The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-285-RC1, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on tc-2021-285

Anonymous Referee #1

Referee comment on "Snow cover prediction in the Italian central Apennines using weather forecast and land surface numerical models" by Edoardo Raparelli et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-285-RC1, 2022

Review of “Snow cover prediction in the Italian Central Apennines using weather forecast and snowpack numerical models”, by Raparelli et al.

Summary

This paper presents snow cover simulations at 3 km resolution over the Apennines mountain range in Italy, during one winter season, using two very different land surface models: the Noah model including a simple one-layer snowpack scheme resolving the mass and energy budget only, and the Alpine3D model including the complex SNOWPACK model resolving the microstructure of the snowpack. Noah is coupled to the WRF meteorological forecast model, which is also used to drive Alpine3D simulations. The authors assess these simulations in terms of near-surface meteorological variables, snow height and daily snow height variations (against in situ measurements) and in terms of snow cover extent (against MODIS-derived snow cover fraction products). They show lower snow height biases for WRF-Alpine3D but a slightly better representation of snow cover area when compared to satellite products.
Many papers in the past years have analyzed kilometer resolution snow cover simulations in mountains, driven by weather forecasts or analyses. This paper is interesting as it focuses on a lower latitude mountain range, in a different climate and lower elevations. It also compares to different models. However, in its present form, it doesn’t bring much novelty compared to past studies and could push much further the comparison between simple and complex snow cover models. Therefore, I would recommend major revisions before publication.

Major comments

- This paper follows the methodology of several previous papers, and in particular Quéno et al. (2016). To bring more novelty compared to previous papers, it should focus more on the particularity of this study and develop a deeper analysis of its originalities (mostly snow cover simulations in milder climate, and comparison of different complexity models).
- The assessment of the model simulations over only one winter (3 months actually) can be problematic. There is a strong interannual variability between winter seasons, especially in terms of snow cover. Adding some more winter seasons to the comparison would increase the significance of the evaluation (e.g., Essery et al., 2013).
- A more in-depth analysis of the processes driving the snowpack evolution is necessary to highlight differences between simple and complex snowpack models. Particularly, many hypotheses about the better representation of processes by WRF-Alpine3D compared to WRF-Noah are not proved. Pushing further that analysis would add significance to the paper. It would also provide elements for a more advanced discussion of simple snow height or snow cover extent statistics.

Specific comments

- Abstract: the results should be more summarized, with less numbers.
- l. 36: Sommer et al. (2018) focuses on the impact of wind on snow packing. There are many other papers more appropriate to refer to the impact of wind on snow cover variability.
- l. 40-52: The references of snowpack simulations driven by NWP data at kilometric resolution are the same as in Quéno et al. (2016). More recent publications should be included to update the literature review.
- l. 100: please define Cfs. I can't find what it means in the Köppen system, typo?
- l. 117-122: the presented snow climatological data is very old (1921-1960). Isn't there more recent climatological data over the region? i.e., more recent statistics, instead of simply mentioning a decrease in the past decades (l.122-123)?
- Figures 1 and 2 are probably not necessary, given the large number of figures. A short overview of winter 2018-2019 without figures may be enough.
- l. 188-190: "With exception of the first simulation (…) in the study area." This sentence is very unclear to me. Please clarify.
- l. 199: WRF-Noah model runs from 1 December 2018: is it initialized with no snow cover at that date? If so, is it realistic? It is mentioned later for Alpine3D but should be clarified here too.
- l. 220-221: please clarify for what the neutral atmospheric stability conditions are chosen: turbulent fluxes?
- l. 227-229: Are WRF output variables also corrected for elevation difference between model grid cell and station, when compared to measurements?
- l. 231-235: MBE, MAE and R are rather common metrics, which don't need explicit definition here.
- l. 236-237: As for meteorological variables, what about the elevation difference between model grid cell and station?
- l.239-245 and l. 253-265: The definition of ETS, HI_{rdm}, Table 3, as well as the definition

