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Comment on egusphere-2022-811

Anonymous Referee #1

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-RC1, 2022

Review of "The response of hemispheric differences in Earth's albedo to CO2 forcing in coupled models and its implications for shortwave radiative feedback strength."

This manuscript frames itself as exploring the possibilities of two future scenarios: one where the hemispheric albedo symmetry persists, and one where it is does not. Unfortunately, there is no clear delineation between these two regimes in the model results. Looking at the model spread in hemispheric albedo difference changes, it appears to be a normal distribution centered around -3 W/m2 (Figure 1). There are a few cases overlapping 0 W/m2, but these may simply be due to chance. All models show an initial negative perturbation to hemispheric albedo difference owing to a reduction in clear-sky albedo in the NH high latitudes. For some models, this is partially offset by increases in NH cloud albedo (termed a local compensation). For other models, the NH albedo reduction is matched by a reduction in SH albedo – mostly by clouds in the latitude range 30-60S – termed a remote compensation. For the remaining models, there is little compensation by clouds and the hemispheric albedo difference simply persists. It does not appear that there is any strong correlation between the magnitude of hemispheric symmetry change and SW cloud feedback (as shown in Figure 8), making it difficult to conclude anything about the role hemispheric albedo difference plays for climate projection uncertainty.

It is hard to gauge what we, as readers, are to learn from this study. It seems the conclusions are limited beyond acknowledging there is a large spread in model behavior surrounding clouds, which is already well known. If anything, this work would seem to suggest that models have no inclination for maintaining a hemispheric albedo symmetry, which makes sense given that they do not generally reproduce the observed symmetry (as shown in Supplementary Figure S1). The authors acknowledge that no physical mechanism has been proposed for why the hemispheric albedos should remain balanced, so it is perhaps not surprising that the models are unconstrained for their own hemispheric albedo differences. Some of the relationships examined in this manuscript between the cloud changes and other physical processes in the models may be useful to the scientific community, but I found the interpretation questionable at times (I have added details to these issues below). In its present form, I must recommend this paper be rejected and returned to the authors.

Major issues

Eyeballing the 'end' period in Fig 1a, it seems like the models suggest a normal distribution of asymmetry changes centered around -3 W/m2. Those models that come in at 0 W/m2 change seem to do so by chance. I attempted to go through the various tables and figures to determine if the models that start with a symmetric albedo (Figure S1) are the same ones that have a small 'End' – 'PI' hemispheric albedo difference, but I couldn't find such a relationship. Do the models that have a small perturbation change to warming at the end of 150 years have any consistent relation to their initial hemispheric albedo difference? Is there any change in the distribution across models of hemispheric differences with warming?

Line 128: "While models agree on clear-sky albedo reductions in the NH in response to warming, the spread in magnitude of total albedo reductions points to differences among the models in whether clouds serve to either amplify or reduce the total albedo reduction in the hemispheric mean." There is very little agreement in the magnitude of clear-sky albedo change in response to warming (Fig 2b). Are the authors arguing that clouds determine how much sea ice is lost? How do we know that is the case? Comparing Figs 2a and 2b, it appears that the spread in total albedo change at 90N is smaller than the clear-sky change. Wouldn't such a result suggest that the clouds are generally offsetting the clear-sky response to minimize the change (like the local compensation discussed later)?

Lines 156-157: 17 models amplify and 16 reduce. There are 34 models… so 1 has no significant response? Looking at Fig 3a, it appears several of these bars are almost unreadably small. Is it really only one model where SW CRE change is not statistically distinguishable from zero?

Line 209: "Planetary albedo is reduced in the Antarctic sea ice zone (Figure 6a); this is most likely the result of increasing liquid-phase precipitation reducing the sea ice surface albedo, and decreasing snowfall that otherwise would stabilize sea ice albedo." Why not simply a result of changing temperature or ocean circulation? I struggle to understand from the results shown how we can conclude the phase of precipitation falling on sea ice is the "most likely" cause of the albedo changes there. I see that SSTs are brought up in section 4, but I think it would be valuable to bring these changes into the discussion in section 3.3.

