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The Cryosphere Discussions

Interactive comment on "Multi-physics ensemble snow modelling in the western Himalaya" *by* David M. W. Pritchard et al.

Anonymous Referee #2

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In this manuscript, Pritchard et al. present a new application of the Factorial Snow Model (FSM) as applied in a 2D configuration to investigate the snowmodel configuration on Himalayan snowpack simulation. First, I would like to say it was a delight to read a well written and generally clear manuscript. The figures are of a superb quality, and overall the manuscript is well done. The scientific content appears to be of generally high quality, and I believe it will be of interest to The Cryosphere readership. I would recommend this for publications, however there are a few points I have concerns with, detailed below.

My most pressing concern is that I am not convinced it is appropriate to compare the aggregate snowpack runoff with measured discharge when there is no hydrological routing in the model, nor any other hydrological processes, e.g., groundwater, soil stor-

age, frozen soil infiltration, and ET. It is not clearly demonstrated that these processes can be ignored, especially for late seasons flows and over such a large area. I really like the story that the inclusion of improved process representation (e.g., inclusion of liquid water flows in snowpacks) improves the hydrology, however I believe this study is putting the cart before the horse, and that such claims are not supported. I would strongly suggest that these comparisons be removed.

Second, I'm concerned that two, generally critical cold-regions processes, are ignored: blowing snow sublimation and horizontal mass redistribution. These combine to a) remove mass via sublimation – O(5-30%) of total precipitation depending on area; b) clear snow from steep slopes; c) cause avalanching that results in deep, persistent snowpacks at the base of the slopes. Personally, I've found that the Snowslide parameterization is insufficient without blowing snow processes to make a profound impact on snowcover heterogeneity, likely in line with the observations of the authors. I think this would be especially the case at the resolutions being used herein. I'm deeply sympathetic to the incredible challenge that operating a blowing snow and diagnostic avalanche model is over this type of basin, viz. uncertainty in wind and precipitation fields. I also realize the authors are also well above the snowdrift resolving scales of approx. 1 m to 150 m. However, I cannot help but worry that this confounds the SCA and albedo aggregation comparisons over the entire basin. Personally, I miss a more detailed treatment and explanation on these processes in the manuscript, and I believe it would be strengthened by highlighting these limitations to a greater degree.

I think the vertical analysis of sensitivity is a novel approach and deserves a stronger place in the paper. The sensitivity to the climate anomalies was also a nice contribution.

Specific comments:

P1. L.14-15 "These[. . .]adjustments. "This sentence is unclear

P1. L.18 "anomaly space" is not clear and I would reword for the general reader

P2. L.20 “This reflects”. What, specially, does ‘this’ refer to? I have this point throughout where ‘this’ is used at the start of a sentence, but it is not entirely clear exactly what ‘this’ refers to.

P2 L. 21 “application-relevant” should be described – what application?

P2. L.23 “recent data” what data are these? Remote sensing? In situ?

P2. L.27 “there to be only groups[. . .]variable” Awkward, consider revising

P2. L.28. “model complexity” Please define exactly what you mean by this

P3. L.6 “The aim [. . .]” awkward, revise.

P3. L.8 “application-orientated”, same as above, what does this encapsulate?

P3. L.14 3500-4500 mASL missing. Throughout I would prefer units to be attached to the first number. As well, it is generally assumed m is ASL. Is ASL really needed here?

P3. L.21-22 “this indicates [. . .] variability” is unclear to me. You’re saying that even at these cold, high elevations there is always sufficient energy to melt the entirety of the snowpacks?

P4. L.1 “This beings” what is this?

P4. L.14 “physically realistic” define what you mean by this (similar to complexity)

P4. L.27 Should add Marks, et al. 1999 for iSnobal

P5. L.19 A description on site locations would be beneficial. Are these valley sites? Cold air drainage susceptible?

P5. L.25 I found this section unclear with respect to, exactly, what Micromet algorithms were used. If I understood the text correctly you derived the lapse rates to use in the micromet algorithms? If this is correctly, just explicitly state this. I’m not familiar with the HAR dataset; does it provide prognostic variables at multiple pressure heights and these were what you used to derive the vertical gradients?

P6. L.7 “To account for [. . .]” This is unclear to me what is model and what is observed. I would be more explicit here

P6. L.15 “other variables” Please state what these are here, or give some examples.

P6. L.31 “This confirms [. . .]” what is this.

P6. L.31 “confirms” I would consider changing to “supports” or “agrees with” (comment applies throughout the text).

P7. L.2 Define satisfactory quality

P8. L.21 Blowing snow and avalanching, as per my comments earlier.

P8. L.25 “Again this [. . .]” what is this?

P8. L.25 I would lay out what compensatory effects are present here

P10. L.25, 26 16 and 26.8 need units

P10. L.30 What are mm/a ?

P11. L.2 I perhaps missed it but ensure LST is defined

P11. L.15+ This section is missing details described in the figure caption regarding opt0-opt1. The isotherm was not clear to me. I would ensure it is described in this text.

P12. L.4 RMDS, should this be RMSD? I would also ensure this is defined and the equation shown in the methodology

P12. L.10 Consider w/c for “confirms”

P13. L.16 “Anomaly space” would benefit from a definition in the methodology

P31. Figure 1 Is 0 m the correct lowest elevation for this site?

References

Marks, D., Domingo, J., Susong, D., Link, T., & Garen, D. (1999). A spatially distributed

energy balance snowmelt model for application in mountain basins. Hydrological Processes, 13(12-13), 1935-1959.

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