Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2022-129-RC1, 2022

Comment on gmd-2022-129

Bertrand Cluzet (Referee)

Referee comment on "Operational water forecast ability of the iSnobal-HRRR coupling; an evaluation to adapt into production environments" by Joachim Meyer et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-129-RC1, 2022

Operational water forecast ability of the iSnobal-HRRR coupling; an evaluation to adapt into production environments

Joachim Meyer et al.

Review by Bertrand Cluzet

GENERAL:

In smowmelt-dominated water catchments, operational snowpack modelling is essential for water resource management and flood risk assessment. Most of the operational modelling services use highly calibrated temperature-index snow models, which may fail to capture changing snowpack dynamics amid climate change. In this paper, the authors make use of a physically-based snowpack modelling framework (AWSM, Havens et al., (2020)), whose core is the iSnobal model (Marks et al., 1999), and which includes a dedicated downscaling scheme, to perform 4 years of simulation at high resolution (50m) over a high alpine watershed (1373km2), tributary of the Colorado river. Compared to the Havens et al., (2020), the main innovations reside in the use of HRRR, a high resolution (3km2) weather forecast model to force iSnobal, and the use of a dedicated downscaling scheme for wind fields.

Model performance is evaluated against in-situ measurements of Snow Depth, snow depth high resolution aerial maps from the Airborne Snow Observatory (ASO) across the entire basin, and inflow measurements at the basin outlet. The output from the operational temperature-index model SNOW-17 is used as a benchmark.

Results show that iSnobal captures well the accumulation of snow, but seems to have a too low ablation rate, potentially due to a bad representation of net shortwave flux.

The authors conclude that this snowpack modelling chain seems suitable for further operational use, provided some improvements to the snow model are made.

Overall, I think that this paper makes a nice contribution to the snowpack modelling community, by offering a promising model workflow to perform high-resolution, distributed, physically-based simulations using downscaled numerical weather prediction models. Although this topic has already been assessed by other teams, an excellent introduction makes a very good statement for it, and the paper was, overall, very pleasant to read. I also appreciate the effort for reproducible, open-science by the authors, which is in line with GMD standards. I find, however, that the manuscript is not suitable for a journal like GMD in the present form, for the following main reasons. (1) The degree of scientific/modelling innovation is too limited for the scope of GMD (Havens et al., (2020) would probably have been a good fit). (2) I also am a bit disappointed by the fact that despite that the authors downscale the model to a very high resolution (50m), none of the processes that would justify to use such a high resolution (namely snow-vegetation interaction, wind drift, and potentially gravitational redistribution of snow) seem to be accounted for in the model. (3) The evaluation itself is not consistent with these modelling hypotheses, as comparison with ASO data should discard forested pixels, which seem to cover an important fraction of the domain.

I am very positive, though, that this work is suitable for publication, probably in another journal considering the major adjustments I suggest in the following. The authors will also find suggestions of adjustments further down, as well as inside the comments on the manuscript pdf.

1. Consider reducing the model resolution to a scale that is consistent with the modelling hypotheses for further publications (or include relevant processes at 50m resolution)

2. Put more stress on the use of spatial data from ASO for evaluation in the body of the paper (see below).

3. Increase the level of rigor and the amounts of details in the model description section (see below).

MINOR COMMENTS:

(General)

- please avoid using the term of “coupling” between HRRR and iSnobal, as the model relation between HRRR and iSnobal is only one-way (no feedback from iSnobal to HRRR, HRRR is “forcing” iSnobal). This is essential as atmosphere-Snow coupling is a big challenge that several teams are currently addressing (e.g. Sharma et al,. (in review) and references therein), and while it is perfectly fine to just use HRRR as a forcing, we should avoid ambiguities.

Consistently, I would strongly suggest the authors to change all instances of “iSnobal-HRRR” to “HRRR-iSnobal” which would be more faithful to the causality between the meteorological forcing and the snow model.

- Overall, I think that the paper would strongly benefit from including more figures in the body of the paper (in particular S3-S5, and S6) rather than in supplements. S3-S5: ASO data are a wealth of information, and the paper is too shy about it!

(Abstract)

consider using the keyword of AWRS somewhere in the abstract, as this seems to be the key software that makes these simulations possible, and this study is a nice advertising ‘scientific application’ for it.

(3.1)

- Please revisit the model description section as it is a bit shaky and raises more questions than it actually clarifies the framework. It should clearly appear on the body of the paper whether the model accounts for wind redistribution of snow and models the canopy. This information may be available in the references (after 30mins of browsing through Marks et al, 92, 99 and Havens et al., 2017,2020, my understanding is that it is not), but such essential information should explicitly appear in the paper.

- similarly, the description of the study area (Sec. 2) l 114-116 is too vague, in particular in terms of vegetation cover type: it seems from Hubbard et al., (2018), and Fig. 3., that the forest covers an important part of the watershed and may impact the snowpack dynamics (canopy interception, increased longwave radiation...). If that’s the case, and based on my understanding that the model does not account for the presence of a canopy, then I would strongly recommend to:

* include a rough landcover type to maps on Fig. 1

* add the two unrepresented sites on Fig. 3

* discard forested areas when comparing to any observation, in order not to compare apples and oranges.

(3.4)

- consider moving this section up, (3.2) as it directly answers several questions raised by (3.1).

- l. 173-174: see comment on the output resolution of Wind Ninja.

- l.174-177: please revisit the description of the handling of shortwave radiation in a more rigorous/precise manner.

(3.5)

- l.179-182: Whether your model includes wind drift/ preferential deposition of snow is really essential here as according to Winstral et al., (2014), 100-250m would have been enough if these processes are disregarded.

(5.2)

- l. 240ff: this paragraph is very difficult to follow.

- l. 233 239: still understanding that wind drift and vegetation are not accounted for, I could not wrap my head around the considerable variability in the modeled snow melt rates of the 4 neighboring pixels , at only two of the three field sites (Fig. 4 and S1).

(Figures)

- I found the interpretation of figures (S3 and Fig. 5) a bit difficult due to the unusual computation of model bias/difference (even though I appreciate the effort of the authors to indicate model under/overestimation, I think that it would ease the readability just to follow conventions). Usually, people take the observation as a reference, and compute "bias = model – obs", resulting in positive bias for model overestimation and negative bias for underestimation. I strongly recommend using this convention throughout.

- In S3, blue (resp. red) is usually the color used for model overestimation (resp. underestimation of snow amounts, since it looks wet (resp. dry).

References:

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Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2022-129/gmd-2022-129-RC1-supplement.pdf