Cryosphere: Manuscript tc-2021-290

Land-atmosphere interactions in sub-polar and alpine climates in the CORDEX FPS LUCAS models: I. Evaluation of the snow-albedo effect

Reviewer #1:

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 reanalysis 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?

Thank you for your interest in our work and your constructive comments. We agree with you that the organization of the article could be modified starting with the basics, as you suggested, looking at the simulation of the snow cover. This point was discussed several times by the authors ending in the decision to start with SASI, however, the organization you suggested makes more sense so we will modify it in the new version of the article, also clarifying the motivation for this work from the start.

In general, we believe that the modifications you suggested can be addressed. They will improve the quality of the article as some parts deserves more explanations, such as the Introduction better explaining the motivation of the article or the derivation of SASI with satellite observations. I will provide preliminary answers to the general and minor comments below. A more detailed answer to your comments will be provided with the new version of the manuscript at a later stage of the review process as we are currently working on your reviews.

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:

$$SRF = - SW \, fsno \, \Delta\alpha$$

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 σ(fsno) is replaced by the mean value fsno for the given calendar month. Since the systematic intermodel differences in fsno are often substantially larger than the corresponding differences in σ(fsno) (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.

Yes, we agree with the reviewer, the organization of the article will be modified in the next version of the manuscript, starting with a comparison of the representation of snow cover. Furthermore, thank you very much for the detailed explanations provided here. We will do our best to address this point, better including this background information in the new version of the article.

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 fsno. Please try to clarify this.

Thank you, we agree that the different approaches of the LSMs should be clarified, we will modify the text including the information we can find on the subject. Furthermore, yes, we agree with the reviewer that the value of 0.4 as the average albedo difference between snow-covered and snow-free surfaces might not be ideal. The influence of the choice of this value can be tested and will be tested before we submit the manuscript again. Depending on the results we find we will keep or update this value and describe these sensitivity tests in the new version of the article.

I suggest that, to evaluate the robustness of your results, you compare the standard deviation of albedo assumed by the SASI formula (i.e., $0.4\sigma(fsno)$) 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.

Thank you for this suggestion, we will do our best to realize this evaluation and include its results in the manuscript.

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 fsno, its standard deviation $\sigma(fsno)$ and SASI in different rows, and the three regions in different columns.

Thank you for this suggestion. It was indeed difficult to find a good way to talk about the intermodel differences as, as suggested by the reviewer, there is a very sparse mix of RCM-LSM combinations. However, the comparisons of WRF/CCLM/RegCM groups is feasible and could bring valuable information in the article.

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.

Thank you, the supplementary material seems like the best option in this case. We will add these figures in this section of the manuscript.

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.

Thank you for the suggestions, the description of SASI will be modified in the different part of the article mentioning it using one for your suggestions.

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).

Thank you, this mistake has been corrected in the text.

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

Thank you for pointing this out, references will be added for the sentences you suggested.

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

This sentence will be reformulated to clarify the effect of the feedbacks.

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

The description of SASI has now been reformulated following the suggestions from the minor comment #1.

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.

Thank you, this reference has been added to the manuscript.

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

Thank you, the part of the sentence mentioning GRASS and FOREST experiments has been removed.

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 fsno and whether or not snow on vegetation is included in fsno.

This description will be modified in the new version of the text.

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

Thank you, this has been corrected in the text.

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.

Thank you, this will be corrected in the next version of the text.

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

This point will be clarified in the next version of the text.

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

Thank you, this will be corrected in the next version of the text.

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

This point has now been clarified in the text.

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).

Yes, thank you this will be modified in the next version of the article.

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?

Yes, thank you this will be clarified in the next version of the article.

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!

Thank you, this sentence has been reformulated to clarify the this point.

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.

Yes, thank you this will be modified in the next version of the article.

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)?

Thank you, this will be clarified in the next version of the article.

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

Thank you for the clarification, this will be corrected in the next version of the article.

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?

Thank you, this point has been clarified in the text, reformulating the sentence.

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).

Thank you, this point has been clarified in the text, reformulating the sentence.

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

Thank you, this point has been clarified in the text.

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

Thank you, albedo has been replaced with surface net SW radiation.

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

Yes, this point has been clarified in the text.

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.

Yes, we agree with your comment. This sentence has been modified to include your suggestion.

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.

Thank you, these suggestions will be included in the new version of the figures.

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).

Thank you, these suggestions will be included in the new version of the manuscripts as we are currently working on it.

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.

Thank for you for noticing this, we will modify the y-axis and see if this figure is necessary in the new version of the article.

Technical and language corrections

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

Thank you this mistake has been corrected in the text.

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

Thank you, the latter simulation has been removed.

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

Thank you, correction made in the text.

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

Thank you, this is corrected now.

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)."

Thank you, this sentence has been modified in the text.

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

We will do our best to address this point in the next version of the article.