

I thank the authors for responding to my original comments. Most have been resolved; however, there was a misunderstanding related to my comment:

L253: The use of a local T2m is non-standard, and does not correspond to the feedback quantification model by Cess and Potter 1988. Perhaps the authors could offer some explanation here, and a description of what impact this change has on the results, and their interpretation?

My objection was not to the use of T2m versus surface temperature; instead, I was pointing out that the *local* temperature (i.e. the value at each grid point) is not the same as that defined by Cess and Potter. It should really be the *global* mean temperature that is used to normalize the local albedo change, but in practice others have used the NH land mean temperature.

Reply:

Thank you very much for your comment. Indeed, there was an initial misunderstanding concerning your original comment. To answer your question about the use of local T2m: Indeed, you are right in that the feedback quantification model by Cess and Potter 1988 was using large scale temperature mean, as you pointed out many recent studies use NH means. We chose to use local T2m and then average our computations in the end, because the special conditions of the WMO stations (clear cut grass) will not be comparable with the climatic conditions of, lets say the vegetation of the NH. Therefore comparability would be questionable in any case. We use very specific point data and therefore we can not just insert large scale temperature dynamics into our equation. The resulting interpretation would be extremely vague. We decided to focus our study on the comparison of local dynamics in observations and reanalysis , but wanted to give also a broader picture to the reader, as to how the geographical distribution of our values is like. We state that our results in fact are not to be seen as a Northern Hemisphere impact analysis but rather as a contribution to reanalysis improvement. As for that, we decided to use local T2m, where its evolution should fit to the underlying vegetation and which can be adapted to our reanalysis experiment.

As to your question about the impacts of that choice: Since albedo changes in our stations are much more dramatic than in model or satellite grid cells, using geographically "smoothed" temperature data would eventually lead to a much stronger impact of albedo changes on temperature changes. As discussed above, this would have no meaning however, since a "grass only" world is unrealistic. On the other hand, linking albedo changes to local T2m changes and then geographically average the response makes more sense in our case. That said, our "grass only"

experiment was mainly used to show that reanalysis do in fact a decent job in representing snow albedo feedbacks and react in the right physical way to changes in their parameters. We now added a paragraph in the discussion section, summarising the above points. See line 550-560 now.

L154: delete ((

R: Deleted

L162-181: I recommend moving the information about the ERA-LG simulation to a separate subsection called "Idealized simulations", rather than in the same paragraph as the description of ERA-L, under the heading "Reanalysis Data".

R: Very good idea. We implemented it like you proposed.

L320-331: The aerosol results are very speculative, and given that no evidence is being shown, I recommend postponing this material for a future contribution, where the authors can fully demonstrate the process.

R: We think that going into detail and showing evidence would distract from the main story of the manuscript. We follow your advice and move the paragraph into the supplement.