

Responses to Reviewers for Water-856153

As requested by the editor, this new version of the responses to the reviewers presents in more detail the changes put forward following the comments of Reviewer 1.

Reviewer 1

Comments and Suggestions for Authors

This study investigated impacts of data availability on the accuracy of temperature index snow model at a study area in Canada. The topic of this study seems to be interesting. However, the methodology is not described well. Currently, it is difficult to properly evaluate this study.

Specific comments:

1) The methodology is not described well in this manuscript.

We agree with the reviewer that the methodology section was not sufficiently well described in the previous version of manuscript. In summary, we have added the following:

a) A better description of watershed:

In our previous version of manuscript, there was a brief description of watershed. As suggested by the reviewer, we added information on the average slope, elevation, and on the topography of the study area, as well as on some geological characteristics.

“It focused on sub-basin 7 that drains an area of 3.49 km² (Figure 1), with an average slope of 10.7° and elevation ranging from 725 to 977 m ASL [51].”

“This region lies in the Laurentian Mountains in which the U-shaped valley is covered with glacial deposits [51].”

b) Details on SWE's sampling strategy

In order to better describe the SWE sampling strategy, we added the following sentence:

“An intensive manual snow survey was conducted during winters 2016-17 (W1) and 2017-18 (W2) around each snow-profiling station, for a total of 1061 observations. These samples were collected from a subplot nearby the snow profiling station, where a minimum of 5 and a maximum of 12 samples per visit were collected depending on the exact canopy structure.”

c) Flow diagram

In order to improve the readability and methodological clarity, we placed the flow diagram near the melt modelling section (2.5). We believe such change helps to showcase the modelling strategy implemented in this study.

d) Addition of references

We have included some additional references to support our statements in introductory and methodological sections, including papers referring to challenges in identifying the precipitation phase (Wayand *et al.*, 2016), errors in physically-based snow models (Raleigh *et al.*, 2015), as well as novel satellite-based approaches to estimate snow interception (Russell *et al.*, 2020).

e) Justification for the use of sublimation as an input to TI model

We added the following sentence to clarify the use of observed sublimation as an input to our models:

“Snow sublimation is difficult to monitor in situ as it requires sophisticated instrumentation [62]. As such, TI models are typically supplied with ad hoc average sublimation values distributed equally

among the studied period. Supported by eddy covariance sensors, this study aims to identify the effect of inserting direct observation of snow sublimation into the base TI model”.

f) Use of empirical equation 11 to estimate snow density from snowpack depth

We have added the following sentences:

“Snow density undergoes gravitational settlement and metamorphism over time. The formulation presented in Equation 9 estimates fresh snow density with no inclusion of abovementioned processes. In order to estimate the aged snow density (ρ_{sa}) in the above equation, we utilized the empirical formula (Equation 11). Note there are obvious shortcoming when empirical formulas like Equation 11 are used. Nevertheless, in many instances they represent our “best guess” and as such, are often implemented in land surface models such as the Canadian Land Surface Scheme (CLASS).”

In addition, we have fixed all the minor issues raised by the reviewer. We believe this has improved the readability of the methodology section, and as such this should make our methodological choices clearer.

2) It is difficult to understand the purpose of each comparison.

To help better understand the purpose of each comparison, we added the following sentence at the end of the Introduction, in the paragraph presenting the objectives of the study:

“By conducting a series of tests that include one process/input at a time and quantifying the associated performance, we seek to highlight the relevance of taking these processes into account in a temperature index model.”

3) Meanwhile, there are various errors in modeling, and the effects of the errors are very complicated. Removing a kind of error in modeling does not always increase the accuracy of the modeling. For instance, Test 6 did not show the best results in this study. Currently, it is difficult to understand the importance of the comparisons in this study.

We agree with the reviewer, there should not be systematic improvement in model performance when additional input variables or processes are included. However, as mentioned in the Introduction, it is also important to point out that these variables/processes may help better describe the snow accumulation and melt. TI models are empirical approaches regularly applied in remote locations with limited data availability. Thus, in this study, we aim to highlight potential deficiencies of TI models when used in forested areas providing some inputs/processes which are often used in physically-based snow models such as SNOWPACK and CROCUS.

4) Please avoid using a one-sentence paragraph.

Thanks, we rectified the problem.

5) Please use the same font for the variables between in the equation and in sentences.

Thanks, we changed them.

6) L.56: “Besides, difficulties in the spatial interpolation of variables, such as wind speed and incoming radiative fluxes, hinder their applicability at larger scales” Nowadays, atmospheric reanalysis data or its downscaled data can be used as input to a snow model.

