

Interactive comment on “Spring snow albedo feedback over Northern Eurasia: Comparing in-situ measurements with reanalysis products” by Martin Wegmann et al.

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Received and published: 2 March 2018

L166: It is not clear whether the authors themselves performed the ERAI-LG simulation, or if this is an available reanalysis product from ECMWF? If the latter, please provide a reference. If the former, please provide much more information about how the experiments were configured and performed. This is pivotal to be able to assess whether the differences between ERAI-L and ERAI-LG are, in fact, explained by the land cover, and not some other confounding variable(s).

A: Thank you very much for this valuable comment. Indeed, a more detailed explanation about this experiment was missing. We now include a new paragraph, starting in

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line 166.

L197: by "attributed to ocean areas" I assume that the authors mean that they were coastal sites, and that the predominant land cover type in the corresponding ERA grid cell was ocean. Is that correct? Perhaps a clarification is required here.

A: Thanks for the comment. Indeed that was what was meant. We changed the text accordingly. See line 210 in the new document.

L210: Given that observational in-situ data are available 1964-2015, and reanalyses are reliable at least over the satellite era, some justification is required here for why the period 2000-2013 was selected for the study (especially since 2009 data are missing almost everywhere, so nâLj13).

A: Thanks for the comment. Data availability of this in-situ radiation dataset is limited to 2000-2013, at least that is the timerange that was supplied by the institute. We added this information in the text. See line 225 in the new document.

L232: Is there any sensitivity to the grid cell extraction method? For example, another approach would be to use a "nearest-neighbor" remapping; would this change any answers?

A: We checked for nearest-neighbor remapping and in fact the results did not change in any meaningful form.

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?

A: Thank you for your comment on using 2m temperature. Using 2m temperature has multiple reasons. The original definition is using "surface air temperature" which is also remarked by Cess and Potter 1988: "Here, and in the remainder of this paper, we employ surface temperature, rather than surface air temperature, as an indicator of

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surface climate. The reason for this is that in their study of climate feedback processes Cess et al. [1985] found that these processes are more appropriately defined in terms of surface temperature. A further benefit, with respect to GCM inter-comparisons, is that GCMs explicitly calculate surface temperature, whereas they differ in their definitions of surface-air temperature." However, Cess and Potter 1988 already point out a crucial point, which is comparability. In a perfect world, near surface temperature would be just the very thin first layer above the surface, however comparing a broader network of stations with reanalyses, this variable is not available. Therefore, we used the closest possible to "near surface" temperature available for us. That said, using T2m is rather standard by now, as studies by Fletcher et al. 2015, Xiao et al. 2017 and Kevin et al. 2017 show. Nevertheless, we added an interpretation of the results concerning using 2m temperature. See line 280 in the new document. Xiao, L., Che, T., Chen, L., Xie, H., & Dai, L. (2017). Quantifying Snow Albedo Radiative Forcing and Its Feedback during 2003–2016. *Remote Sensing*, 9(9), 883. Kevin, J. P. W., Kotlarski, S., Scherrer, S. C., & Schär, C. (2017). The Alpine snow-albedo feedback in regional climate models. *Climate Dynamics*, 48(3-4), 1109-1124.

L259: Surely a major limitation of estimating α_{land} using MAMJ when $Sc=0\%$ is that there are many locations for which Sc is always > 0 in MAMJ. What do the authors use for α_{land} in those cases? And how much "more realistic" do the authors find that using MAMJ is, compared to August? My suspicion is that the values should be very similar.

A: Thanks for the comment. Maybe the text is a little bit unclear at this point, but since we have daily data, we look for days where snow is zero. And in June there is always at least one day without snow. If there are multiple days without snow, we use an average value of snow free albedo. That said, we tested using August values, and the results are very similar. Nevertheless, taking values out of MAMJ seems less artificial, which we mean by saying "more realistic". We adjusted the wording. See line 289 in the new document.

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L289: Perhaps the authors have some additional evidence (spatial maps, for instance) to support the claim that the higher correlation for MERRA2 is due to aerosol deposition? If so, then I think it needs to be shown, because on its own Figs.2c-d do not really allow us to draw any meaningful conclusions about physical processes. Also, on L396 the authors state that it is the vegetation schemes in MERRA2 and ERAI-L that decrease the snow albedo; is this contradictory to the point about aerosol deposition?

A: Thank you for this interesting comment. Indeed, we probably put too much emphasis on this point and toned down our wording. Vegetation schemes are responsible for a longterm albedo reduction but not so much on a day to day scale. We now include a small paragraph where we investigated the day-to-day variability in combination with aerosols. See line 322 in the new document.

L306: The issue of grid vs point comparisons is a very common problem. I wonder if anyone has attempted to use spatial interpolation (e.g. kriging) on the 40+ station observations to produce a "gridded" snow depth product?

A: We are working on a gridded product (with 400 stations as input) and it will hopefully be ready for research in 2018. Stay tuned for follow up paper by the first two authors.

L308: I am not sure where the evidence is presented to support the claim about snow-free albedo?

A: We deleted this statement.

L399: I am confused by Figs.4-5. In Fig.4b it is shown that the mean SNC term ($\alpha_{snow} - \alpha_{snowfree}$) is similar for the stations and ERAI-LG, and in Fig.4f the mean α_{snow} values are also similar. Yet, in Fig.5a, the $\alpha_{snowfree}$ values are hugely different (for which I could find no explanation), so how can Fig.5a be correct, and yet still produce similar SNC in Fig.4b?

A: Thank you for your comment. SNC is not only albedo contrast (see equation 1), however SNC is a product defined by albedo contrast (Figure 4e) and snow melt sensitivity

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(4d). Albedo contrast is “so” similar because the differences in the y-axis of snow free albedo are actually relatively huge, but absolutely rather small in the grand scheme of things. That said, albedo contrast between stations and ERAI-LG is still roughly 0.05.

L402: If the observed snow-free albedo is similar to that for grass, why does the ERAI-LG simulation still do so badly in this quantity (Fig.5a)?

A: Thank you for this comment. An explanation can be found in L574. Changing the vegetation scheme only helps to make the radiation characteristics over snow covered grid points more realistic. Snow free albedo is still as seen from satellite and is not dynamic in reanalyses. We made sure to underline that point.

L424: The sentence ending "overestimated complete snow cover albedo cancel each other out." seems to be highly important; however, it was not clear which panels of Fig.4/5 are supposed to show this cancellation? Also, what is "complete snow cover albedo"?

A: Indeed, additional information was missing, the sentence was not complete. We changed the wording and added information. See line 460 in the new document.

Supplement Figs.5-6: I recommend centering the colorbar labels in the bins, so that it is clear which color corresponds to which vegetation type.

A: We improved the Supplement Figures 5&6

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-266>, 2017.