



EGUsphere, referee comment RC2  
<https://doi.org/10.5194/egusphere-2022-811-RC2>, 2022  
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## **Comment on egusphere-2022-811**

Anonymous Referee #2

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Referee comment on "The implications of maintaining Earth's hemispheric albedo symmetry for shortwave radiative feedbacks" by Aiden R. Jönsson and Frida A.-M. Bender, EGUsphere, <https://doi.org/10.5194/egusphere-2022-811-RC2>, 2022

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Jönsson and Bender explore changes in albedo, radiative fluxes and cloudiness in order to improve the understanding of the hemispheric symmetry of the planetary albedo and its possible changes in a warming climate. This is performed by investigating output from the CMIP6 in combination at some points with satellite retrievals. The topic of hemispheric symmetry of the planetary albedo is an exciting and highly debated one, in particular in light of possible changes in a warming climate. The study is of interest to the readership of EGUsphere. It is written in excellent English and the figures are in good quality.

The analysis is thoroughly conducted and broad in scope. In fact, my most important remark is that there is so much material that at multiple times I was a bit lost in understanding as to how a particular result allows to conclude about the causes for changes in hemispheric difference in planetary albedo.

The Discussion section is excellent, but does not really discuss the results in light of the literature. It would rather be better as part of the Introduction, and then the discussion of the results could refer to it. I do not provide a specific suggestion for shortening the results sections, but I propose the authors consider moving some of the material to an annex to streamline the discussion.

Besides this, I only have a number of specific remarks.

I72 It is regressing the global mean temperature against the top-of-atmosphere radiation imbalance (the effect forcing is the y-axis intercept only)

I88 I find this definition throughout the manuscript puzzling, since now all signs for CRE are the opposite ones compared to the all-sky and clear-sky differences. I think this definition requires that in Fig. 2, Fig 3 etc the reader is reminded about this difference in definition.

I154 (Fig. 1 and subsequent similar figures) – it would be useful to colour the numbers in the scatterplots by the colour used for the corresponding lines in the line plot to allow to make the association at least vaguely.

I159 I propose it might be better to use the same y-axis scaling in all panels

I173 I do not understand Fig. 3b. The three bars for each model should add up. Why is that not the case? also: Clarify in Caption that this is the difference between mid and PI

I202 Would it maybe be interesting to express precipitation and e-p in energetic units for comparison to the SW fluxes? Are the authors sure about no mistake for the models that substantially cool the NH high latitudes between mid and end?

I209 What is cause and what is effect is not fully clear. It may also be that after sea ice melting, clouds are much warmer if connected to the warm ocean rather than cold sea ice. Maybe reformulate to "this is most likely related to"

I212 To me it is not clear enough why Fig. 6b,e are not largely redundant with Fig. 6a,d

I215 What exactly are the "conditions" if not extent of sea ice?

I222 I would formulate the other way around, y-axis plotted against x-axis. Clarify that

cloud fraction is from MODIS, not CERES.

I245 "within" rather than "close to", I guess, since many models have lower values.

I315 This "model dependence" I do not understand. Of course the models show different results, so the results are model-dependent. What exactly is meant, a specific influence of the dynamical core of CESM?