of Jaccard index, MDHD and ASSD are the same as in Quéno et al. (2016). Given that the validation method is the same as that paper, I would make the definition of validation metrics much more concise and refer to Quéno et al. (2016).
- l. 250: please specify the value of the threshold SWE W_{max}.
- l. 252: how is the threshold of 51% chosen?
- Figure 5: plots are not very informative because of the large number of dots. Consider perhaps smaller dots, or density plots?
- Section 4.1: all evaluation metrics about meteorological variables are strongly impacted by elevation difference between simulations and observations. If no elevation correction is made, some statistics on elevation differences should be provided.
- l. 323-325: regression lines don't give much information. It would be more informative to describe tendencies for each model (e.g. underestimation of highest snow depth by this or that model, etc.).
- Figure 7: consider using smaller dots to make it easier to interpret.
- l. 358-361: "it is likely due to the more complex multi-layer Lagrangian scheme of the Alpine3D model (…)". There is no result in this paper to support this hypothesis. The effect of the different complexity of models on the snowpack evolution would be very interesting to compare, but there is no proof here to: 1) differentiate between snow compaction and melting; 2) claim that the more complex model reproduces better either compaction or melting.
- L. 369-374: Why not masking out forested areas when comparing MODIS to models?
- Figure 10 could be included in Figure 9 to compare visually more easily and see more directly the impact of forested areas.
- l. 392 and Table 6: probably a factor 100 error (rather 6% than 0.06%)
- Section 4.2.2: as far as I understand, the snow cover fraction is computed a posteriori using equation (6). However, the fractional snow cover can have an impact on the snow cover dynamics, especially at 3 km resolution. Why not using a fractional snow cover scheme in the models, e.g. Helbig et al. (2021) or simpler schemes?
- l. 416-417: use North-South instead of upper part-lower part which could suggest elevations.
- l. 431-432: accumulation should be used for snow water equivalent increase. Please reformulate the sentence, and in particular "snow height accumulation" and "snow water equivalent accumulation".
- l. 428-443: The interpretation of Figure 15 is a bit confusing. Apart from a little more SWE in Noah than Alpine3D in early December, the SWE is always higher for Alpine3D than Noah. Furthermore, a more in-depth analysis separating snow accumulation and melting amount would be necessary to draw more solid conclusions which would allow to better clarify the differences between models.
- Conclusion: values from the results should not be repeated there but summarized.
- Conclusions linking the bulk performance metrics to the different representation of processes in the models are not proved, only hypotheses at this stage.

Local remarks and typos

- l.9: define LSM
- l. 31: typo snowpack
- l. 35: precipitation, singular
- l. 60: snow layers

- l. 104: altitudes -> elevations
- l. 137, l. 142: leaded -> led
- l. 148: performance
- l. 154: delete area or domain.
- l. 154: near-surface
- l. 167-170: to be deleted, redundant.
- l. 194: a model
- l. 281: altitude -> elevation
- l. 313: the three time series
- l. 320: most of the 13 sites
- l. 436: It is
- l. 450: comparing them to
- Language proofreading by a native speaker could be useful.

References

Essery, R., Morin, S., Lejeune, Y., and Menard, C. B.: A comparison of 1701 snow models using observations from an alpine site, Adv. Water Resour., 55, 131–148, doi:10.1016/j.advwatres.2012.07.013, 2013.

Helbig, N., Schirmer, M., Magnusson, J., Mäder, F., van Herwijnen, A., Quéno, L., Bühler, Y., Deems, J. S., and Gascoin, S.: A seasonal algorithm of the snow-covered area fraction for mountainous terrain, The Cryosphere, 15, 4607–4624, https://doi.org/10.5194/tc-15-4607-2021, 2021.

Quéno, L., Vionnet, V., Dombrowski-Etchevers, I., Lafaysse, M., Dumont, M., and Karbou, F.: Snowpack modelling in the Pyrenees driven by kilometric-resolution meteorological forecasts, The Cryosphere, 10, 1571–1589, https://doi.org/10.5194/tc-10-1571-2016, 2016.