Review of Pauze et al. : Improvements of a subcanopy snow model and its interception conveyed by observations from a mid-altitude alpine site

The study by Pauze et al. presents improvements to the MEB/Crocus forest snow model developed at Meteo-France, specifically relating to the treatment of snow interception and unloading. These model enhancements rely on an extensive forest snow process datasets comprising a wide range of variables acquired at the Col de Porte site in the French Alps, which allow independent evaluation of several forest snow processes. Besides model improvements, the data also allows testing of different top-of-canopy forcings. The presented amount (and quality of the work) is remarkable and I commend the authors for its comprehensiveness – the effort of getting to the bottom of each issue clearly comes through! Given the widespread use of Crocus in the community and prospective applications of MEB/Crocus, the relevance of such a study is unquestioned. The manuscript generally reads really well and is logically structured, and I believe this study will make a nice contribution to the literature following moderate revisions.

Major / general comments

Title: This might be personal taste, or I may be missing the point, but I find the formulation 'subcanopy snow model and its interception' suboptimal. Much of what you are doing applies to the canopy parametrization, not the subcanopy snow model, and as processes relating to interception are part of the canopy representation rather than the snow below it 'its interception' sounds a bit funny. Moreover, you don't only address interception but also unloading of intercepted snow. Something like 'coupled canopy-snow model and its representation/treatment of canopy snow (or intercepted snow)' could be a worthy alternative? Moreover, my understanding is that model development papers in GMD need to feature model name and version in the title, which is not currently the case.

Manuscript length: The combination of presenting new model developments and observational data that was only partially introduced in previous literature makes for a manuscript that is at the upper limit in terms of length, but for good reasons. Still, the present manuscript length motivated me to provide some suggestions for shortening or moving content to the Appendices (see line-to-line comments). In general, there are many sentences introducing the purpose of specific sections or paragraphs that I believe are not really needed, since the structure of the manuscript is logical and the section headings are pretty explicit. I would recommend the authors consider shortening these – but I recognize this is personal taste.

Figures: The figures are conceptually really nice and contain plenty of interesting information, but they could be even better with more consistent formatting in terms of figure and label sizes, and some of them (especially their y-axes…) are way too small given the amount of content. An example to illustrate what I mean: Figure 6 only occupies a third of the page width but labels are extremely hard to read, same for Figure 8 – could be at least twice as large; in comparison, Figure 10 is pretty huge relative to its content. Same applies to other figures… I think revisiting figure formatting is worth the effort to better them justice!

Impact of top-of-canopy forcing: I very much appreciated this aspect of the paper as it addresses a recurrent but poorly addressed issue in forest snow modelling. Two questions remained for me:

1. While wind speed is scaled assuming a logarithmic profile, I was wondering how measurement height of air temperature is treated by MEB – what kind of profile is assumed? Or, in other words, does the above-canopy temperature measurement matter?

2. As the choice of a wind scaling approach appears to have a major impact, I was surprised this result was not picked up in the discussion or conclusion. For future/other model applications, it would be interesting to have your thoughts on i) recommendations on how to choose a scaling approach/effective roughness length and ii) to what extent this result is specific to MEB/Crocus or transferrable to other forest snow models.

Use of LAI vs. LAIeff: This approach is interesting, and in my opinion, it addresses an issue that is not sufficiently acknowledged in forest snow modelling literature: the fact that canopy structure metrics themselves come with an uncertainty and biases may arise due to inconsistencies in the methods used to derive them at different sites. In theory, a local LAI should already incorporate the effect of tree height (for the same tree species, more leaf area per unit area if the tree is taller…); I would expect the boreal forest at which traditional interception parametrizations were derived to feature lower LAI than Col de Porte if trees are so much smaller; if the LAI values indicated for the two sites are too similar, the issue might be in the method used to estimate it rather than in the parameter LAI itself? This might be an interesting aspect to discuss, especially in the context of transferability of the study's findings (see also next comment).

