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Author replies to the comments by Referee #2:

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The authors would like to thank Referee #2 (Dr. Wolfgang Gurgiser) for helping us to improve our manuscript. Your time and effort is highly appriciated – thank you very much!

Please note: page and line numbers in the updated manuscript might not be the same as in the previously submitted version due to changes in the text as well as due to utilization of a different latex template. All relevant changes in the manuscript are highlighted in red, all removed text is struck through.

GENERAL COMMENTS:

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1. Referee comment:

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Calculating surface snow melt from the surface energy balance means that temperature does not directly control whether there is melt or not. However, in the general model description (p. 8160, lines 10-15) the authors state that air temperature has to be at the melting point temperature of ice. If the model should be an "energy balance snow model" this condition should be removed.

Answer by the authors:

Referee #2 gives a definition of a surface energy balance model that somehow differs from our understanding and that of other members of the hydrological community (e.g. Mauser and Bach (2009) or Warscher et al. (2013)). We think the usage of the term "energy balance model" is not inadequate only because melt conditions are indicated by air temperatures equal or higher than the melting temperature of ice, as long as the actual melt for every time step is calculated on the basis of an energy balance approach. The reason why we derive melt conditions using air temperature as an indicator is due to the fact that we do not explicitly calculate the snow temperature in ESCIMO.spread. If we would do so, we could close the energy balance even for non-melt conditions (snow temperature defines outgoing longwave radiation) and derive melt conditions directly from the calculated energy balance. Without snow temperature beeing calculated for every time step, closing the energy balance is only possible for melt conditions (here assumed for temperatures equal or above the melting point of ice), where snow temperature can be set to the melting temperature of ice allowing closure of the energy balance with melt derived from the available energy at the snow surface.
To leave behind the assumptions described above would require the calculation of snow temperature for every time step and if possible for different snow layers, as currently realized only in the most physically based and complex snow models available (see SNTHERM by Jordan (1991), or CROCUS by Brun et al. (1989) and Brun et al. (1992)). However, due to the computational limitations associated to every spreadsheet-based model, such extensive and complex calculations are hardly realizable in case of the ESCIMO.spread model. To still overcome the deficiencies pointed out by Referee #2, we have implemented a pragmatic approach for estimating the snow temperature proposed by Walter et al. (2005). The method uses negative energy balance values to cool down a single layer snow pack. Although the assumption of a single layer snow pack represents a simplification of real conditions, we have achieved good model performance comparing simulated snow temperature to observations inside the forest canopy at site Vordersteinwald (Nash-Sutcliffe

Model Efficiency (NSME) = 0.57). By impementing this method for calculation of snow temperature into ESCIMO.spread (v2) we are able to close the energy balance for every model time step (even for non-melt conditions), derive melt conditions from the energy balance itself and remove the condition that snow melt is restricted to cases, where air temperature is above or equal the melting point of ice. We thank the referee for bringing this point into the discussion and are glad to have improved the model in this way. The masucript has been updated accordingly (see paragraph 1 on page 6 as well as section 2.3).

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2. Referee comment:

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If I understood correct there is a problem in the model design as described in Section 2.3: From the description of the concept and equation 7 it seems to me that there could be cases where the cold content of the snow is "saturated". If this is true, this would violate energy conservation in any energy balance model. More generally spoken, it seems that this conceptual parametrization cannot be implemented in an energy balance based model approach.

Answer by the authors:

We thank Referee #2 for this valuable comment. He correctly points out that in the conceptual cold content approach implemented in ESCIMO.spread (v2) cases might occur where energy deficits that should further cool down the snow layer might be neglected due to a maximum cold content defined in the model's parameter section. This maximum cold content of course states a violation of energy conservation even though it is suggested in the literature (e.g., Blöschl and Kirnbauer, 1991). We are glad to announce at this point that with the new approach implemented for the calculation of snow temperature (see answer to 1. Referee comment) the cold content of the snow pack can be directly infered from the snow temperature. The original conceptual approach has been removed in the updated version of the model with the manuscript updated accordingly (see section 2.3 (Eq. 6) of the updated manuscript).

