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Reply on RC1

Edoardo Raparelli et al.

Author 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-AC1, 2022

REVIEW 1

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.

We thank the reviewer for the time spent evaluating our work and we thank him/her for the useful and constructive comments. We have answered all comments.

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).

As the reviewer suggests, these themes are a peculiarity of this paper. Thus, they will be further investigated and developed in the next version of the manuscript.

- 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).

We will extend model simulations to winters 2019-2020 and 2020-2021, thus covering three entire winter seasons. Winter 2019-2020 was dry and mild, while winter 2020-2021 was much snowier than the previous two, thus the models are evaluated over winters with large interannual variability.

- 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.

To better understand the differences between the models in reproducing the snowpack evolution, the authors also considered snow density data, obtained from other sources. Knowing snow density, we were able to compare measured and simulated snow water equivalent data, and thus to assess the contribution of compaction and melting to snow settlement and the ability of the models to reproduce the observations.

Specific comments

- Abstract: the results should be more summarized, with less numbers.

This will be modified in the revised manuscript.

- 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.

We will look more carefully at the past literature in order to cite papers more focused on 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.

We will look more carefully at recent literature in order to update the literature review.

- l. 100: please define Cfs. I can't find what it means in the Köppen system, typo?

Yes, it is a typo, since the inner, mountain part of central Italy is Cfb. Also, we would add the "inner" attribute to "central italy" in the text, for a better geographical reference. After the check, we also decided to add a further reference, since the Köppen classification was adapted by Pinna (1970) for Italy.

Pinna, Mario, Contributo alla classificazione del clima d'Italia, «Rivista geografica italiana» LXXVII, II, 6/1970, p. 129-152.

- 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)?

A more detailed discussion about these aspects and more recent references will be added to this section of the manuscript.

We are aware that the snow climatological dataset is dated by now. However, reports and maps provided by the Ministry of Interior in the early 70s represent the latest regional-scale information about the snow depth for the whole Italy. After that period, the fragmentation of the National Hydrographic Service into twenty different regional offices (one per Italian region) led to the loss of homogeneity in data collection and statistics. This issue is well known by the Italian hydro-meteorological community, as stressed by Libertino et al. (2018); Rossi (2020) and Alberton (2021). While several studies are available in the Alps and northern Italy, central and southern Italy suffer from a poorly densed snow gauges network. The data scarcity did not encourage research in this area.

Nevertheless, since central Apennines are particularly prone to avalanche hazards, which has caused frequent casualties in the last few years, some authors have focused their attention on the occurrence of extreme snowfall events. The most cited study in this field is Piacentini et al. (2020), where records of recent extreme snowfalls are provided. The occurrence of extreme snowfall and high snow occurrence/accumulation variability of this part of Italy, is also highlighted in a recent study from Fazzini et al. (2021), published after our manuscript submission.

The decreasing trend in winter precipitation in Central Italy in recent decades is mentioned by several studies, such as Brunetti et al. (2000), Pavan et al. (2008), Longobardi and Villani (2010). Romano and Preziosi (2013), Appiotti et al. (2014) and Scorzini and Lopardi (2019). The last paper also presents a comprehensive summary of available studies in Italy.

The last available statistics, specifically carried out on snow gauges data, is provided for the Gran Sasso d'Italia massif, the highest peak of Apennines, and is provided by the Interregional Association for coordination and documentation of snow and avalanche problems (AINEVA), where a decreasing number snow days are recorded on altitudes below 1300 m a.s.l, in the 30-year period 1978-2007, even if events with intense snowfalls are increasing in some specific areas (Romeo and Fazzini, 2008).

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Scorzini A.R. and Lopardi M., Precipitation and temperature trends over central Italy (Abruzzo region): 1951-2012, Theoretical and Applied Climatology, 135,959-977, 2019, https://doi.org/10.1007/s00704-018-2427-3

- 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.

The figures will be moved in the supplementary material.

- l. 188-190: "With exception of the first simulation (…) in the study area." This sentence is very unclear to me. Please clarify.

This sentence is related to lines 199-201. Our simulations are constituted by a series of 60 hours model runs. In each simulation, the atmosphere is initialized with NCEP data, while the soil and snowpack are restarted with the previous run, in order to avoid discontinuities in snowpack simulation. Initial conditions for snowpack and soil are taken from NCEP data only for the first run of this series.

The text will be rearranged to make this point more clear.

- 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.

We have used the snow cover provided from NCEP data. However, on 1 December 2018, a thin and patchy snowpack was present only at the highest elevations of our domain that are not covered by the measurement network used in this work.

This point will be clarified in the revised manuscript.

- l. 220-221: please clarify for what the neutral atmospheric stability conditions are chosen: turbulent fluxes?

The atmospheric stability affects the computation of the heat fluxes. We imposed neutral conditions because it provided the best results in terms of simulated snow height compared to other atmospheric stability conditions used in several sensitivity tests.

