14).

Another effect of the warm land surface is to shift the rising branch of the annual mean Hadley circulation to the north of equator, clearly seen in Fig. 12. Therefore, it is not actually a good idea to set the land surface as glacier when the sea ice closes off at the equator, just letting snow to accumulate on the land surface (i.e. do not do any special treatment) is probably more realistic. The authors do not need to worry about the glacier formation on the tropical lands, they will remain thin after even a few thousands of years because the net precipitation rate is small in a hard snowball Earth. That means, the authors need to redo the simulations without setting the land surface to glacier in order to show to the readers proper picture of atmospheric circulation.

3. If the authors are willing to redo the simulations, the authors may want to look at how the snow cover changes with time over both land and sea ice; how the ocean stratification and MOC evolve, both the timescale and pattern could change significantly from those shown in the current manuscript; the gradual evolution of atmospheric circulation and how long it takes to reach equilibrium.

4. Although land surface is assumed to become ice once the ocean is completely covered by sea ice, the land seems to have much lower albedo than the ocean. This is the major reason that the results here are very different from previous simulations. Abbot et al. (2013) did not include any continent explicitly, so they avoided this complexity. In your case, the snow is hard to accumulate on land, which creates a positive feedback that makes the land even warmer. If you prescribe the land as ice with thick snow, the results may look similar to previous

modeling results. I am not asking for more sensitivity test but something you can discuss.

5. Fig. 7 shows something interesting. Around year 1280, sea ice starts to grow near 30°S while the higher latitudes are still having a net melting. This process seems to trigger the runaway effect, why does this happen?

Other Comments

1) Please remove the statement about biogeochemical changes in the abstract as readers would expect much more from the manuscript by reading the abstract.
2) L16: "iron formation" to "banded iron formation" since iron formation could have many other forms and mechanisms.
3) L20: please explicitly state that the change of solar luminosity with time was estimated from solar models (i.e., not geological records) and provide relevant references.
4) L22: "Thus, it is …", I cannot see the logic from the context why "thus" should be used here.
5) L24: "modelingcan" -> "modeling can"
6) L38: "AOGCM" -> "AOGCMs"
7) L39-40: "reconstructions of the continental distribution" is repetitive
8) L57-59: In my opinion, although Ramme and Marotzke (2022) provided a nice demonstration of the ocean circulation during the snowball termination, the sea ice in their snowball state was quite thin so that the freshwater layer was easily eroded away. The simulations in Zhao et al. (2022) provide another perspective.
9) L98: is the shortwave albedo described here the mean of visible and near-infrared wave bands?
10) L99-101: Is snow aging considered over both land and sea ice in MIROC4m? This is important for explaining the different results between your model and other models since your model results seem peculiar. Also, I assume that the sea-ice albedo is thickness dependent and the value you provided represents the maximum.
11) L105-106: This is quite surprising because many models use sigma layers near the bottom because of its large variation in bathymetry and z coordinates near surface for the small variation of surface height.
12) Fig. 2: it will be useful to show the evolution of oceanic heat transport here; the shallow MOC is usually called STC (as mentioned above) in the field of physical oceanography; the deep MOC I guess, is driven by winds in the same way as STC,

which is fundamentally different from the deep MOC in TSI100 and TSI096. Although the deep MOC in the latter is also maintained by wind driven upwelling, it's sinking is due to density anomaly. The authors may want to make it clear whether the sinking is due to density anomaly or downward Ekman pumping.

13) Fig. 3: is there atmosphere-ocean heat exchange at year 1450-1499?
14) Fig. 5: what do the contours show in panel f.
15) L243&L332: I think Abbot et al. used an albedo of 0.6, why do the authors think it was 0.7?
16) L267-270: The explanation provided here is very unlikely. The ocean temperature below the permanent sea ice is always below 0°C whether the AABW cell is strong or not. This is clearly demonstrated in Fig. 2 of Yang et al. (2012). I would think the high snow albedo over sea ice is more likely the reason. This can be tested but I will not ask that much.
17) L289: A counterpart is missing for "than" in this sentence.
18) L291-293: there seems to be a grammatical error around "relate"
19) L302: "but" does not seem to be an appropriate conjunction word here.
20) L310: It should be more specific how your hydrological cycle contradicts with the geological record, not obvious to all readers.
21) L310-313: I don't understand why vapor condensation can induce a strong temperature inversion near the surface since condensation gives off large amount of heat to the surface.
22) Fig. 14: It will be useful to also show surface temperature here.

Reference (all the other references can be found from the reference list of the manuscript)

Zhao, Z., Y. Liu, H. Dai (2022), Sea-glacier retreating rate and climate evolution during the marine deglaciation of a snowball Earth, Global and Planetary Changes, 215, 103877, doi: 10.1016/j.gloplacha.2022.103877