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Geoscientific Model Development Discussions

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Interactive comment on "ISBA-MEB (SURFEX v8.1): model snow evaluation for local-scale forest sites" *by* Adrien Napoly et al.

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General comments:

The manuscript presents valuable model developments focused on forested snow-process modeling in a popular land surface model. The model developments are structural in nature and specify the relationship between vegetation and the snow surface simulated below the vegetation canopy. As compared to the undeveloped land surface model, simulated snow duration and soil temperature are significantly improved. The developments presented here are valuable to the scientific community and deserve prompt publication pending a small number of minor and technical revisions. These revisions are mainly contextual and should not change any major findings or results.

Scientific comments:

I really enjoyed this manuscript and commend the authors for their hard work. I have three overarching comments:

1. I liked the organization of this manuscript. The results were presented in a fashion that made both the model developments and findings easy to understand. On my second readthrough, I found that my mind was already primed to identify the issues with the default ISBA model that made modeling snow in forests troublesome. However, when I first read through this manuscript, I wanted to know more about the sources of model errors before getting into the model details. Only in lines 195 – 200, and the following sections describing MEB, did I start to understand what was being corrected. Specifics about the changes to the model framework and how that influenced the snowpack could be put earlier to prime the reader for what to expect. This need not be lengthy (only a few sentences) and could be included in an individual paragraph, headed by the sentence on lines 85 – 87.

2. I was confused about the simulation setups. For instance, it appeared like the simulations were performed and compared versus observations at a point, although 1) the model is often used for distributed simulations, and 2) the use of snow/vegetation fractions implied a gridcell or patch of much larger size. While the ISBA parameters (transmission coefficient, veg parameter, etc.) were left unchanged or defined by the datasets in Table 2, the MEB canopy longwave radiation transmission was tuned. Although the tuned radiation transmission (0.4) was close to the default transmission (0.5), the snow depth RMSE for the default transmission was larger (Figure 11) and approached the default ISBA simulations (Table 8). It would be nice to include the MEB simulation using all default parameters in a comparison (maybe similar to what was done for the simulation with no interception in Figure 6).

3. The model developments are valuable for not only offline land surface models. I was therefore curious to hear more about how the authors expect the ISBA MEB developments to influence coupled and distributed land-atmosphere simulations. Also, how do you expect the model developments to perform or differ in landscapes (such as the

United States Pacific Northwest Cascades) where elevation gradients are large, temperatures are warmer, snow depth is typically deeper, and the canopy intercepts much larger amounts of snow for longer portions of the snow season? A brief discussion about model transferability would be valuable. Finally, from a modeling perspective, how much (if any) do the MEB developments increase the computational cost?

Technical comments:

Line 2 "...adopts a default configuration...": Does this mean the default ISBA model? Maybe consider simplifying this whole sentence to clearly state that the ISBA model uses a composite soil-vegetation energy budget that struggles with representing snow in forested regions.

Lines 11-12 "A consistent positive impact for soil temperatures...": With the statistics that follow (-6.2 to -0.1 K), I am not sure if "consistent positive impact" means that the soil temperature always increases (positively), or if simulated soil temperature improves.

Line 14 "...time of ablation...": Does this mean the date of first snow-absence (or melt-out), or the rate at which snow melts? You use "last day of snow" in the results. For consistency, I would pick one and stick with it.

Line 16: "cause" should be "caused".

Lines 20 – 21 "...one third of which consists of boreal forest which corresponds to subarctic and cold climates.": Maybe revise for conciseness to "...one third of which consists of boreal forests in subarctic and cold climates".

Line 52: "2009, Rutter et al." should be "Rutter et al., 2009".

Line 55 – 56 "...they determined that liquid water retention was a key process required for simulating the accurate timing and amount of snowmelt and thus discharge": Can you be more specific? After reading the Boone et al. (2004) paper, I am still not sure what you mean here. I am guessing that liquid water retention references the soil column and that soil columns with a larger holding capacity simulated daily discharge better. However, the composite ground representation seemed to be a first-order driver of whether snowmelt was even entering the soil column at the correct time. By "liquid water retention" do you mean delayed snowmelt by non-composite snow schemes?

Line 62 "…certain snow processes…": I would be explicit here (interception, solar shading, longwave enhancement, etc.). What processes require "explicit representation of the vegetation canopy"? Also, what does "explicit" mean (canopy height, canopy density, subgrid canopy coverage/placement, vegetation species, LAI, etc.)? The required information about the canopy vary across different models.