Good point. We have included the following two sentences:

“For research applications, reanalysis datasets such as ERA5 provides an option where these inputs are readily available at hourly timesteps [38]. Nevertheless, one of the shortcomings of such dataset is its spatial resolution (i.e. 31 km × 31 km), thus creating difficulties when undertaking small-scale studies.”

7) L.141-142: Please briefly describe the topography of the study area.

See answer to comment 1. We have added the following sentence:

“It focused on sub-basin 7 that drains an area of 3.49 km² (Figure 1), with an average slope of 10.7° and elevations ranging from 725 to 977 m ASL”.

8) L.146-148 : Please add a reference for this description.

Thanks, we have added a reference to this description.

9) L.152-154 : “Turbulent fluxes were analyzed over 30-min spans and averaged to be on an hourly scale like the rest of the model inputs.” This sentence is not clear.

We have rephrased it. The new version now reads:

“These data were available at 30-min spans and averaged at hourly timesteps similar to the other model inputs.”

10) L.152-164 : “Based on numerous snow pit surveys, the snowpack temperature during that period typically ranged between −0.2C to −0.7C, thus existing gaps were replaced by a constant temperature of −0.45 C.” Please add references.

We refer here to snow pit surveys that we conducted as part of the overall study, but which are not described in this paper. We are working on a publication where these data will be featured. We have reworded the beginning of the sentence as follows: “Based on our field-based snow pit surveys [...]”

11) L. 165-166: Is the difference between the actual snow surface temperature and the recorded data closest to the snow surface negligible for the simulations in this study?

This is the hypothesis we are making. Given that the vertical spacing on the snow profiling station is quite small (10 cm), we are confident that the recorded data closest to the snow surface is a good proxy for the snow surface temperature. We have made that clear in the text:

“Snow surface temperatures were estimated by taking the measurements reported by the thermocouple in the snowpack closest to the surface, neglecting differences with the actual surface temperature.”

12) L.172 : Please define “the base TI model” before use.

Thank you for your suggestion. We have added a small paragraph defining the base model. Also, we have placed flow chart diagram closer to the abovementioned paragraph to improve the clarity of our methodology.

The paragraph reads:

“Figure 2 summarizes the modelling strategy, which is divided into two phases. The first one aims to determine the best model or base model, with a very succinct representation of the processes, and the second one aims to evaluate the performance gains associated with the addition of supplemental processes. ”

13) L.173-175 : Please explain it in a little bit more detail here.

We have modified this section accordingly. It now reads:

“During our search for the base TI model, we deliberately neglected key components such as interception and solar radiation and other relevant inputs/processes that affect snow accumulation and its melt. These factors were included in the second phase of testing (section 2.6) to isolate their associated gains.”

14) L.178-180 : Please define T_{air} . Also, what is the threshold temperature?

There should not have been T_{air} in the equation, so we removed it and replaced it with T_a . Thank you.

15) L.185 : Why is the unit of the end of daylight “h”?

It stands for the end of daylight for the current day, exactly as in Tobin et al. (2013).

16) L.201 : Please explicitly describe the difference between Equation 1 and 5. Is Equation 6 not used for Equation 1?

As described in the article, equation 1 represents the simplest version of a TI model, with a fixed melting factor (m_{fi} , see Table 1). Equation 5, on the other hand, includes a variable melting factor, m_f , described by Equation 6, and an additional term to account for radiation. We believe that these differences are already well presented in the paper and therefore have not made changes.

17) L. 225-226: This description is not clear.

As suggested by reviewer, we have now clarified this description. It now reads:

“As mentioned above, TI models commonly use a threshold temperature in the range of 0 to 2°C. The first test was designed to assess the impact of providing the measured snow surface temperature instead of using a constant threshold.”

18) L.228-231 : Please add more detailed explanation. What are compared? What is the purpose?

We have completely rewritten this paragraph. It now reads:

“Snow sublimation is difficult to monitor *in situ* as it requires sophisticated instrumentation (Molotch *et al.*, 2007). As such, TI models are typically supplied with *ad hoc* average sublimation values distributed equally across the study period [e.g., 3]. Supported by eddy covariance sensors, this study aims to identify the effects of inserting direct observations of snow sublimation into the base TI model. To do so, two different avenues were explored. In the first approach, we supplied the TI model with sublimation data recorded above a juvenile forest (site J4) for sites with a canopy, while in our second approach, we supplied the model with sublimation data that was recorded at the sapling site (near O2) for sites with shorter vegetation.”

19) L.229 : Does “the model” in this sentence the TI model?

