The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-15-EC1, 2019

Interactive comment on "Uncertainty in predicting the Eurasian snow: Intercomparison of land surface models coupled to a regional climate model" *by* Da-Eun Kim and Seon Ki Park

Thomas Mölg (Editor)

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Received and published: 10 April 2019

Dear colleagues,

Two referees have evaluated the manuscript, and I would like to thank both of them for their thoughtful comments. I would also like to thank the authors for their detailed response and suggestions of how to address the referee comments in a revised MS.

The fact is that both referees express major concerns, and I can understand their main comments and doubts about the present version of the study. In this regard the author responses are not convincing enough in my opinion. I see some remaining, substantial

weaknesses in connection with the following.

(1) Both referees criticized the lack of depth in motivation and in novelty of the study. The authors responded along the lines that several previous studies conducted a similar type of comparison, but that is not a convincing argument. Although I agree entirely with the authors that not every study can/must be embedded in a large international project to feed the motivation part, it would be necessary that some novel or additional aspects are added to a new study (e.g. as suggested in item (2) below). Without doing so, the manuscript will receive little attention.

(2) One of the important goals and motivations will not be met. The authors state that they intend to examine "different physical processes in the LSMs", and I agree that this would be worthwhile and would add flavor to the study that would diminish the weakness expressed in (1) above. However, the whole study is strongly descriptive, and only occasional sentences on qualitative interpretations of what physical processes could be acting in the background are contained. To elevate the value of the present study, the authors should analyze the snow energy budgets and internal snow processes (if resolved in the LSM) by a coordinated quantitative design. Only this would enable to understand how the different snow cover distributions evolve with different LSMs.

(3) The referees also suggest that the meteorological drivers above the snow surface must be evaluated, and I fully agree in particular with regard to item (2) above (it is another important ingredient for "understanding"). The authors are right by saying that not every study can conduct its own measurements and measure all components (e.g., radiation terms). Yet there are several global data sets that cover at least the basic drivers like air temperature and precipitation, based on observations. I would see ERA5 as the right choice if the former would not exist, but the authors should try first to evaluate their results against measurements of atmospheric drivers (not reanalysis).

Since (1)-(3) are substantial, I am sorry to say that I discourage submission of the proposed revised manuscript at this stage. Please note that none of the negative points raised here are a critique of the basic value of your work, but they are meant to help you improve the details of your study and, therefore, increase the likelihood that your final version of this work will receive more attention and have more impact. In this context, I hope that the reviews and my comments provide you with valid input to work on an improved version of the study, for which you are welcome to consider TC again as a publication platform.

Thomas Mölg

Handling Editor & Co-Editor-In-Chief TC

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