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Interactive Comment

Interactive comment on "Exploring the impact of forcing error characteristics on physically based snow simulations within a global sensitivity analysis framework" *by* M. S. Raleigh et al.

Anonymous Referee #3

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Overview This manuscript explores the relative effects of bias and error distributions on the Utah Energy Balance model's sensitivity across Peak SWE, Ablation Rates, Snow Disappearance, and Sublimation predictions. The work exploits detailed forcing observations at 4 seasonally snow covered sites: (1) the tundra Imnavait Creek in the Brooks Range in Alaska, (2) the Col de Porte site in the Chartreuse Range in France, (3) Reynolds Mountain in Idaho, and the Swamp Angle Study Plot in the San Juan mountains of Colorado. The core contention of the work is that forcing bias and errors could dominate structural and parametric uncertainties for snow-affected regions with strong observation limitations. Overall I found this hypothesis somewhat self-evident,

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Interactive Discussion
Discussion Paper

although the overall study does highlight the importance of observation errors and uncertainties. I believe this manuscript requires revision to reach its full potential.

Major Comments

1. Limited Analysis: The core results in Figures 5-11 are discussed with extreme brevity and little analysis. The authors have made the chose to provide a more detailed exposition in their Discussion but at present the Results do not even orient the reader very well across individual plots. Figures 5-8 are summarized in text that mixes results across figures and severely limited in its value. The question that emerges when reading this is that either the authors could compress their results into fewer and better designed figures or they could tease more model related insights in their analysis text.

2. Discussion Disconnected from Results: The most interesting portions of the discussion relate to the contention of the relative importance of structural uncertainty to forcing errors. Unfortunately, this text references other published work strongly and does a very poor job connecting to directly to the Results/Figures of this paper. Transitioning from Section 4 to Section 5 almost feels like your reading a different paper. Overall the structure and writing of the work varies significantly from the well written Introduction, the detailed Methods, and more detailed Discussion versus the extremely cursory Results.

3. It is unclear how generalizable the results are beyond this study: Many of the results are not very insightful and seem to convey a very place-based specificity for deviating cases. The reporting of sensitivities in the Results are not well articulated in terms of their dependency on site location, the nuances of the Utah Energy Balance model, and scenarios. In its present form, I am not convinced that manuscript provides insights and it may be conflating several factors that could influence the differences in sensitivity (model choice, site selection, scenarios). Explanation of the stronger results, such as distribution choice minimally impacts computed sensitivities, is limited and not compelling. The core of the Discussion section is the best overall text of the paper.

It would have been far better to lead with your core hypotheses in the Results section and test them explicitly through the analysis of your results. The Discussion would then emphasize key caveats, insights, and implications.

Minor Comments

1. It would have been interesting to explore 2nd order and 1st order differences from the total indices in the results.

2. A better explanation of the scales assumed in the measures used to report sensitivities and caveats as to what cannot capture would be helpful.

3. Very little treatment is provided for the convergence rates of the total order indices and their associated bootstrap intervals as a function of your sampling.

4. How stable and/or separable are the factor prioritization rankings? What results have higher confidence?

5. It would improve the manuscript to better understand the justification of the ranges tested in the Sobol analysis. Would a slight change in your a priori ranges change factor rankings?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 13745, 2014.