Hydrol. Earth Syst. Sci. Discuss., 11, C6793–C6798, 2015
www.hydrol-earth-syst-sci-discuss.net/11/C6793/2015/

Hydrology and Earth System Sciences
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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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Received and published: 15 April 2015

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

Comment: *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 num-*

ber 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.

Response: We thank you for your positive and constructive feedback.

General Comments:

Comment: *[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.*

Response: The underlying assumption made here is that the original precipitation data had an unresolved bias prior to the simulations. We wished to begin the sensitivity analysis with reasonably realistic simulations of the observed snowpack, and hence made these precipitation adjustments. We argue that this is not problematic because we do not compare the sensitivity analysis SWE simulations to the observed SWE.

Comment: *[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.*

Response: Thank you.

Comment: *[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)?*

Response: Thank you for making this excellent point. You are correct that in reality a bias in Tair will induce a bias (of the opposite sign) in RH. To avoid this issue, we could have constructed the analysis such that we considered errors in Tair and the vapor pressure, but did not do this for simplicity and for general applicability (given that many datasets report RH and not vapor pressure).

Manuscript Revisions: We now state in section 3.3.2: "A key assumption to the Sobol' approach is that the factors are independent; hence, our analysis does not consider the case of when specific error types are correlated (e.g., a positive measurement bias in Tair that propagates a negative bias to RH)."

Comment: *[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 radiation)?*

Response: We think that the relative importance of Qsi errors is less than that of Qli errors because the high albedo of snow minimizes how much energy Qsi transfers to the snowpack.

Manuscript Revisions: We now note this in Section 4.2: "In one sense, this was surprising, given that the bias magnitudes were lower for Qli than for Qsi (Table 3). However, the albedo of snow minimizes the amount of energy transmitted to the snow-

pack from Qsi, thereby rendering Qsi errors less important that Qli errors. Additionally, the non-linear nature of the model may enhance the role of Qli through interactions with other factors."

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

Response: We appreciate that you liked this discussion.

Comment: *[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.*

Response: The snow model provides an option for turning off the ground heat flux. Because ground heat flux typically has a small contribution to the energy balance, it is assumed negligible in some snow modeling applications (e.g., Essery, 1997; Jepsen et al., 2012; Letsinger and Olyphant, 2007), and we chose to mimic those approahces. This indeed would be problematic for calculating the energy balance during snow-free periods and in areas with intermittent snowpacks, however, the focus of the study was on the snow-covered periods (minimum continuous duration of 15 days, as stated in section 3.3.5).

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

Response: Thank you.

Comment: *[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.*

Response: We had not considered this relationship before and thank you for making this suggestion. While this is worthy of further attention, 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 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.

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