Moreover, the use of LAIeff in the computation of other energy fluxes is mentioned, but not presented very clearly; Turbulent fluxes are mentioned without details on the impacted parametrizations, and it remains unclear whether radiative fluxes are affected. This aspect could be better detailed for the sake of completeness, for instance in an Appendix.

Distinction between solid and liquid discharge: Section 4.3 is very interesting and I fully agree with it. However, on top of considerations on the study's implications for future observations, I think some more discussion on implications for model choices and transferability would be appropriate. The high sensitivity of unloading to vegetation temperature is intrinsic to MEB, since a degree-day approach is being used to approximate canopy snow energy balance, but there are other potential drivers of rapid unloading (e.g. insolation, see e.g. Katsushima et al. 2023 https://doi.org/10.1002/hyp.14922) or small-scale spatial variability of canopy snow (e.g. preferential deposition/lateral snow redistribution driven by the unloading process, see e.g. Mazzotti et al. 2020 https://doi.org/10.1029/2019WR026129) whose impacts MEB can't 'see' since these processes are not implemented. Moreover, the impact of canopy dripping on the sub-canopy snowpack depends on how liquid water is routed through the snow scheme – a sophisticated snow model like Crocus may be highly sensitive to canopy dripping, while a one-layer energy balance model may see no impact at all.

Minor / line-by-line comments (I made many, but most are suggestions to improve language/readability and should hence be straightforward to implement. It was a pleasure to read a manuscript that hasn't been 'polished' by AI.)

L30: Discrepancies compared to what? Revisit wording

L45: Since you specify that MEB/Explicit Snow is intended for climate application, you may want to mention an intended application of MEB/Crocus as well

L59: ‘forest vegetation density spatial variability’ → change to ‘to the spatial variability in forest density’

L72-76: I would omit this content here and save the details for the methods section

L80: add ‘to date’ at the end of the sentence

L100: ‘an open field which has been taking measurements’ – is it really the field who takes the measurements? 😊 ‘field on which measurements have been taken’, rather?

L105: I guess you want to add surface pressure to the list, even if it’s of minor importance

L106: It’s unclear what you mean by ‘expertly adjusted’, please revisit wording

L108: Is the correction a constant factor? If yes, it wouldn’t harm to provide the value, if not, never mind.

L113: I would specify they have been used for validating Crocus – this makes a stronger case for also validating Crocus under forest at this site.

L122ff: Please revisit wording… it’s unclear whether you are mentioning two different data sources and using both of them, or whether the first is part of the second.

L125: Capitalize appendix

L144ff: The description of the setup is a bit abrupt for someone without previous knowledge about it. What boxes, why ‘new’? Are they weighted after every snowfall event? And are these measurements different to those used in Helbig et al. (2020)?

L152ff until end of section: Given the length of the manuscript and the level of detail of this section, I would move this content to Appendix D

L160: Careful, MEB/Crocus-new and -old have not yet been defined at this point.

L175: is ‘between’ correct here? Do you mean ‘including’ or ‘comprising’?

L183: non-covered → change to ‘not covered’ or ‘snow-free’

L186ff: What about water leaving the liquid reservoir in the presence of sub-canopy snow? It is also routed through the snowpack (as if it was rain?)

L188ff: This paragraph could be omitted (considering the length of the manuscript)

L216: The fraction of snow-covered CANOPY area? I would specify.

L222: I would specify that this is an unloading RATE

L226: ‘has been employed since a long time in MEB’: Do you mean it has been implemented in MEB and used prior to this study?

L235: ‘These poor results in SWE and snow depth lead us to modify’ → suggest modifying to ‘The poor model performance with respect to SWE and snow depth motivate us to modify’

L253: Observations from the boxes transect introduced earlier? Or from another source?

L265: Suggest removing first sentence, as it essentially repeats what you already stated at the start of the previous paragraph.

L272ff: 'Increases linearly between [...]' – the formulation is a bit misleading because it implies the linear increase as a function of temperature in the interval -3 to 0 degC, which is not what the equations suggest. I would reword to 'Is a linear function of solid precipitation, with the gradient determined by temperature as follows:' or similar. Moreover, there is a typo in one of the two equations (either >= or <= but not both otherwise the case Temp = -3degC is not constrained...)