- l. 227-229: Are WRF output variables also corrected for elevation difference between model grid cell and station, when compared to measurements?

The meteorological variables are not corrected by elevation difference when compared to measurements. The reason is due to the fact that we want to show the difference between

Noah and Alpine3D, when both models are forced with the same meteorological variables. Meteorological variables can't be corrected for Noah, since it is online coupled with Alpine3D, and thus they are not corrected for Alpine3D too. This means that it is more interesting to compare measured meteorological variables and uncorrected meteorological variables since the latter is used to drive the snow models. However, some statistics on the elevation difference between a model grid cell and station elevation will be added to supplementary material, since they are fundamental to assess how real topography is represented in the model.

- l. 231-235: MBE, MAE and R are rather common metrics, which don't need explicit definition here.

As the reviewer suggests, the error metrics won't be explicitly defined in the revised paper.

- l. 236-237: As for meteorological variables, what about the elevation difference between model grid cell and station?

As already said for the meteorological variables, some statistics on the elevation difference between model grid cell and snow station elevation will be added to the revised manuscript.

- l.239-245 and l. 253-265: The definition of ETS, HIrdm, 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).

As the reviewer suggests, the aforementioned metrics won't be explicitly defined, but referred to Quéno et al. (2016)

- l. 250: please specify the value of the threshold SWE *Wmax*.

Wmax varies locally according to MODIS land use classification, so that it is not a single value. A table with the correspondence between land use class and *Wmax* will be added to supplementary material.

- l. 252: how is the threshold of 51% chosen?

Since we had to build binary maps from snow cover fraction maps we decided to use a common threshold for MODIS and snow cover models, thus we arbitrarily decided to use the threshold of 51% to discriminate between snow and no-snow.

- Figure 5: plots are not very informative because of the large number of dots. Consider perhaps smaller dots, or density plots?

Since the number of dots in each plot is really large, decreasing the dot size does not significantly improve the plot readability. Thus we will use density plots, as the reviewer suggests.

- 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.

As already said in previous answers, statistics on elevation differences between a model grid cell and station elevation will be added to the supplementary material.

- 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.).

As the reviewer suggests, we will further compare observed and simulated snow height also in terms of maximum overestimation and underestimation. We will extend the same approach to the snow height variation analysis.

- Figure 7: consider using smaller dots to make it easier to interpret.

As for Fig. 5, we will use density plots to make the figure more informative.

- 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.

The introduced snow density measurements gave the opportunity to calculate snow water equivalent. This allowed us to differentiate the contribution of melting and compaction to the observed snow settlement and compare it to model simulations.

- L. 369-374: Why not masking out forested areas when comparing MODIS to models?

Jaccard Index and ASSD can be computed only from binary matrices, so filled entirely by 0/1 or False/True values. Masking out forested areas, thus assigning to those cells an arbitrary value (-9999 on NaN for example) will make it impossible to compute the two indices. However the masking of the forested areas can be introduced for the comparison of the snow cover area fraction (Tab. 6 and Fig. 12), but then some metrics would be computed neglecting forested areas (MAE, MBE and R) while some other not (J and ASSD). In order to maintain coherence within the analysis, the authors decided to not mask out forested areas.

- Figure 10 could be included in Figure 9 to compare visually more easily and see more directly the impact of forested areas.

As the reviewer suggests, the forested areas of Fig. 10 will be superimposed on Fig. 9 in the revised manuscript.

- l. 392 and Table 6: probably a factor 100 error (rather 6% than 0.06%)

This is not an error. Snow cover area fraction ranges from 0 to 0.3%, thus MBE and MAE equal to 0.06% are reasonable values.

- 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?

Noah implements Eq. 6 thus it calculates at runtime the snow cover fraction. To the authors knowledge, Alpine3D does not implement schemes for snow cover fraction calculation, thus the authors applied Eq. 6 to Alpine3D a posteriori in order to derive snow cover fraction also for that model and compare it to MODIS. Helbig et al. (2021) scheme will be taken into account for future works on this topic.

- l. 416-417: use North-South instead of upper part-lower part which could suggest elevations.

We will apply the suggested correction in the revised manuscript.

- 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".

As the reviewer suggests, "snow height accumulation" and "snow water equivalent accumulation" are not correct in this sentence. We will reformulate the sentence in the revised manuscript, also highlighting the differences with the measured snow water equivalent.

- 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.

As already said in previous answers, the access to measured snow height and density data will permit us to compute measured snow water equivalent. Thus it will be possible to compare measured and modeled snow water equivalent and draw more solid conclusions on the differences between the models.

- Conclusion: values from the results should not be repeated there but summarized.

The suggested correction will be applied to the revised manuscript.

- Conclusions linking the bulk performance metrics to the different representation of processes in the models are not proved, only hypotheses at this stage.

We will put more effort to identify the main causes of differences between the models, also considering snow water equivalent.

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.

These typos and remarks have been all corrected.

References

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