

## ***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.**

**M. S. Raleigh et al.**

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Received and published: 16 April 2015

Note: reviewer comments are in italics and the authors' responses and manuscript revisions are in normal face.

**Comment:** *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.*

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**Response:** Thank you.

**Comment:** *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 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.*

**Response:** This is a reasonable point, and we can include more in-depth reporting of the results. While there may be interesting linkages between climate and model sensitivity, we note that we are hesitant to generalize relationships between site geo-characteristics and sensitivities indices because of the relatively low number of sites represented ( $n=4$  sites, 1 year each) and the confounding number of differences between our sites (e.g., snow climate, latitude, elevation, wind exposure/sheltering, etc.). We would require a much larger population of snow measurement sites in order to more robustly test relationships between sensitivity indices and site characteristics such as elevation and latitude. A successful example of relating climate characteristics to sensitivity can be found in van Werkhoven et al. (2008), which had 12 sites and 39 years each, making it possible to explore inter-site and inter-annual variations in climate and linkages to model sensitivity. We now emphasize in Section 2 that we selected the four sites to check for climate dependencies, but are unable to generalize the results due to the low sample size.

**Manuscript Revisions:** We have expanded the results and discussion sections to include more in-depth analysis of the site-specific sensitivities and our views on the

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generalizability of the results, and we now expand Table 1 to include summary statistics of site meteorology for context.

We now emphasize in Section 2 that we selected the four sites to check for climate dependencies, but are unable to generalize the results due to the low sample size. We note in the discussion however, that there are common results that emerge across all sites, such as the dominance of precipitation bias on SWE, ablation rates and snow disappearance (NB scenario) and longwave bias on all four outputs (NBlab scenario). This suggests that there may be common features in model sensitivity to forcing errors across distinct climates.

Further suggestions follow:

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

**Manuscript Revisions:** We have expanded Table 1 to include summary statistics of the meteorology at each site (temperature, precipitation, and wind only).

**Comment:** *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.*

**Manuscript Revisions:** We now include the term “undercatch” in this paragraph and provide more references. The 60% correction at IC is consistent with analyses of undercatch errors at Wyoming-type gauges in wind-blown areas in the Alaska tundra

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(Yang et al., 2000), and we now make a note of this.

**Comment:** *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 presented 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.*

**Response:** This is an excellent point; we can understand how this is eye-catching, as the pervasiveness of undercatch errors makes it a rare necessity to decrease precipitation data. As we initiated our analysis, we found that running an “off-the-shelf model” (i.e., no parameter adjustments) with “off-the-shelf forcing datasets” (most with precipitation undercatch adjustments already made) rarely resulted in close agreement (i.e., within 10%) of modeled and observed SWE. We can point to multiple sources of uncertainty here, including: (1) model forcing, (2) model parameters, (3) model structure, and (4) model evaluation (e.g., SWE) data. Because you are most familiar with RME, we will focus on that site (WY 2007) as an example to explain why adjusting the initial precipitation data was the most straightforward approach to arrive at reasonable SWE simulations (relative to the observations).

In Figure 1 (this document, see below), we compare SWE and accumulated precipita-

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tion and snowfall datasets at RME, and contrast uncertainties due to evaluation data, and model structure (rain-snow partitioning as an example), parameters, and forcings. We make the following observations:

- **Evaluation data uncertainty:** Snow pillow SWE generally agrees with snow course SWE, though the pillow SWE ablates more rapidly than snow course SWE in April (Figure 1a, below). The consistency between these datasets does not provide evidence that the evaluation uncertainty is causing the discrepancy between modeled and observed SWE.
- **Structural uncertainty (rain-vs-snow):** Using four different methods for delineating snowfall results in a range of about 180 mm of accumulated snowfall by season's end (Figure 1a, below). Snowfall delineated with dewpoint temperature (from Reba et al. 2011) underestimates SWE whereas snowfall delineated with a linear temperature threshold (UEB) overestimates SWE (Figure 1a, below). Because we are looking at accumulated snowfall and not SWE, this does not take into account the three distinct mid-winter melt events, so the simulations with the dewpoint-based approach will have more SWE underestimation than what is suggested in Figure 1a (below).
- **Parameters (rain-vs-snow):** Perturbation of the UEB rain-snow threshold temperatures results in a range of about 70 mm of accumulated snowfall by season's end (Figure 1b, below). For the selected parameter values, this range is smaller than the range encompassed by the four methods of delineating rain and snow (Figure 1a, below).
- **Forcing (precipitation):** Assuming there is still a bias due to under- or over-correction in the original data, we examine snow accumulation under the case of -30% to +30% biases (Figure 1c, below). A range in snowfall accumulation of 125 mm exists when considering +/-10% bias and 250 mm when considering +/-20% bias.