Line 69: "GGMs" should be "GCMs".

Line 83: "computations" is misspelled.

Line 101 "…certain key features": Can you be explicit here (snow depth, SWE, etc.)?

Line 116: I would delete "for research studies which consists in" and put the colon after "…default ISBA configuration, where:"

Line 137: Some models partition snow layers based on SWE instead of snow depth. It is worth mentioning what ISBA does here.

Section 2.1.4: I think the discussion at the end of the section highlights one of the most important model developments. This is alluded to briefly in the abstract, in the parenthetical from lines 147 – 148, and lines 165 – 166. Sections 2.1 and 2.2 demonstrate the differences between these two model developments (ISBA and MEB) well. However, the impact on heat/energy fluxes (Figure 3) is already demonstrated in Napoly et al. (2017). I think these results could be referenced early-on in this manuscript and used to elaborate how these changes are important for this snow-modeling investigation here. I also think that Figure 2 is a great conceptual that could be referenced earlier to demonstrate how the models differ in their subgrid representation of snowpack.

Lines 258 – 262: I have concerns about the assumption that intercepted snow has negligible effect on the canopy albedo. Although Pomeroy and Dion (1996) found canopy structure and solar angles to be the first-order drivers of radiation absorption by the canopy, multiple studies since have linked differences between observed and modeled albedo (and differences between models) to modeled canopy interception (e.g., Bartlett et al., 2006; Loranty et al., 2014; Roesch and Roeckner, 2006; Thackeray et al., 2014). What sort of impact do you anticipate if the canopy albedo were to vary with interception? Canopy typically intercepts much more snowfall in the United States Pacific Northwest and many other maritime snow climates. Therefore, how do you expect this assumption to influence simulations in other climates?

Lines 270-271: At this point of the text, I want to know more about the canopy interception model and parameters that are used. I think it is first mentioned in Line 482 that you use the Hedstrom and Pomeroy (1998) method. I think the Hedstrom and Pomeroy parameterization is a good choice since it was developed for this particular region. However, it is worth noting that the Hedstrom and Pomeroy method varies dramatically from the Storck et al. (2002) method which does better for regions with warmer, and more cohesive snowpack. In fact, a number of snow interception parameterizations exist (e.g., Hedstrom and Pomeroy, 1998; Roesch et al., 2001; Storck et al., 2002), most of which are heavily-parameterized and are not very transferable between climates. I really like the discussion on snow interception sensitivity in Section 4.2.4. I think a simple 1-2 sentence acknowledgement about different interception routines and the impact of tuning interception parameters on modeled snowpack would be valuable.

Line 288: Delete “In order” or make “To” lower-case.

Section 3: It would be nice to know what resolution these simulations were being performed at since measurements are at points. How co-located in space are observations? Do you expect any variability from ground measurements like manual SWE measurements which likely did not come from the same spot each time? I especially find the SWE measurements in OBS and OJP, and the accuracy versus the simulations, interesting in water-year 2004 (Figure 5).

Line 355 – 357: Is the difference in sublimation averaged across all model domains? I would expect the ratio between in-, and under-canopy sublimation to be different as site characteristics vary. In fact, in lines 362 – 363, it looks like it does vary across the sites. What is the ratio between "sublimation of the snowpack and total sublimation of snow" (lines 362 – 363)? Is this the ratio between the snow sublimated from the ground layer versus the total (sublimated from the ground and canopy)? If so, the average ratio here (0.45) seems to align with your 12%:27% split presented in the topic sentence.

Line 365: I think Figure 6 is referenced before Figure 5.

Line 367: For consistency, "Table 5" should be "Tab. 5".

Lines 380 – 385: Is RMSE calculated only for periods where snow exists in 1) the observation? 2) either the observation or simulation? or 3) for the full 3-year period including snow-absence? I think this may have been answered in lines 430 – 440. If so, please move this up earlier.

Line 500: Change "has" to "was" or "has been".

Figure 1: There is no caption for this Figure. The layout for this figure may also be difficult for typesetting. Could these figures go beside each other with labels specifying the ISBA and ISBA-MEB frameworks?

Figure 5 and Figure 9: The horizontal time axis represents an explicit date (as compared to an annual composite or average). Can "Time (Year)" be changed to an explicit date (e.g., Jul 01 through Jul 04)?

There are no references to Table 6 or Table 7 in the text.

Figures 7, 8, 10, and 11. Label subplots (a, b, c, etc.) in accordance to references in the text and figure captions.

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