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The Cryosphere Discussions

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

Anonymous Referee #1

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Remarks to the Authors

Review of "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

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General comments:

In this study, the authors performed inter-comparison of the performance of four land surface models (LSMs) simulating snow depth, fractional snow cover, and albedo in

Eurasia. These four LSMs are the Unified Noah land-surface model (Noah LSM), the Noah LSM with multi-parameterization options (Noah-MP), the Rapid Update Cycle (RUC), and the Community Land Model version 4 (CLM4). All the LSMs were coupled with the Weather Research and Forecasting (WRF) atmospheric model, then the inter-comparison was performed during the period from 1 June 2009 to 31 August 2010, which includes a model spin-up period of 6 month. From model validation results, the authors argue that the performance of Noah-MP was the best among these four LSMs in terms of reproducing measured snow depth, fractional snow cover, and albedo during the study period. Then, they highlight that simulation of the Eurasian snow cover is strongly affected by the choice of LSMs coupled with regional/global atmospheric models.

After reading this manuscript, I had several concerns about their study as follows. Please note that page and line numbers are denoted by "P" and "L", respectively.

- The main conclusion of this study "prediction of the Eurasian snow cover is sensitive to the choice of LSMs coupled to the global/regional climate models, and hence the future climate projections" (P. 1, L. 14 $\sim$ 15) is a matter of course. In the book by Armstrong and Brun (2008), it is presented clearly that simulation results by a LSM can change dramatically depending on the choice of model setting. In case several LSMs are compared, the difference can become much larger as reported by Etchevers (2004) and Krinner et al. (2018). Therefore, this reviewer thinks that there is nothing new in the present study.

- It is true that a model inter-comparison study is sometimes very useful and informative; however, this reviewer thinks that its purposes, protocol, and analysis strategy should be examined carefully in advance as performed by Krinner et al. (2018). It is well known that each LSM have their own purposes. In general, requirement level from a parent atmospheric model to a LSM is higher if a coupled system that consists of the atmospheric model and the LSM is used for long-term climate simulations, whereas, the requirement level would be relatively low if the coupled system is used for shortterm weather forecasts. It is because temporal evolution of snow physical conditions are much slower than that of atmospheric physical conditions. These imply that even a very simple LSM is sometimes very useful for a weather forecast model as long as the LSM can give reasonable bottom boundary conditions of the atmosphere. In addition, there is a possibility that some LSMs were tuned to give best performance in a specific area. I do not know in detail about WRF as well as the LSMs used here; however, I suspect some (physically simple) LSMs used here (for example, Noah LSM) should be used only for weather forecasts, although relatively detailed LSMs like CLM4 is suitable to be used for both purposes; the present inter-comparison procedure is a bit unfair for some simple LSMs (regarding the problem of the inter-comparison procedure, I mention below). Therefore, this reviewer was not convinced fully that performing this kind of inter-comparison is necessary and important.

- The inter-comparison procedure used here is too simple to rely on: The authors present mean difference, relative bias, root mean square error, and spatial correlation coefficient (Table 3); however, they are spatially and temporally averaged (in the model domain shown in Fig. 1 as well as the study period) (P. 10, L. 28 $\sim$ 29). In addition, their "observation data", which are the Canadian Meteorological Centre (CMC) Daily Snow Depth Analysis Data and the Moderate Resolution Imaging Spectroradiometer (MODIS) albedo, are not direct measurements; they can have error, which are generally larger than the error involved in direct measurements. This reviewer strongly recommends performing thorough model evaluation at the sites, where detailed insitu meteorological and snow measurements data are available. Figures 6, 7, 10, 11, 14, and 15 suggest that model performance and characteristics change from place to place dramatically. The authors suggest considerable effects of canopy on reproducing realistic snow depths (P. 10, L. 5 $\sim$ 8). Maybe, assembling and summarizing point validation results depending on land surface types is valuable and informative for readers. Furthermore, detailed model validations in terms of e.g., precipitation, downward radiations, surface atmospheric temperature, humidity, wind speed, pressure, snow depth, albedo, land surface temperature, etc are certainly needed to understand "why differ-

ent LSM produces different results in snow-related parameters" (P. 2, L. 30 $\sim$ 31). Due to the lack of detailed model validation results, I am not sure whether their suggestions regarding the further model improvement (P. 15, L. 15 $\sim$ 21) are reliable or not. Substantial efforts are needed to increase the reliability of their arguments.

- Finally, I have to suggest that using so many similar figures (e.g., Figs. 2, 7, 8, 10, 11, 12, 14, and 15) give the impression that the manuscript is a bit verbose.

Overall, this reviewer thinks that the current version of the manuscript cannot be published in the journal The Cryosphere.

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Specific comments (major)

P. 1, L. 12$\sim$14: As far as I learned from this manuscript, CLM4 is the most detailed and sophisticated model among the four LSMs in terms of incorporated snow physics. For example, CLM4 is the only model that can calculate snow albedo considering the effects of snow grain growth and light-absorbing impurities explicitly. However, the authors report that Noah-MP's performance was the best during the study period. I think there is a possibility that atmospheric conditions simulated by WRF was not realistic, which induced some compensations in the coupled system. Or, there is another possibility that given mass concentrations of light-absorbing impurities in CLM4 are not realistic as mentioned below. Therefore, I strongly recommend again to conduct detailed model validations in terms of e.g., precipitation, downward radiations, surface atmospheric temperature, humidity, wind speed, pressure, snow depth, albedo, land surface temperature, etc as mentioned above.

