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Comment on tc-2021-290 by Daloz et al.

Anonymous Referee #1

Referee comment on "Land–atmosphere interactions in sub-polar and alpine climates in the CORDEX flagship pilot study Land Use and Climate Across Scales (LUCAS) models – Part 1: Evaluation of the snow-albedo effect " by Anne Sophie Daloz et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-290-RC1, 2021

General comments

The paper focuses on a snow-albedo sensitivity index (SASI), which describes interannual variations in surface net shortwave radiation resulting from anomalies in snow cover. The behavior of SASI is intercompared in a set of ten regional climate models (RCMs) from the LUCAS study, and it is also compared to satellite and renalysis data.

It is shown that (1) SASI most typically peaks in the melting season; (2) there are substantial differences in the simulation of SASI among the models as well as between the models and observations; (3) the choice of the land-surface model can influence the intermodel differences in SASI substantially, but differences in other parameterizations such as convection or planetary boundary layer processes can also be important; (4) and the differences in SASI are more related to differences in (standard deviation of) snow cover than downwelling solar radiation in the models.

The coordinated LUCAS simulations represent a valuable dataset, and documenting the intermodel differences in snow conditions and the level of model-vs-observations (dis)agreement is a worthy effort. I think there is potential for this paper to be published in The Cryosphere, but there are issues that should be carefully considered by the authors. In particular, I'm wondering if SASI is the most natural starting point for this paper. Would it not be better to start the story from the basics, that is the simulation of snow cover itself? Indeed, the motivation for considering SASI should be outlined more clearly. E.g., why is it important to compare the snow-related variability in the surface energy budget, when the systematic differences in snow cover between the models exceed the interannual variability?

Major comments

1. If/when this is the first snow-focused study on the LUCAS simulations, I think you should not start from a derived quantity (SASI) but more from the basics: document the snow cover and perhaps also the snow water equivalent in the simulations. Plot(s) like Fig. 2 would do the job.

There are two reasons why dicussing the systematic snow cover differences would be important. The first point is their large effect on the surface energy budget and hence the simulated climate. For the sake of the argument, one could define a ``snow radiative forcing (SRF)'' or ``snow radiative effect (SRE)'' as a difference to the snow-free case:

$$SRF = - SW f_{sno} \Delta\alpha$$

This is similar to the definition of SASI in Eq. (1) of the manuscript (and with the same notation), excecpt that the standard deviation of snow cover $\sigma(f_{sno})$ is replaced by the mean value f_{sno} for the given calendar month. Since the systematic intermodel differences in f_{sno} are often substantially larger than the corresponding differences in $\sigma(f_{sno})$ (which can be easily inferred from Fig. 5), it follows that the intermodel differences in SRF exceed those of SASI.

Second, showing the monthly climatology of snow cover in the simulations would help to explain much of the variations in SASI. Intuitively, interannual variations in snow cover for a given month/region are small in the cases in which the climatological snow cover fraction is close to either 1 or 0. The former applies e.g. to northern parts of Scandinavia in winter, and the latter to most regions in late spring and summer. Conversely, the interannual variations in snow cover (and hence also the values of SASI) are more likely to be large when the climatological snow cover fraction takes intermediate values. This applies to two cases. First, in the snowmelt period, snow cover fraction decreases rapidly. Therefore, interannual variations in snowmelt timing can result in large year-to-year variations in snow cover. Second, in the more southerly regions, snow cover in winter may be thin and intermittent (i.e., snow comes and goes). Consequently, due to variations in weather conditions, the interannual variations in snow cover can be large.

2. In definining SASI, the assumption of a surface albedo difference of $\Delta\alpha$=0.4 between snow-covered and snow-free land seems somewhat arbitrary. It is also not fully clear what is meant by snow cover fraction: does it include only the snow cover on land, or also snow on vegetation? Judging by section 2.1.3, the LSMs have different approaches, but it is not obvious from the text, what this means for f_{sno}. Please try to clarify this.