Line 211: "This allows the sea ice albedo feedback to affect the SH polar climate in models where SH extratropical SW CRE increases more strongly; the result can be seen in increased SW radiative heating at the surface (Figure 6b, e)." How do we know causality here? I don't follow how Figure 6 demonstrates the SH polar changes' impact on the extratropical response.

Section 3.3: I struggle to follow the argument of the poor correlation between sea ice extent and changes in extratropical SW CRE changes. Why are the authors only looking at changes in maximum sea ice extent? Why not some time-integrated sea ice extent measure? Wouldn't the sea ice minimum be more interesting because a larger retreat during summer would have impacts on surface fluxes that could change the clouds and circulation patterns nearby? All the changes in clouds have been annual averages, so why compare them with a seasonally dependent measure of sea-ice?

Line 244: "…meaning that the perturbation in asymmetry due to strong forcing in all models 150 years after the onset of abrupt CO_2 forcing is close to the interannual variability seen in the past 20 years of observations." If all models are close to the interannual variability, what does that tell us? How do we reconcile that result with the discussion around Figure 1?

Line 255: "When the difference between NH and SH $\Delta(\alpha_{clear} - \alpha)$ is larger, asymmetry is more effectively maintained." Is this true? Eyeball estimates in Figure 8c don't show a clear signal. Is this plotted somewhere else or has a correlation been computed?

Given that many of the comparisons involve differences between models, differences across hemispheres, differences between all-sky and clear-sky fluxes, and differences in time, it may be helpful to readers to define a consistent use of language. For example, increase & decrease could mean value changes (keeping track of the sign), while amplify and reduce could mean magnitude changes (absolute value). It would also be helpful to define early on what latitude bands the authors mean when using terms like subtropics, extratropics, mid-latitudes, polar, Arctic, and Antarctic. Finally, having grid lines (or at least a zero-line) for Figures 1 and 2 would make examining the sign of the changes easier.

Minor issues

Line 163 "on both the the degree" -> "on both the degree"

Figure 4a – is the colorscale reversed here? How do the lines peaking over +10 W/m2 have an average of -1 W/m2? The caption text suggests they are the same variable differencing the same time periods. It doesn't match Figure 5 either.

Figure 4b-f are the bounds too narrow on these plots? Where are models 9, 7, and 1 in panels c-f?

"We henceforth use the difference in 30-60° S area mean SW CRE between the 'End' and 'Mid' periods as an indicator of the impact of cloud albedo contribution changes on TOA albedo in the SH extratropics among models." Is 30-60S SW CRE well-correlated with the total SH SW CRE change? In other words, is it fair to focus on this region because variability here corresponds to the total variability we are concerned with (the remote/SH albedo changes)?

"Note also that SW CRE at higher latitudes (> 60° S) also becomes more negative consistently in models with SW CRE increases in the SH extratropics." Is poleward of 60S considered extratropics here?

"net poleward transport of moisture away from the SH extratropics (☐30-50° S) to the polar region (> 60° S)" Now extratropics is 30-50S?

"Atmospheric moisture content increases in the SH (Figure 5a) as clouds are lost and the atmosphere is warmed." -> This reads as if the cloud loss helps *cause* the increase in atmospheric moisture, which I am guessing the authors did not mean to imply.

Figure 7 – colorbar is flipped again?

Line 268: "These two possibilities, local or remote compensation, would also mean that SW radiative feedback strengths are either strongly positive or somewhat negative, respectively." Isn't this flipped? Remote compensation has the strong positive SW radiative feedback.

Line 290: "role in determining the the observed" -> "role in determining the observed"

"Although tropical clouds and albedo seem to play a secondary role in determining the observed hemispheric albedo symmetry on time scales longer than a year, this should also be taken into account in understanding hemispheric albedo symmetry-maintaining mechanisms that involve the extratropics, as it can mean that some of the compensation offered by extratropical albedo reductions in one hemisphere can be buffered by tropical albedo increases, which may require more substantial high latitude albedo reductions to maintain hemispheric albedo symmetry." -> should be separated into multiple sentences for clarity

Appendix B and Figures B1-B3 are not referenced anywhere in the text.