P. 4, L. 1: The authors state that Ms in equations (1) and (2) is snowmelt rate. Does it mean that meltwater produced in the model as well as liquid precipitation (rainfall) runoff instantaneously? No refreezing process in the model? If so, describe it, then state that Ms cannot be negative.

P. 6, L. 12 ∼ 13: How did the authors give mass concentrations of black carbon, dust, and organic carbon for CLM4? Giving realistic values for these light-absorbing impurities is crucial for accurate and reliable model inter-comparison.

P. 9, L. 14 ∼ 30: Before discussing features of calculated snow depths (even qualitatively), validations of simulated snow depths are needed. Many readers would not understand why the authors argue calculated snow depth patterns are "reasonable" (L. 15).

Figure 6: According to Table 3, CLM4 tended to underestimate snow depth; however, Fig. 6d indicates that CLM4 overestimates snow depth like other LSMs. Please discuss.

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Specific comments (minor)

P. 1, L. 11 ∼ 12: It is unnecessary to mention "Bidirectional Reflectance Distribution Function (BRDF)" here. BRDF data are not used to validate LSMs.

P. 1, L. 21 ∼ 22: I think the sentence "in winter the land surface temperature is higher than the air temperature, whereas in spring the former becomes lower than the latter since snow reflects solar radiation with its high albedo (e.g., Park et al., 2017)." is unnecessary, because this situation is not always true in all over the cryosphere.

P. 1, L. 22 ∼ P. 2, L. 1: References for the statement "The temperature gradient between the land surface and the ocean is one of the important factors that influences the atmospheric circulations." are needed.

P. 2, L. 5: Describe quantitatively.

P. 2, L. 8 ∼ 10: The authors can merge these two sentences "Furthermore, the Eurasian snow is highly related with the climate and weather systems in Asia. Many studies have been conducted on the correlation between the South Asian cli-

mate/weather systems and the Eurasian snow.".

P. 2, L. 11: Explain the interaction mechanism mentioned here briefly.

P. 2, L. 14 ∼ 16: What is the key findings of these studies on the "interactions"?

P. 2, L. 30: The 1st goal sounds strange. The developers as well as most users of these LSMs might already understand them.

Section 2.1: Consider to add a table describing key differences in snow physical processes incorporated in the LSMs

P. 4, L. 1 ∼ 2: Indicate directions of energy fluxes mentioned here.

P. 4, L. 11: "tuning parameter" of what?

P. 4, L. 30: What do the authors mean by "destructive metamorphism"?

P. 4, L. 33: "Melting factor" might be a tuning parameter. What effects can we expect from the modulation of this parameter?

P. 5: L. 6 ∼ 8: The explanation on "snow age" is difficult to understand. Why do the authors relate snow age to grain growth effect and soot effect?

P. 6, L. 3: Change 2.5 K to 275.65 K

P. 6, L. 15: How often did the authors perform initialization of the atmosphere and land? Only at the beginning of the calculation period (1 June 2009) (climate simulation mode) or every day (weather forecast mode)?

P. 6, L. 25: It is not necessary to mention vorticity and ozone here.

P. 7, L. 3: What is "snow-climate classes"? Please explain more.

P. 7, L. 11: What does the "differences" tell us? Please describe.

P. 7, L. 30: The intention of "microphysical options" is not clear. Please detail more.

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Technical corrections:

P. 1, L. 24: “temperature gradient” -> “horizontal temperature gradient”

P. 2, L. 19: “soil and vegetations” -> “soil, snow, and vegetation”

P. 6, L. 13: “ice effective grain size” -> “effective snow grain size”

P. 6, L. 15: “boundary conditions” -> “boundary conditions of the atmosphere”

P. 7, L. 10: “very high reflections” -> “relatively high reflections”

P. 7, L. 10: “very low reflections” -> “relatively low reflections”

P. 8, L. 1: Change “fractional snow cover with —” to “fractional snow cover Fs with —”, then, remove “Fs is fractional snow cover —” in L. 3.

P. 8, L. 4 $\sim$ 6: Move “Among the LSMs coupled to WRF, we excluded the Thermal Diffusion (TD) scheme (Dudhia, 1996) and the Pleim-Xiu LSM (Pleim and Xiu, 1995), since they do not consider the snow depth in their snow scheme.” to Sect. 2.1.

P. 9, L. 1: “value” -> “value of snow depth”

Table 1: Cite Oleson et al. (2010) for CLM4.

Table 3: Indicate units.

Figures 1a and 2: Consider to arrange easy-to-understand numbers in the color bars.

Figures 3, 4, 5, 9, and 13: The x-axis should not be noted in the number of days after 1 December 2009. Using date is much better to understand.

Figures 4b and 4c: The unit of y-axis is strange?

Figure 4 caption: “The amount of (a) —” -> “The area-averaged amounts of (a) —”

Figure 5 caption: Why not 273.15 K rather than 273.16 K?

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References:

Armstrong, R. L. and Brun, E. (Eds.): Snow and Climate: Physical Processes, Surface Energy Exchange and Modeling, Cambridge Univ. Press, Cambridge, UK, 2008.

Etchevers, P., et al.: Validation of the energy budget of an alpine snowpack simulated by several snow models (SnowMIP project), Ann. Glaciol., 38, 150–158, doi:10.3189/172756404781814825, 2004.

Krinner et al.: ESM-SnowMIP: assessing snow models and quantifying snow-related climate feedbacks, Geosci. Model Dev., 11, 5027-5049, doi:10.5194/gmd-11-5027-2018, 2018.

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