I suggest that, to evaluate the robustness of your results, you compare the standard deviation of albedo assumed by the SASI formula (i.e., $0.4\sigma(f_{sno})$) with the actual standard deviation of monthly-mean albedo values $\sigma(\alpha)$. The monthly value of albedo could be calculated based on the values of downwelling and upwelling (or downwelling and net) SW radiation. Note that $\sigma(\alpha)$ may also be influenced by albedo variations due to other factors than snow (e.g. vegetation), but I would assume that in the winter/spring seasons, the interannual variations in surface albedo are overwhelmingly dominated by variations in snow conditions.

3. The explanations regarding the reasons for the intermodel differences remain rather vague. Perhaps it is not possible to go very deep with an ``ensemble of opportunity'' like the LUCAS simulations, where you have a very sparse matrix of RCM-LSM combinations. Nevertheless, I think the analysis could be clarified by considering more explicitly the three ``groups'' of models you have available (the WRF group with 3 models, the CCLM group with 3 models, and the RegCM group with 2 models). I would suggest one extra figure for each of the groups, showing the monthly (January-June) values of downward SW radiation, climatogical snow cover f_{sno}, its standard deviation $\sigma(f_{sno})$ and SASI in different rows, and the three regions in different columns.

Most of this information is already available in the figures, but not in a form in which the behavior of the models within each group can be compared easily. If you think this is too much for the main paper, placing these figures in the Supplementary material would be an option.

Minor comments

1. lines 34-35: I think that characterizing SASI as ``the radiative forcing due to the snow-albedo effect'' is misleading. At least to me, the most natural definition for the radiative forcing due to the snow-albedo effect would be the difference to the snow-free case (see major comment 1). If you want to call SASI a radiative forcing, then something like ``radiative forcing associated with interannual variations in the snow-albedo effect'' or ``radiative forcing associated with snow-cover anomalies'' is suggested.

2. lines 63, 66, 195, 652: The SASI index is not defined in Xu and Dirmeyer (2011), and neither in Xu and Dirmeyer (2013) (Journal of Hydrometeorology, pages 389–403). The correct reference would be Xu and Dirmeyer (2013) (Journal of Hydrometeorology, pages 404-418).

3. lines 70 and 143: please add a reference for this statement (the impact of snow cover on precipitation is not obvious to me).

4. lines 75-76. Positive feedbacks amplify anomalies. Negative feedbacks act to damp them.

5. line 81. Radiative forcing associated with snow cover anomalies? See the first minor comment.

6. lines 85-87. Other studies could also be mentioned. See, for example,

Diro, G.T., Sushama, L. and Huziy, O. Snow-atmosphere coupling and its impact on temperature variability and extremes over North America. Clim Dyn 50, 2993-3007, https://doi.org/10.1007/s00382-017-3788-5, 2018.

7. lines 115-116. It is not necessary mention the GRASS and FOREST experiments here (they are already mentioned on line 97-98).

8. lines 149--157: I find this description unclear. Given the definition of SASI (Eq. 1), the key questions here are how do the models define the snow cover fraction f_{sno} and whether or not snow on vegetation is included in f_{sno}.

9. line 174: You also use the snow cover from ERA5-Land (in Fig. 5).

10. line 180: The use of ``two different thresholds (20% and 50%)'' immediately raises questions like why do you apply two thresholds, which of them do you apply in your figures, or is it perhaps case-dependent.

11. lines 190-191. To be sure, is this ``MODIS masking'' applied to all model results throughout the paper?

12. line 197: ``net radiation'' is wrong. It should be the downward radiation. But perhaps this is just a typo?

13. line 197: I suppose standard deviation refers here to the interannual variation of monthly-mean values. Please be explicit about this.

14. lines 221-222: ``then decreasing when snow starts melting'' gives the impression that SASI reaches its maximum value right before the ablation period. But a comparison of SASI (in Figs. 2, 3), snow cover (Fig. 5) and SWE (Fig. S1) rather gives the impression that SASI peaks in the middle of the ablation period (which is what I would also assume based on physical reasoning).