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3. Referee comment:

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The implementation of available parametrizations is a good strategy to theoretically show the various impacts of trees on snow cover. I understand that the available measurements (one spot in the presumably very heterogenic canopy) do not allow evaluating the benefit of considering each of the processes individually. However, for the general model evaluation presented in the paper I have the following suggestions: All evaluation of inside canopy model results is based on quality criterions calculated between measurements and individual model results (like global radiation). In my opinion it would be necessary to calculate the increase in model skill when model results/measurements are adapted/not adapted for inside canopy conditions. For example, calculate the increase in skill when modeled inside canopy global radiation is used instead of outside canopy global radiation (similar for temperature, humidity, SWE, wind speed). This strategy would also avoid that high model skills (e.g. high R^2) can (partly) be a result of pronounced daily cycles in both measured and modeled variables (e.g. true for global radiation, temperature etc.).

Answer by the authors:

Referee #2 is right when stating that the efficiency criteria used for evaluating the performance of the canopy submodel (particularly R^2) are biased by daily cycles in many variables e.g. temperature or global radiation. Indeed, the approach proposed by Referee #2 of comparing the efficiency criteria resulting from oposing simulated and observed conditions in the canopy (as done in the submitted manuscript) to those achieved by directly comparing the observed conditions outside the canopy to those observed inside would allow to isolate the increase in model skill from application of the newly implemented canopy model. To put this suggestion into practice, we have calculated the predictive capabilities of outside-canopy observations for inside-canopy conditions for all affected variables (temperature, global radiation, relative humidity, wind speed and snow water equivalent) (see Table 2 in the updated manuscript) and have compared the resulting efficiency values to those achieved using the canopy submodel (see results section in the updated manuscript, last paragraph of page 18 – first paragraph of page 19 as well as first paragraph of page 20). Adding these analyses to our manuscript was an enormous improvement, we thank Referee #2 for this fruitful suggestion.

SPECIFIC COMMENTS:

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1. Referee comment:
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p. 8156, line 4: “a concept for cold and liquid water storage consideration” should be replaced by “a concept for cold content and liquid water storage consideration”

Answer by the authors:

Thanks, we have corrected the manuscript accordingly (see line 2-3, page 2 in the updated manuscript).

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2. Referee comment:
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p. 8156, line 14: “The validation results indicate a good overall model performance in and outside the forest canopy.” “good” could come along with objective quality criterions like RMSE (e.g. “The validation yields good/fair/... results with RMSE of ± xy RMSE [mm WE] / ± xy RMSE [mm WE] for outside / inside canopy conditions. Maybe the authors are also willing to consider this approach in Section 5.

Answer by the authors:

Referee #2 is right, statements like "... good overall performance ..." without providing the respective values of efficiency criteria only provides a subjective perspective on model performance. We have followed the reviewer's suggestion to include efficiency criteria in both the abstract and section 5 (as the Nash-Sutcliffe Model Efficiciency (NSME) is one of the most trusted and common

criteria for the quantitative evaluation of hydrological models, we have provided values for this criterion together with values of the RMSE as also proposed by referee #1).

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3. Referee comment:

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In the introduction (p. 8158, lines 5-10) the authors state that the model only requires few input data. Hourly input data of temperature, wind speed, relative humidity, global and longwave radiation are quite expensive in my point of view as this requires a nearby automatic weather station (always limited to one point) or demanding downscaling approaches when using atmospheric model data.

Answer by the authors:

Referee #2 correctly points out that with six required meteorological input variables (precipitation, temperature, wind speed, relative humidity, global radiation and longwave radiation) it might not be adequate to claim ESCIMO.spread requires only few input data. We have therefore rephrased the sentence to (see page 4, line 7 of the updated manuscript): "With hourly recordings of temperature, precipitation, wind speed, relative humidity, global as well as longwave radiation the model's demand on meteorological input is covered by those variables most commonly recorded at any state of the art automatic weather station". Thank you for pointing this out!

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4. Referee comment:

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In the introduction (p. 8158, lines 10-15) the authors state that the model "is even capable of simulating the evolution of a seasonal snow cover under climate change conditions" because temperature and/or precipitation trends can be applied. In my opinion this statement is very optimistic given the fact that (1) e.g. changes in precipitation very likely will also impact air humidity, radiation, temperature etc. and (2) the parametrizations for inside canopy conditions require many empirical parameters. Probably it would be more reliable to write something like "the model is able to calculate simple sensitivity tests for changed temperature/precipitation".