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Based on the ranges in snowfall accumulation in these comparisons (and neglecting other processes such as snowmelt), it appears that the most likely cause of the mismatch between modeled and observed SWE is either (1) structural uncertainty (selected rain-snow delineation parameterization) or (2) precipitation bias (on the order of 10-15%). Addressing (1) would require modifying the source code of UEB to incorporate a different parameterization, but this might be somewhat arbitrary because no independent dataset exists (to our knowledge) that can provide clues which rain-snow delineation method is most realistic at each site and should be selected. Therefore, we concluded that the more straightforward approach would be to address (2) by making some adjustments to the precipitation data.

We note that when forced with the precipitation data (no new adjustments), UEB consistently overestimates SWE throughout most of the season (Fig. 1d, below). In contrast, decreasing the precipitation by 10% yields closer agreement with the snow pillow SWE. The UEB simulations of SWE without new precipitation adjustments exhibit a Nash-Sutcliffe (NS) of 0.88 and RMSE of 40 mm, relative to snow pillow SWE. When the 10% decrease in precipitation is applied, UEB yields a Nash-Sutcliffe (NS) of 0.95 and RMSE of 25 mm SWE. These NS values are in fact comparable to the performance of Isnobal that you have referenced (from Reba et al., 2011).

Finally, we note that calibration of model parameters is a step that usually occurs after the sensitivity analysis has determined the most sensitive factors, and this is a reason why we did not calibrate the model prior to the analysis. However, if we consider the interplay between optimal rain-snow threshold parameters in UEB and a potential precipitation adjustment, we find that it is essential to adjust the precipitation in order to find an optimal parameter set (Figure 2, below). Leaving the precipitation unchanged would require potentially unrealistic snow and rain threshold temperatures (-4 C and 0 C, respectively) to arrive at the most optimal SWE simulations (Figure 2b, below), and these parameters are at the edge of the parameter space (suggesting they are not really the optimal parameters). By decreasing the precipitation by 10%, it becomes

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possible to find a parameter set that is both optimal and realistic. While we are neglecting other processes, this brief analysis provides support for adjusting the precipitation data.

**Manuscript Revisions:** We briefly expand our discussion at the end of section 2 of why we adjusted the precipitation data.

**Comment:** *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).*

**Manuscript Revisions:** We have changed the sentence to say “Below, we provide a brief summary of the Sobol’ sensitivity analysis methodology but note that further details can be found in Saltelli et al. (2010).”

**Comment:** *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.*

**Manuscript Revisions:** We have clarified this point by introducing a distinct variable ( $\phi$ ,  $\phi$ ) for the forcing errors.

**Comment:** *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?*

**Response:** There are no direct quotations in the Essery et al. work that are relevant to our discussion. In order to provide a more direct comparison, we have obtained the modeled SWE ensemble from Richard Essery and have created a new figure comparing the forcing uncertainty to structural uncertainty (see Figure 3, below). This illustrates our point that structural uncertainty is only marginally larger than uncertainty due to measurement precision for peak SWE and snow disappearance, and that field uncertainties (due to wind drift and gauge undercatch) are larger than the structural un-

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certainty. The uncertainty due to structure for ablation rates however is notably higher than the gauge and lab levels of uncertainty.

**Manuscript Revisions:** We now include the figure comparing the forcing uncertainty to the Essery et al. (2013) structural uncertainty and focus the discussion around that figure.

**Comment:** *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 it's proper context.*

**Response:** Thank you for catching this problematic comparison.

**Manuscript Revisions:** We have rephrased this to say: "Prior investigations into the relative importance of forcings to ablation were typically framed for a snowpack at the end of winter, such that P uncertainty was not considered (e.g., Zuzel and Cox, 1975)."

