

## ***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.***

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In this open discussion forum/review, the other reviewers have amply summarized the contents of this manuscript, so I don't find the need to restate the contents of this work. The other reviewers have also made some excellent suggestions. The paper is well-written and provides a potentially-extensive analysis of errors that haven't been previously assessed. My major concerns are largely in line with the three major comments provided by Referee #3. Once addressed this work would move from a cursory analysis to an extensive one. As you can tell already, I would also like to see a better discussion of the results. Particularly, a more extensive analysis of how some of

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the site-specific results may/may not relate to site-specific climatology. This type of analysis could be initiated by providing a summary of conditions at each site during the years of analysis. Meteorological summary statistics with a brief description in the Study Site section should be included. This would give the readers (and the authors) guidance as to how the snow regimes differ at each site and how that might be influencing findings/results. These observed differences might be correlated with the modeling results providing greater context and transferability of the presented findings. Further suggestions follow:

Study Sites: as mentioned above, please summarize the observations at each site. This should be included in Table 1.

Lines 98-100 (the precipitation corrections): Nowhere in this paragraph is the term “undercatch” referenced. All prior works on these types of adjustments have been based on the theory of wind-induced undercatch. Schmucki et al. is certainly not the only work that should be referenced here. Given that I think the authors are trying to adjust for this process, a 60% adjustment at IC is a very large number (Schmucki et al. applied increases of 5-17% to account for undercatch)! Is there something else going on at this site (e.g. the SWE measurement is located in an enhanced deposition zone, wind speeds are extreme, etc.). Something needs to be stated to justify this large an adjustment.

On the other side of the coin however, the question of why was there a need to decrease the precipitation measurements at CDP and RME begs for an explanation. Perhaps this is reflective of a modeling deficiency or errors in other observations? A large amount of prior modeling has been conducted at these two sites. I am particularly familiar with the work done at RME where in order to properly model snow evolution at that site it was necessary to adjust the shielded-gauge precipitation catch for undercatch. The “corrected” published data, which generally increased solid precipitation by 10-12%, reflects the undercatch correction which has been applied in every study I know of that has been conducted at this site. This includes the 25-year analysis pre-

sented in Reba et al. (2011), which had a Nash-Sutcliffe efficiency coefficient of 0.90 for modeled SWE over the entire period. So I ask, why the need to decrease the data in order to properly model SWE in the current work? As the authors note, accurate precipitation data is vitally important to simulating SWE evolution. A more detailed explanation is needed to explain these eye-catching adjustments that were necessary to properly model SWE.

Lines 236-238. I think this sentence would sound better if it was re-written in a manner that stated you provide a “brief (or other adjective)” description while further analysis/details/information can be found in Saltelli and Amnomi. (Just a personal opinion there).

Section 3.3.3. As mentioned in F. Pianosi’s comment, the transition from  $\theta$  (parameterizations) in (1) to  $\theta$  (forcings) in (2 and 4) should be cleared up.

Lines 415-420. Could you please provide some direct quotes of the structural uncertainties found in Essery et al. (2013) so that the readers of this manuscript can directly see these comparisons rather than having to dig up the Essery work?

Lines 446-448. The Zuzel and Cox findings are being presented out of context. Zuzel and Cox assessed the most important factors for snowmelt for a given snowpack; precipitation (or accumulation amounts) was never a consideration in their analysis. The current findings are really not so “surprising” as the entire winter is analyzed including both accumulation and ablation phases. Great care should be taken when comparing the current findings to research findings solely focused on the ablation phase. If you choose to continue to use this reference, please review the work fully and put it in its proper context.

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

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