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Discussions

Interactive comment on "Sensitivity of snow models to the accuracy of meteorological forcings in mountain environment" *by* Silvia Terzago et al.

Anonymous Referee #1

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General comments:

The authors of the manuscript (ms.) have tested six non-calibrated snow models at one mountain location by varying the time-resolution and origin of model forcing. The quality of meteorological forcing is indeed an important element in snow modelling, and the authors have here examined the sensitivity of snow model performance to varying input data quality. The manuscript is quite well-written, and the illustrations are mostly clear and understandable. The research idea is based on straightforward model testing, i.e. running an ensemble of models with different forcing data and calculating statistics on how the models' performance (evaluated against point snow observations) vary. However, the two main weaknesses of the ms. are in my opinion that 1) the model evaluation is made at only one single site (Torgnon), and that 2) there is no discussion

or treatment of uncertainty in solid precipitation measurements, which directly affect snow amounts in the comparison. Although the ms. may provide an interesting case-study for those interested in the specific models and the local site, the ms. does not have, in my opinion, large enough impact and interest for a wider audience to warrant publication in HESS. After reading the ms. twice, I felt in the end that I did not get much relevant information out of it for my modelling work, in another place, another country. As the authors themselves state (p. 22, lines 8-9): "This study offers some hints on this research topic".

Specific comments:

* As snow is often spatially very inhomogeneously distributed, normally the utility in snow modelling is to get a grasp of this spatial variability. Thus, simulating snow in just one point has limited relevance, mostly restricted to snow process studies. In another point, the authors' results and model ranks might be changed significantly. As the authors themselves state, on p. 2, line 15: "Snow models are generally evaluated at a number of sites"; on p. 26, lines 2-3: "Further analysis at other test sites would be useful to explore the extent to which our results could be generalized to different situations or models".

* The authors note on p.4 line 1-12: "the uncertainty on snow simulations due to the forcing can be comparable to or even larger than the uncertainty". They also refer on p.7 line 2 to Kochendorfer et al. (2017), who assess and provide algorithms to deal with the undercatch of solid precipitation. However, no effort is made to discuss, assess or correct the precipitation measurements at Torgnon station for the undercatch and/or examine the sensitivity of the authors' results for the inherent uncertainties in the observation-based model precipitation input (their CTL experiment with "optimal forcing").

* The authors claim that a bias adjustment of forcing data leads to more precise results (p.9. lines 29-31). This seems to me like a rather trivial point.

* The linear interpolation of shortwave radiation e.g. in the TIME-12h case, causing the large deviations of +97 W/m2 (p. 18, lines 3-4), is an unrealistically simplistic way to make the interpolation. In real modelling practice, I suppose most of us would use a sinus-curve form or something like that. Consequently, the issue here is more of poor modeling practice than lower time-resolution. This is only mentioned in section Discussions (p.24, line 10), but would have been best to put into practice already in the authors' study.

* The point-specific biases and errors of the MeteoIO, GLDAS, ERA5 and ERA Interim, described in Sections 5.4 and 5.5 for the single Torgnon site, emphasize the weakness of this case study: the model evaluation results are difficult to generalize outside this site, where things and biases could be very different. Also, compensating errors may occur in the models which improve model performance. In other words, one gets "right results for wrong reasons" (p.13, lines 14-16; p. 21, lines 4-5; also p.24, line 17).

* Normally, the authors' results would be compared to previous studies in the section "Discussion". However, this three-page section only has one (!) single reference to other published studies. It also repeats many things already pointed out previously in the ms.

Other points:

* p.7 lines 22-23: provide values for the vertical gradients used here.

* Fig. 2. The meaning of dashed line circles in the Taylor graph should be explained.

* The Appendix is short (a little figure and table, and five lines of text) and could be easily added to main text in Section 3.2.

* The ms. contains a lot of numbers in tables 3-4, and displaying them more reader-friendly, like in the nice Fig. 6, would be good.

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