15. lines 236-238, 247-249. Regarding the role of the atmospheric model, I am not sure if there is anything special about the convective or
planetary boundary layer parameterizations as such; changes in other physical parameterizations such e.g. the cloud scheme could also be important. In general, I would expect that the impact from the atmospheric model comes mostly through the effects of precipitation and temperature. (the latter influencing both the phase of precipitation and snow melting). Have you looked at the differences in temperature and precipitation between WRFc-NoahMP and WRFa-NoahMP? Judging by Fig. 2 I would guess that WRFc-NoahMP either precipitates more, or features a colder climate in winter/spring than WRFa-NoahMP?

16. line 241-242: "WRFa-NoahMP shows an earlier poleward migration of high SASI values compared to WRFb-CLM4.0". A plain language
translation of this would be that snow melts earlier in WRFa-NoahMP!

17. lines 267--268: ``The maximum in SASI marks the transition between the accumulation and ablation periods''. In my understanding, the transition between the accumulation and ablation periods refers to the time when snow cover and SWE are at maximum. Your results suggest that SASI increases when snow starts to melt, and it is at maximum when snowmelt is well underway, i.e., definitely after the snow cover/SWE maximum. See also minor comment 14.

18. line 271: the later maximum of SASI for ERA5-Land than satellite data for East Baltic and Scandinavia is consistent with later snowmelt in ERA5-Land (as seen from Figs. 5 and S1). Incidentally, could that be related to the different data periods (1986-2015 vs. 2003-2015)?

19. lines 273-276: A problem with this explanation is that East Baltic has lower elevations than East Europe.

20. lines 323-324: It is not clear what is meant with ``a common bias between the models''. Systematic differences between the models,
or systematic differences between the models and observations?

21. line 339: ``rate of snow melting'' or ``timing of snowmelt''? Also, specify explicitly that with melting, you refer here to the reduction
of snow mass (SWE).

22. line 368: Radiative forcing associated with interannual variations in snow cover?

23. line 370: replace ``albedo'' with ``surface net SW radiation''.

24. line 382: Please specify what you mean with a ``common bias regarding snow cover''. Overestimation? Underestimation??

25. line 387: How can you infer this from the available dataset, when there are presumably many other differences between the LSMs?
What one could probably say is that there was no systematic difference between the PFT-dominant and PFT-tile models.

26. The figures and table(s) should be organized in such a way that they support a visual comparison of simulations with the same model
components (see major comment 3). Figure 2 is well-designed in this respect: the models/simulations within the WRF group, the CCLM group and the RegCM group can be easily compared. Please apply this ordering of simulations also in Figs. 5, 6 and S1 and in Table 1. In addition, Figures 3 and 4 could be improved by using, for simulations within each group, the same color but different symbols for the different simulations.

27. Fig. 2. As noted in the first major comment, I strongly recommed adding a similar figure for snow cover. Also, similar maps for the
interannual standard deviation of snow cover fraction and the downwelling SW radiation would be useful for visually explaining the behavior of SASI. (If you think this increases the number of figures too much, the use of Supplementary material is always an option).

28. Fig. 4. The y-axis labels are wrong (it is correlation, which is unitless. Also, I'm not fully convinced this figure is necessary in the first place.

Technical and language corrections

1. line 107: ``Section 4 the last sections''

2. line 111: Delete the latter ``simulations''.

3. line 159: Replace ``counts'' with ``includes''?

4. line 165: Replace ``first very'' with ``very first''.

5. lines 318-322: This could be streamlined. ``In January, WRFa-NoahMP simulates consistently the least snow cover in the three regions (0.4 for Scandinavia, 0.3 for East Baltic, and 0.1 for East Europe), while WRFa-CLM4.0 simulates the largest snow cover (1.0 in all three regions).''

6. Fig. 2. Can anything be done to the strange land mask in CCLM-TERRA?