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Hydrology and Earth System Sciences
Discussions

# *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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The work presented by Raleigh et al. investigates the impact of uncertainty in individual meteorological forcing variables on simulation of snow processes at selected sites using the Utah Energy Balance (UEB) model. The manuscript investigates how different error distributions and magnitudes can impact quality of simulations of key snow variables by using the Sobol' sensitivity analysis methodology. The number of model simulations needed for individual sites/experiments varies approximately between 70,000 and 130,000. The authors found that model outputs were generally more sensitive to systematic biases in forcing in comparison to random error. In addition, simulations



indicated that model was more sensitive to the magnitude of forcing rather than the distribution of errors.

I particularly like the manuscript and I think it should be accepted for publication after minor revisions (see my comments below). This is a good example of model diagnostics employed in a relevant context (understanding impacts of forcing uncertainty). We usually focus on uncertainty in parameters, but forcing can play a significant role (especially with such models where both local in-situ and global gridded forcing data are commonly available). The large number of model simulation does not concern me because (1) evaluating the total number of simulations without actual simulation time is somewhat meaningless (how long does it take to run a single year simulation in this model?), and (2) the authors are clearly using such approach to diagnose model uncertainty in detail and recognize that there are more simple approaches that can be used but the emphasis here is on the benefits of using Sobol'. Finally, the manuscript is well written, it explains the strategy very well and includes very good tables and figures.

General Comments:

[1] Section 2: If the goal was to understand impact of forcing uncertainty on simulations, I do not understand why precipitation adjustments (due to wind conditions) were employed prior to the simulation? It would have been interesting to see the overall results related to precipitation. I suspect that would increase uncertainty even more.

[2] Section 3.1: Very good explanation of why such metrics were used. Other studies should follow this example when listing metrics used in their experiments.

[3] Section 3.3.2: The Sobol' method assumes factors are independent from each other. Can you safely assume that for each forcing data analyzed (e.g., Tair and RH)?

[4] Section 4.2: Could the fact that Qli bias was found to be the most important factor (given its low error magnitudes compared to Qsi) indicate some structural limitation in radiation partitioning parameterization in the model (longwave versus shortwave radia-

tion)?

[5] Section 5: I particularly like the discussion on limitation of the analyses described by the authors.

[6] Table 2: What is the limitation of fixed ground heat flux? Isn't it calculated in the model? In addition, I imagine that setting it to zero all the time could potentially be problematic.

[7] Figures 1 and 2: Excellent figures explaining/summarizing the methodology employed in the study.

[8] Figure 5: Have the authors looked at relationships between certain site characteristics and the magnitude of sensitivity from each factor. For instance, Figures 5 and 7 show an interesting relationship between site elevation/latitude with precipitation forcing for snow disappearance (third column in both figures). Given the site arrangements in the figure, both cases show an increase in sensitivity with elevation (and consequently decrease with latitude). With respect to precipitation and elevation, this can show the difficulties of measuring precipitation according to elevation (especially given the fact that most continuous weather monitoring networks are placed in low/mid-elevation locations). I wonder if there could be other relationships the authors can investigate to see more of those relationships. I see this as a good additional exercise to understand forcing uncertainty and model diagnostics.

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