For the submission: "The response of hemispheric differences in Earth's albedo to CO_2 forcing in coupled models and its implications for shortwave radiative feedback strength" (https://doi.org/10.5194/egusphere-2022-811)

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Below: responses by the authors are in standard text, and the quoted reviewer comments are italicized.

Reviewer 2

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.

We would like to thank the reviewer for their critical reading and comments on our manuscript. We especially appreciate the reviewer's suggestion in focusing the manuscript in order to communicate our main points more effectively. With that, we recognize the need to guide the reader in a better way. Specifically, we think that the suggestions to move many of the details of the analysis to the supplementary material as well as to introduce some of the literature material in the Discussion section in the Introduction section are very helpful, and for this we are grateful. Beyond this, we have also changed the title of the manuscript in order to more effectively and concisely convey its focus.

We feel that the reviewer's opinions shared on our manuscript have greatly helped to improve it. With these suggestions in mind, we have revised our manuscript and would like to invite the reviewer to read.

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

l72 It is regressing the global mean temperature against the top-of-atmosphere radiation imbalance (the effect forcing is the y-axis intercept only)

This has been corrected to refer to the net TOA radiation imbalance as the regression variable (lines 77-79) in the revised manuscript; we thank the reviewer for catching this.

l88 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.

We have used the CRE convention of clear-sky minus all-sky fluxes, which gives SW CRE to be negative (in order to be associated with reduced absorption of SW radiation and thus cooling). We do understand the potential for confusion and agree that the reader should be reminded of how changes in SW CRE correspond to changes in absorption/reflection, and have adjusted the figure captions to remind the reader of this definition, as well as its introduction in the Section 3.2. We thank the reviewer for suggesting to remind the reader of the direction of effects.

l154 (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.

We agree that using consistent color scales throughout the text would make the association easier to make, and have adjusted color scales throughout the manuscript to be consistent with Section 3.1.

l159 I propose it might be better to use the same y-axis scaling in all panels

We agree with the reviewer and thank them for this suggestion; this has been implemented in the revised manuscript's Figure 2.

l173 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

The reviewer is correct that the bars should add up, and thank them for catching this mistake; the original figure had errors in the data processing, and we did not catch this. However, this figure has been changed to a different form in the revised manuscript in order to make the analysis easier to comprehend. That the difference is between 'Mid' and PI has been clarified in the new Figure 3's caption, and we thank the reviewer for this suggestion.

l202 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?

We agree that energetic units would make more sense for this comparison, since the argument is based on heat transport considerations, and have adjusted Figure 4 accordingly. While no mistake was found in the calculations for the previous figure, since we focus only on SH processes in Section 3.3, we have opted to quantify only area averages for the relevant SH regions in Figure 4 in the revised manuscript rather than zonal mean profiles.

l209 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"

It is true that cause and effect are difficult to distinguish here, and have rewritten Section 3.3 in order to emphasize that the changes in SH climate variables and heat transports that we find are associations. While the sentence that this comment refers to has been removed in the revised manuscript, we have taken the wording into account in the rewrite, and thank the reviewer for this comment.

l212 To me it is not clear enough why Fig. 6b,e are not largely redundant with Fig. 6a,d

We agree that Figure 6's profiles are largely redundant, and have opted to remove Figure 6 and combine its most relevant components (scatter plots showing the change in 30-60° S mean SW CRE change against > 60° S Antarctic changes) into Figure 4. We are thankful to the reviewer for pointing this out.

l215 What exactly are the "conditions" if not extent of sea ice?

For a given sea ice extent, surface albedo may vary and thus indicate the presence of albedo feedbacks in effect, which we find to be occurring more strongly in models with greater SH extratropical cloud reductions. However, we agree that sea ice is the primary condition to investigate when considering Antarctic albedo and that further checks would be useful in addressing this, which Reviewer 1 pointed out and suggested; to this end, we have also plotted asymmetry changes against annually integrated sea ice extent and sea ice extent maxima, which we found to not have a strong control on the differences in asymmetry between 'Mid' and 'End' (see Supplementary Figure S7). We thank the reviewer for posing this question.

l222 I would formulate the other way around, y-axis plotted against x-axis. Clarify that cloud fraction is from MODIS, not CERES.

The suggestion to plot the present-day values on the y-axis has been taken into account in the revised Figure 5, and cloud fraction has been attributed properly to MODIS in the revised manuscript (in the Methods and materials section, 2.1, and in the caption of Figure 5).

l245 "within" rather than "close to", I guess, since many models have lower values.

This sentence contained a mistake in comparing the interannual variability within the observed albedo symmetry time series; this has been corrected in the revised manuscript. The responses are an order of magnitude larger than perturbations on the interannual timescale, and are therefore significant (lines 222-225). We are thankful to the reviewer for pointing this out.

l315 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?

We agree that this formulation is unclear and that the statement of model dependence is trivial, and have opted to remove this sentence; we thank the reviewer for commenting on this.