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

Matthieu Lafaysse (Referee)

matthieu.lafaysse@meteo.fr

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General comment

Pritchard et al presents a multiphysics spatialized snowpack modelling application in the challenging context of Himalaya with all the implied limitations in terms of data availability. This is a remarkable effort and this new application of FSM demonstrates the interest of multiphysics frameworks even in such complex contexts. To my mind, the level of importance of the different results presented in this paper is variable. For instance, the elevation-dependence of the snowpack model sensitivity (Section 4.2.4 and Figure 6) is a very innovative result compared to the existing literature and is of major importance. The analysis of the climate sensitivity of the different multiphysics options (Section 4.4 and Figure 9) is also a new and very promising method for snowpack model evaluation, and the different behaviours between options is especially interesting. Conversely some other sections present results which are either less robust (comparison of snowpack runoff with river discharge) either easily expected (best skill of a prognostic albedo to simulate albedo). The number of results presented in the paper is rather large and the paper may be more striking if more focused. Therefore, I definitely think that this paper deserves publication but I would suggest to remove some unnecessary results and a few other modifications as suggested below if it is possible (nothing being absolutely necessary).

Detailed comments

Page 1 Line 15 'atmospheric stability adjustment' I know what the authors are talking about but I think this is unclear in an abstract for a standard reader who can not be supposed to know in details how are computed the turbulent fluxes in such models.

Section 3.3 Snow models are known to exhibit a highly variable skill over time from one year to another. Therefore, it is critical to provide the evaluation period for the different evaluated variables to assess the robustness of the conclusions.

Page 6 Line 25 If I understand well, the modelled SCA of a pixel can only be 0 or 1, is it correct?

Page 6 Line 29 Can you provide the details of the normalization? Are there estimates of the magnitude of the remaining errors after normalization?

Page 8 Lines 6-14 I am not convinced that it really make sense to compare simulated snowpack runoff with observed river flows without any hydrological modelling to raise significant conclusions about the best-performing snow model configurations. The authors state that "such large differences in timing are unlikely to be accounted for by runoff routing or other hydrological processes at this time of year" but this is not demonstrated. Over mountainous basins of this surface in European Alps, it is common that evapotranspiration represents 30 to 60% of total discharge. It is also common that aquifers delay for 2 to 4 months a significant part of the runoff. How can we be sure that this is not the case in this study catchment? Several papers argue that these hydrological processes must not be ignored in snow-covered basins. Unfortunately, if some prevailing processes are missing to simulate the discharge, I am afraid that the sensitivity analysis should be limited to the 3 first paragraphs of Section 4.1.1 because the conclusions of the last one may rely on unrealistic assumptions. Obviously, I understand that it would be nice to show that including all physical processes improve the resulting discharge but my feeling is that the evaluation variable is not direct enough for such a conclusion.

Page 8 Lines 21-22 Please add "(not shown)" as this statement is not supported by the presented results.

Page 9 Lines 1-2 "The slowest-responding model configurations (. . .) are too slow in their SCA decline from June onwards." Is this statement sensitive to the assumption of a binary modelled SCA? Did the authors try to use depletion curves which are largely used in the literature?

Section 4.2.1 Indeed the prognostic parameterization of albedo seems more consistent with observed time variations. However, is this result really surprising? I understand that the FSM framework does not consider the different parameterizations as equally probable but that they represent the variety of snow schemes used in climate models. I think that it could be expected to find that the diagnostic parameterization (based on surface temperature which is actually quite surprising) has a poorer skill than the prognostic one when considering only albedo evolution and I am not sure that we are really learning something here.

Page 11 Lines 1-3 Indeed this is an interesting result. Actually, I am not so surprised. The classical formulation of turbulent fluxes is known to not be valid in stable conditions and especially in mountainous environments where turbulence is much more influenced by the local topography than by the snow roughness length. There is a variety of numerical artefacts to correct this behaviour in several models but even the utility of a stability correction in such conditions is subject to debate.

Section 4.2.4 presents some of the most important findings of this paper. I am not sure that it is sufficiently highlighted.

Page 12 Line 31 The introduction of the sensitivity to climate input in a section dedicated to the interannual variability of the skill of the different members is a bit confusing. It is hard to see why the authors choose to introduce the dependence to forcing uncertainty here while they have ignored it in all the previous sections. It is definitely useful to discuss the dependence and robustness of the results as a function of the forcing, but I would recommend to better isolate this discussion in a dedicated section instead of introducing it this way at the end of an already dense and complicated paragraph.

Section 4.4 Despite the limitations already mentioned between observed river discharges and simulated snowpack runoff, I really like this approach (in terms of methodology). I definitely agree with the implication discussed Page 15 lines 23-25. I would even say that analysing the correct climate sensitivity of the different members might be a more robust way of members selection than what have be done at the moment on direct evaluation variables. It might be a methodological recommandation for further multiphysics snow modelling as well as for the ongoing evaluations of the ESM-SnowMIP models, which can be mentioned in the discussion of this paper.

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Discussion paper