L278: 'the maximum size for the solid reservoir' – is that the same as 'solid interception capacity'? Consider rewording?

L288: 'The vegetation temperature is used here' – based on the equation in MEB/Crocus-old, vegetation temperature was used there already, so it's confusing that this aspect is highlighted here – while it appears that the threshold temperature for unloading T_v^U was adapted? Please clarify.

L297: This is not a multiplication by 10 of the original value – I think what you mean is 'the coefficient is one order of magnitude larger than the original value' (sorry for being picky...)

L300: Period missing between the two sentences.

L305: 'Quite round form' → suggest changing to 'rounded shape'

L328: I assume this linear regression is fitted to the year with T32 data? I would suggest to provide the equation. Was T32 used directly for the year where measurements were available, or did you still use the parametrized data?

L337: It remains unclear what kwind is. Based on the presented units, I interpret it as a scaling factor to be applied to the wind measurement at 10m - but it would help to state this explicitly.

L352: T32 measure → should be 'measurement'

L354ff: This paragraph is very long given the limited impact of the different temperature forcings you show later, consider shortening.

Results sections: I found the introductory paragraphs of the results section a bit repetitive with respect to the methods. Since the titles are very explicit, they could be removed in the interest of shortening. But up to the authors, this is personal taste.

L396: The RMSE appears to be much larger on the figure, is this including periods without snow?

L405: 'forest wind'→ I recommend being more specific (wind measured within the forest for example)

L451: 'Net increase in canopy losses' → I think this should be canopy 'snow' losses? Otherwise, I don't see why the dripping would categorize as loss. As noted earlier, this sentence raises the question of how dripping from the canopy is routed through the snowpack.

Figure 6 is really interesting but almost illegible at this size, please increase, moreover it took me a while to figure out what modification each step corresponds to (e.g. 'scaling' I initially interpreted that as the modified meteorological input). The 'labels' of the different options could better link to the methods section.

L456: Why was only one year of interception measurements used?

Figure 7: In panel a, I am not fully sure what the cumul snowfall and cumul. rainfall represent – it appears redundant to panel c?

L470: Do you suspect an observation error, or what is your explanation for this discrepancy?

L547: Heading is the same as for previous section but I guess it should be 'Soil temperatures'?

L545: It is interesting that the discrepancies between the modelled and measured daily cycles vegetation temperatures do not translate in errors in subcanopy incoming longwave radiation, which would be expected. Do you have an explanation for this?

Figure 11: I would have plotted MEB/Crocus-old as well so that the sensitivity to the implemented changes at boreal sites can be visually compared to that at alpine sites. It's unclear to me what the lines Tv+2 and Tv+4 mean.

L619: 'Synthetic temperature' is unclear, did you want to highlight that it is a mixture of the three temperatures? Maybe 'integrates temperatures of vegetation, solid and liquid reservoir' is easier to understand?

L625: Haagmans et al. (2025 - 10.1016/j.agrformet.2025.110703) present data that could feed into this discussion.

L628ff: This answers my question on Figure 11, but I think it needs to be introduced at least in the Figure legend with reference to this section maybe? Or was there some confusion with Figure C1?

Figure 12: Obviously, one is left wondering if improvements are achieved especially for the years where performance of MEB/Crocus-new was rather poor. The averages in Figure C2 suggest so but I would have just included all years in this Figure. If performance is not consistent, this could point to processes that are not (yet) represented by the model, see my major comment

L673: 'and in intercomparison on dense alpine forest' → unclear what is meant here.

Figure B1: I love the idea of this figure, but I feel it would be easier to appreciate with less information. Suggestions: 1. The y axes need to be MUCH bigger; 2. Don't show all Z0f values, pick just ~3; 3. keep the snow depth area gray rather than changing colors, I spent a while wondering whether these were really fluxes… 4. Use the same x-axis for open and forest sites to facilitate comparison between the two.