Answer by the authors:

Thank you very much, we have followed your suggestion and have modified the respective section in the manuscript accordingly (see page 4, line 17 in the updated manuscript).

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5. Referee comment:

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P. 8159, line 7: "calculation of the beneath-canopy snow energy and mass balance". If I understood correct (e.g. general comment 2), the model does not always calculate the snow energy balance as the energy balance is not closed in all cases.

Answer by the authors:

Referee #2 is right, in the submitted version of the model and the manuscript, the energy balance could not always be closed due to the design of the implemented cold content concept (see 2nd general comment and the respective answer by the authors). As we have implemented a new approach for the calculation of snow temperature in the updated model version we can now derive the cold content directly from snow temperature. By replacing the old conceptual method for the estimation of the cold content, which led to a violation of energy conservation, with this new approach, the energy balance can now be closed for all cases. Thanks for leading us into the right direction here.

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6. Referee comment:

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p. 8159, lines 5-10 “the new version ESCIMO.spread (v2) reaches beyond the capabilities of most other freely available point-scale snow models”: I’m not sure if this is true as there are meanwhile very sophisticated energy balance models freely available (e.g. http://regine.github.io/meltmodel/). In my opinion the strength of ESCIMO.spread (v2) is that it is very simple/low cost to use and it has extensions to consider inside canopy effects.

Answer by the authors:

Referee #2 correctly states that there are other snow models that share much of the functionality included in ESCIMO.spread (v2), however these models in most cases are not spreadsheet-based point-scale snow models. While the fact that we are refering to point-scale models was included in the submitted manuscript, we have added "spreadsheet-based" in the updated manuscript (see page 5, line 10 in the updated manuscript). Thank you very much for helping us to improve this part of the manuscript. Clearly, the fact that ESCIMO.spread (v2) accounts for canopy effects is one of its strengths, however no modifaction in the mansucript was neccessary here as the last three items in the list of the models key features already refer to this model strength emphasised by Referee #2 (see p. 8159, line 3 – 8 in the previously submitted manuscript).

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7. Referee comment:

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In 2.2 wet bulb temperature is used to separate solid from liquid precipitation which is definitely a reliable approach. Nevertheless, it would be interesting to see the relative differences (%) in calculated snow fall amount for one winter when applying the dry bulb instead of the wet bulb temperature. Thereby it seems important that the relative humidity measurements are bias corrected (nearly 100% RH should be reachable in case of very wet conditions). Furthermore, it could be an idea to interpolate from 100% solid to 100% liquid precipitation for a given range of wet bulb temperature (e.g. 0±0.5 degree) to avoid jumps in the calculated snow fall amounts

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Answer by the authors:

Referee #2 correctly states that it would be interesting to see the difference in solid precipitation when applying i) the air temperature-based approach for separation of liquid and solid precipiatation and ii) the wet-bulb temperature-based approach. We have done this comparison for site Vordersteinwald and the winter 2012/2013. We found out that using a wet-bulb temperature treshold of 273.16 K results in 43% less solid precipitation compared to using an air temperature treshold of 275.16 K (273.16 K wet-bulb temperature equals 273.16 K air temperature for air humidities around 70%, see figure 2 of the manuscript). We have chosen these tresholds as the wet-bulb temperature of 273.16 K is used in the current study as a default value and 275.16 K is a common air temperature treshold for precipitation phase detection and also the default value in the ESCIMO.spread model (v1) (see Strasser and Marke 2010). As this comparison strongly depends on the chosen threshold for both temperature criteria and the results are hence of little general validity, we have not included this information in the updated version of the manuscript. If the editors feel, this information would improve the manuscript, the authors are of course willing to add this information in the final version of the paper.
With respect to the relative humidity values we agree that the fact that 100% are never reached (maximum = 97 %) might be the result of biases in the recorded data. However, Pohl et al. (2014) have shown that the applied SnoMoS over the whole range of possible humidity values generally rather tend to overestimate than to underestimate actual air humidity. Hence, setting the maximum values in the time series to 100% (as often done to correct humidty time series) will result in the desired increase of maximum humidity values, but might also lead to increased overestimation in the mid-range values. We therefore think modifying these measurements would only bring little benefit coming along with increased overall uncertainty. Moreover, certain biases can also be expected in case of many other recorded variables (see Pohl et al. 2014) with a correction of all variables clearly reaching beyond the scope of this study. The decision not to modify the data is further supported by the fact that we checked in- and outside relative humidity to be similarly biased to maximum values of about 97%. Hence, we can exclude additional biases in the model results that might be induced by different accuracies of in- and outside measurements. To test the impact of applying a correction factor (that modifies all humidity data so that the maximum values reach 100%) on the simulated snow cover we have implemented a correction of observed outside-canopy humidity (observed inside-canopy humidity is not used by the model as humidity in the forest is derived from outside measurements) into the model. The results show only an insignificant impact on the model results with the Nash-Sutcliffe Model Efficiency (NSME) staying unchanged in case of outside-canopy SWE (NSME=0.71) and with NSME even slightly decreasing from 0.81 to 0.80 for inside-canopy SWE. If the editors nevertheless consider it important to correct the applied humidity measurements, the authors are willing to include a correction of measured humidity also in the submitted version of the model. In the updated version of the manuscript, potential biases in the measurements are shortly adressed in the conclusions (see line 6 on page 22 of the updated manuscript).
Referee #2 also suggests to include a certain temperature range where both liquid and solid precipitation exist to certain shares. We aggree that this approach might (under certain circumstances and at some sites) lead to better model results as sudden changes in precipitation phase are smoothend out. We have therefore extended the model with this functionality making this temperature range a user-defined parameter in the model's parameter section. As the application of the proposed value of +- 0.5 K has negatively affected the model results at site Vordersteinwald in a test run, we have not used a temperature range for precipitation phase detection in this study, but describe the new functionality in the updated version of the manuscript (see first paragraph of page 8 in the updated manuscript).

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8. Referee comment:

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In Section 2.3 (besides issue 2 in the general comments) it was challenging for me to think of a cold content expressed in units [- mm w.e.]. From my point of view a cold content in an educational tool would be better related to negative temperatures [degree C] of the snow pack (or certain layers) that can – together with the snow mass – be converted into energy content [J]. In a second step this would allow to calculate the energy [J] that is required to heat the snow pack up to the melting point temperature.

Answer by the authors:

Referee #2 proposes to change the unit of the cold content from [mm] to [°C]. From our point of view the favoured unit of the cold content strongly depends on the personal scientific background of the model user and could be [mm], [°C] or even [J] as all somehow describe the energy level of the snow pack - these units can also easily be converted from one unit to the other. From a snow modeller's perspective and also from an educational point of view, the unit [mm] also seems adequate as it is the amount of water that reduces melt for a certain time step due to the actual cold content of the snow pack. As we also agree that judging from the name "cold content" itself the unit [°C] does really make sense as well, we have extended the model so that the cold content now is provided in [mm] as well as in [°C]. We thank the reviewer for his suggestion, we think it really improved the model.

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9. Referee comment:

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Section 4 and Fig. 8: It looks like there is an obvious bias in the RH measurements as the values never reach (nearly) 100%. Please also do a bias check for the outside canopy RH measurements.

Answer by the authors:

See the authors' response to specific comment 7.

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10. Referee comment:

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In the results section the authors rarely comment on Fig. 10 (especially on the second pronounced snow pack in February 2013) which is essential as it shows the model capacity to reproduce outside canopy snow pack without any complications induced by the forest. If the model skill is higher inside the forest than outside (table 2) this could suggest that it is easier to model inside canopy conditions. However, I don't think that this is true but a result of (1) multiple error compensation effects (including errors in precipitation derived from a distant gauge) and (2) at least partly coincidence as there seems to be only 1 point measurement available in the canopy which does not represent the expectable strong spatial heterogeneity. The latter aspect is a serious issue for all the inside canopy evaluation and for future studies it would be desirable to do some small scale (e.g. within a couple of meters) cross section measurements of (at least) SWE inside the canopy.