Indeed, we have included ‘base TI’ as you can read above.

20) L.234: $\rho_{s_}$ -> ρ_{sa} ?

We have rectified this error, thanks.

21) L.235-236 : “As snow density undergoes metamorphism with time, the formulation utilized in Equation 9 fails to come up with accurate estimate.” Please add a reference. If none, please explain it in more detail.

Thank to your suggestion, we have now modified the sentence, added some reference to support our statement. It now reads:

“Snow density undergoes gravitational settling and metamorphism over time. The formulation presented in **Equation 9** estimates *fresh* snow density (Hedstrom and Pomeroy, 1998; Russell *et al.*, 2020), with no inclusion of the abovementioned processes. In order to estimate the snow density (ρ_{sa}) in the above equation, we utilized the empirical formula described in Pomeroy *et al.* (Pomeroy *et al.*, 1998), which is given as: [...]”

22) L.239 : I cannot find T_a in Equations 10 and 11. Is T_a related to Equation 10?

Thank you for finding this mistake, we removed the sentence with T_a .

23) Figure 3: It is difficult to see the red line in the plots.

True, there is very small difference between A1 and A2, and as a result the red line is difficult to see. We have added the following small comment to this effect in the figure caption:

“Note that the curves of algorithms A1 and A2 are almost completely superimposed.”

24) Figure 3: Why does the observation have the standard deviation?

Thank you for raising this issue. We have added the following statement to make this clear.

“An intensive manual snow survey was conducted during winters 2016-17 (W1) and 2017-18 (W2) around each snow-profiling station, for a total of 1061 SWE observations. These were collected from a subplot nearby the snow profiling station, where a minimum of 5 and a maximum of 12 samples per visit were collected depending on the exact canopy structure.”

25) L.266-270 : The observation seems not to be a single value at each time. How were RMSE and Pbias calculated? Was the mean value used?

Yes, observation contains multiple samples. However, we utilized average value to calculate RMSE and Pbias. We have added the following sentence in Section 2.5 to make this obvious:

“Modelled SWE was compared with the mean SWE measured at each site.”

26) L.280-289 : What are differences between this paragraph and the previous paragraph?

These paragraphs have been merged.

27) L.284-284 : “automatic snow depth measurements and field observations” Please explain these data in the Data inputs section.

Done, thanks. We have created a new section:

“2.3.5. Snow depth

An ultrasonic snow depth sensor (Judd Communication, USA) was mounted at each site, except for site O2 where a SR50 (Campbell Scientific, USA) was mounted instead. These sensors provided the time series of snow depth at hourly timesteps.”

28) L.297-299 : It seems that A1 is almost similar to A2.

Indeed, see answer to comment 23.

29) Figure 6: Please explain the panel c and d in the caption.

Done, thanks. The caption now reads:

“Figure 6. Importance of some relevant inputs. Water vapor losses to precipitation ratio (a) for the first winter (W1) and (b) for the second winter (W2). Air (T_a) and snow surface (T_s) temperature (all sites averaged) (c) for the first winter (W1) and (d) for the second winter (W2). In the above plot, the shaded region represents the ablation period and to its left is the accumulation period.”

Additional changes

- We have added three new reference to our manuscript (Raleigh *et al.*, 2015; Wayand *et al.*, 2016; Russell *et al.*, 2020).

References

- Pomeroy JW, Gray DM, Shook KR, Toth B, Essery RLH, Pietroniro A, Hedstrom N. 1998. An evaluation of snow accumulation and ablation processes for land surface modelling. *Hydrological Processes* **12** (15): 2339–2367 DOI: 10.1002/(SICI)1099-1085(199812)12:15<2339::AID-HYP800>3.0.CO;2-L
- Raleigh MS, Lundquist JD, Clark MP. 2015. Exploring the impact of forcing error characteristics on physically based snow simulations within a global sensitivity analysis framework. *Hydrology and Earth System Sciences* **19** (7): 3153–3179 DOI: 10.5194/hess-19-3153-2015
- Russell M, Eitel JUH, Maguire AJ, Link TE. 2020. Toward a novel laser-based approach for estimating snow interception. *Remote Sensing* **12** (7): 1–11 DOI: 10.3390/rs12071146
- Wayand NE, Stimberis J, Zagrodnik JP, Mass CF, Lundquist JD. 2016. Improving simulations of precipitation phase and snowpack at a site subject to cold air intrusions: Snoqualmie Pass, WA. *Journal of Geophysical Research: Atmospheres* **121** (17): 9929–9942 DOI: 10.1002/2016JD025387