## REFERENCES

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

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11, C6799–C6810, 2015

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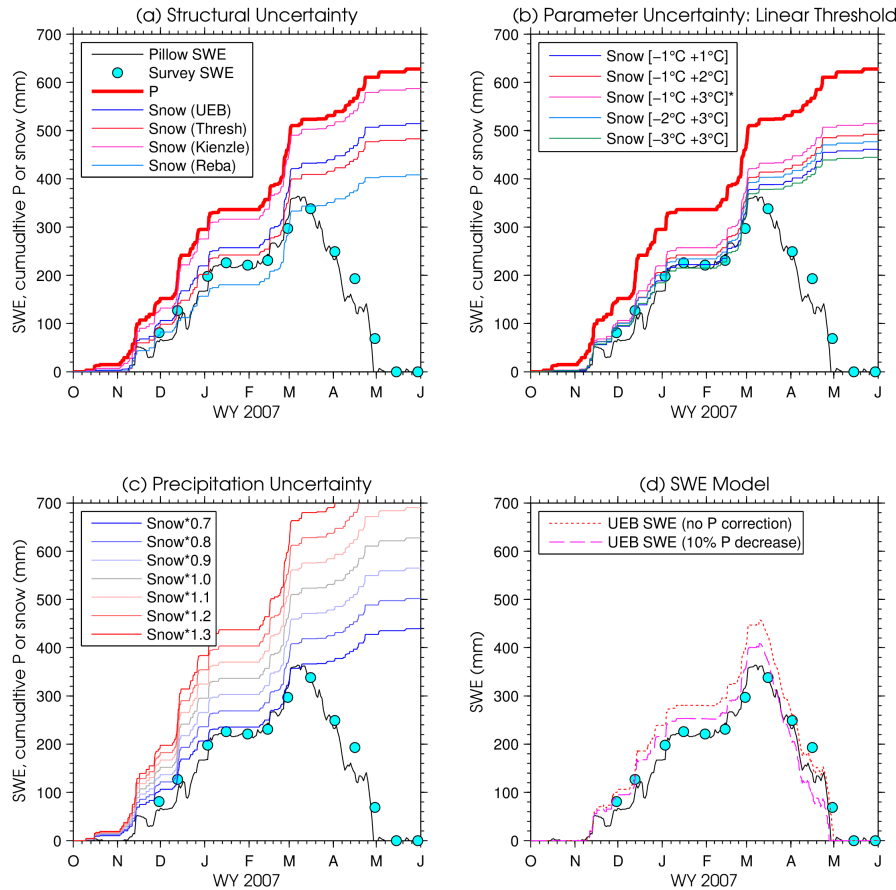
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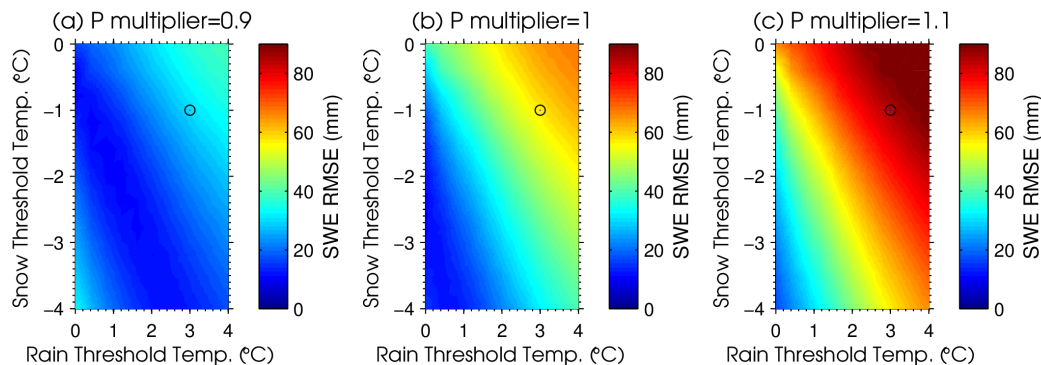
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**Fig. 1.** SWE, accumulated precipitation and snowfall at RME (WY 2007) as a function of uncertainties in rain-snow (a) structure and (b) parameters, and (c) precipitation. (d) Modeled SWE with adjusted P.



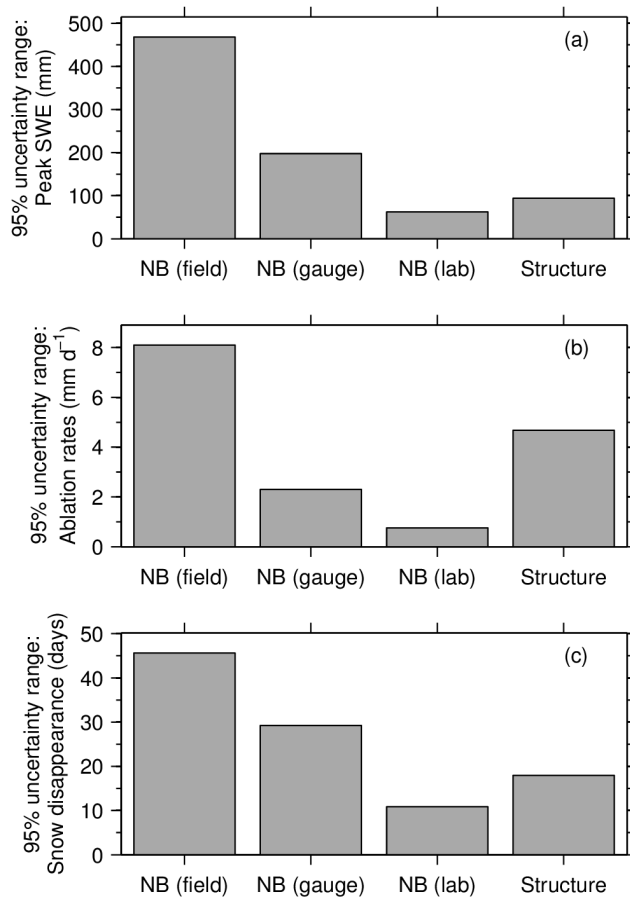
**Fig. 2.** RMSE in modeled SWE at RME (WY2007), as a function of rain-snow thresholds and precipitation multipliers of (a) 0.9, (b) 1.0, and (c) 1.1. The black circle are the default UEB rain-snow parameters.

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**Fig. 3.** 95% intervals in (a) peak SWE, (b) ablation rates, and (c) snow disappearance date at CDP in WY2006 for three forcing uncertainty scenarios and the Essery et al. (2013) structural uncertainty.