Answer by the authors:

Referee #2 points out that the snow simulations outside the canopy as well as the arising differences between the model performance in- and outside the forest canopy should be discussed in more detail. We fully aggree with the referee's line of argumentation explaining the fact that the model results inside the canopy are better than outside at least partly through error compensation effects (including errors from precipitation measurement and the transfer of precipitation information from precipitation gauge Freudenstadt to site Vordersteinwald). We also aggree that it is surprising that a single SWE measurement in the forest can be this close to the simulations, given the expectable heterogenity of snow cover inside forests. To get deeper insights into the spatial snow cover heterogenity inside forests we have just recently installed multiple snow depth measurement units inside different forest sites in the Alpine catchment Brixenbachtal (Tyrol, Austria). The resulting data will be very valuable in future studies, as also pointed out by Referee #2. Following the referee's suggestion to extend the discussion on differences in the model results in- and outside the canopy we have updated the manuscript accordingly (see results section (page 20) and conclusions (first paragraph, page 22)). We thank the referee for pointing out this issue.

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11. Referee comment:
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p. 8172, lines 1-5: The lower R^2 for RH and wind speed might be a result of the weaker or missing daily cycles (see also general comment 3). Please also think again about potential offsets in the RH measurements (Fig. 8).

Answer by the authors:

We share Referee #2's perception that the lower values of R^2 might be a result of weaker or missing daily cycles in wind speed and have added this information to the updated version of the manuscript (see last paragraph page 18 - first paragraph page 19 as well as first paragraph of page 22 in the updated manuscript). With respect to the offset in humidity values please refer to the 7th general comment and the respective answer of the authors.

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12. Referee comment:
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p. 8174, line 23: I think "trend" should be replaced by "patterns".

Answer by the authors:

Thank you very much, we have followed you suggestion and have replaced "trend" by "patterns" in the updated version of the manuscript (see 2nd line of page 23 in the updated manuscript).

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13. Referee comment:

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Fig. 2 and 3 could be moved to an appendix to better focus on the results (Fig. 4 and Figs. 6-11).

Answer by the authors:

We thank Referee #2 for his suggestion, however we think that figure 2 and 3 are very valuable in the context of the implemented approach for wet-bulb temperature calculation. We therefore would like to leave them in the model description section and have not changed the manuscript with respect to the location of these figures.

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14. Referee comment:

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Fig. 2: "rain" and "snow" are a bit confusing in this plot. Maybe the authors could add a sentence from Section 2.2 ("Each of the displayed lines in Fig. 2 could be interpreted as a borderline to separate liquid and solid precipitation assuming a certain threshold wet-bulb temperature") in the legend instead. Please also consider again if it would make sense to implement a temperature range to gradually shift from 100% solid to 100% liquid precipitation.

Answer by the authors:

If we interpret the referee's comment correctly, Referee #2 would prefer the terms "liquid precipitation" and "solid precipiation" to "rain" and "snow" in Fig. 2. Moreover, he proposes to add text to the legend explaining the meaning of the shown lines. We have changed the terms "rain" and "snow" to "liquid precipiatation" and "solid precipitation" in an updated version of Fig. 2. As we think that an explanatory text would better fit into the caption of Fig. 2 than into the legend, we have updated the caption of Fig. 2 accordingly.
Referee #2 also suggests to include a certain temperature range in the model where both liquid and solid precipitation exist to certain shares. We have updated the model accordingly, for details please see the 7th general comment and the respective answer of the authors. We thank the referee for these valuable suggestions.

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15. Referee comment:

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Fig. 6: Maybe a scatter plot of the daytime values could better show the model skill. Currently, it is very hard to distinguish between the lines. Another idea could be to compare mean daily values.

Answer by the authors:

We see that it is hard to distinguish between the grey and black lines in Fig. 6 and have therefore updated all line plots with respect to the colors of the different time series. The colored lines are now much easier to distinguish. Moreover, we have followed Referee #2's suggestion to show the data in form of scatter plots (see figure 10 and 13 in the updated manuscript). This was also suggested by Referee #1. Thank you very much, these changes represent a big improvement of the manuscript.

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