Response to review #2

Snow model comparison to simulate snow depth evolution and sublimation at point scale in the semi-arid Andes of Chile

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Dear editor and reviewer,

First, we would like to thank the editor and the reviewer for their careful second evaluation of our work and the detailed suggestions and comments. Below we adress our detailed responses to all the comments.

As for the first round of reviews, in this response-to-review document we try to clarify and address each of the suggestions, comments and questions made during the review. Therefore we have copied the comments in blue boxes and have addressed them one by one. In the response we use italic fonts to quote text from the revised manuscript. Additional to the revised manuscript, we have uploaded a supplementary version of the manuscript with highlighted track changes that indicate where the manuscript has changed (red=removed; blue=added).

Yours sincerely, Annelies Voordendag & co-authors

Response to the Editor B. Noel

Dear Annelies Voordendag and co-authors,

We have now received comments from reviewer #3. The reviewer is generally satisfied with your edits and requests minor revisions. The authors should give particular attention to the reviewer's main comments, i.e., clarify which (set of) parameterizations are considered optimal/ideal, and elaborate on the difference between the SWE curves in Figs. 3c and 4c. The authors should also consider improving Fig. 3 (legend/caption) following the reviewer's suggestions. Besides other clarifications requested by the reviewer, the authors can find some additional editor comments/suggestions below.

Based on the above, the editor invites the authors to submit a revised manuscript. Note that the revised manuscript will be re-evaluated by the editor before acceptance for publication in TC.

Best wishes, Brice Noël

The authors thank the editor for his positive feedback and suggestions. As requested, in this revised version of the manuscript, we clarified the set of parameterizations chosen for the optimal case (please refer to the answers to main comments made by the reviewer). We also explained the differences in the SWE curves in figure 3 and 4 (see comment X).

Editor comments

As a general comment, the authors should rather use the term "bias" for comparison between models and observations, and "difference" between the two models. For instance, in L307 and L318, "bias" is preferred over "differences" when comparing models with observations.

We agree with the editor, but also note that we prefer to use differences if we compare different parametrizations within one model. L307 and L318 have been adjusted.

L90: Add a reference to Fig. 1b after "(AWS)".

Done.

L177: The sentence is unclear, notably the "resulting which would result in", could the authors reformulate?

The text now reads:
This cumulative SWE approach reduced the inclusion of deposition caused by snow drift which would have resulted in an overestimation of SWE.

L186-187: Do the authors mean "The accumulated daily precipitation (P) that agrees best with observed SWE and SD is further used in this study."? The term "P" is defined later in Table 3, but should be first defined here.

We have added "P_{cor}" to Fig. 3 and clarified the sentence:
The precipitation data set (P_{cor}) that leads to a simulation with best correspondence to the observed SWE and SD is used in the further study.

L246: Do the authors mean "1.6 times larger than the observed ... and the agreement between modelled and observed SD is better for SnowModel than for SNOWPACK ..."?

We clarified the text:
The assimilated precipitation is approx. 1.6 times larger than the observed precipitation and the agreement between modelled and observed SD is better for SNOWPACK than for SnowModel (i.e. (..)).

L262-262: Do the authors mean "Evaluation of simulated SD and SWE based on various parameterizations shows that both models are in good agreement with observations (Fig. 4), although they overestimate SWE at the beginning of the season."?

Yes, the text has been adjusted accordingly:
Evaluation of simulated SD and SWE based on various parameterizations shows that both models are in good agreement with observations (Fig. 4), although they overestimate SWE at the beginning of the season (May/June).

L321: Could the authors add the number of days after 30 November?

We cannot add the numbers of days after 30 November, as it was decided to run the model until 30 November. The decision was taken as the measured snow free surface date was already on 16 October and an additional 45 days period after this was assumed to be long enough for the simulations. If we extrapolate the data from Fig. 5b,d, it will be approx. 2/3 days until the red curve reaches 0 cm/mm w.e. but this is just an estimation. To avoid confusion the sentence has been re-written as follows: *(..); for SnowModel this date ranges between 20 August and 29 November (i.e. 101 days) and the range is similar but a bit later in the season for SNOWPACK (i.e. between 29 August and early December).*

L413-414: The sentence is unclear, do the authors mean that similar conclusions can be drawn for both Alpine and semi-arid environments, i.e., the choice of model structure and parameterizations has a major impact on the performance? Please, reformulate.

Yes, we therefore reformulated the text:
Finally, Rutter et al. (2009) pointed out that no universal 'best' model exists and model performance directly depends on calibration of the models to the specific study site. Here similar conclusions can be drawn for both Alpine and semi-arid environments, namely that the choice of model structure and parameterizations, along with a specific calibration of the parametrizations for the study site, has a major impact on the performance.

Editor's suggestions

L35: to remove "often".

Done.

L48: “impact of” instead of “consequence of”.

Done.

L68: “based on” instead of “in function of”.

Done.

L82: “falls as snow” instead of “arrives as snow”.

Done.

L107: “Therefore, TA and RH data were . . . ”.

Done.

L146: “Finally, the three-dimensional model SnowTran3D (Liston and Sturm, 1998), which simulates snow erosion and deposition, is not activated in this study; . . . ”.

Done.

L221: “perturbed” instead of “disturbed”.

Done.

L255: “markedly increases”.

Done.

L275: “decreases” instead of “reduces”.

Done.

L279: to split the sentence in two “after August. This is likely caused by higher . . . or by a bias . . . ”.

Done.

L290: “12 May, i.e., the first snowfall event, while . . . ”.

Done. The text now reads:
The mean SD difference between the parameterizations is 20 cm (i.e. 18% of the total SD), with a maximum of 152 cm at the first snowfall event (12 May), while (..)

L302: “TA being above the freezing point . . . October, resulting in fast melt simulated . . . ”.

Done.

L402: “is larger than that . . . SnowModel, which is directly related to . . . ”.

Done.

L431-434: “Therefore, we chose a precipitation correction that overestimates snowfall at the start of the season, but does not capture the increase of SD and SWE in mid-June, resulting in a good agreement between simulated and observed SWE from the beginning of July. . . ”.

Done.

L472-473: “assimilated data, which were required for model evaluation.”.

The text now reads:
The final correction of the precipitation data was done with an equation based on TA and WS, as it was unwanted to adjust the precipitation with SNOWPACK’s assimilated data, as this assimilated data is built from data, which

were required for model evaluation.

L481: "from 36% up to 86% of total ablation.".

Done.

L483: "despite" instead of "even though".

Done.

L489-491: "Our study covers the winter season of 2017, and our conclusions on model sensitivity to various parameterizations are specific to that period. ... distinct places to complement our results. Furthermore, additional models could be used, in particular snow models with similar ..."

Done:
Our study covers the winter season of 2017, and our conclusions on model sensitivity to various parameterizations are specific to that period. In further studies, simulations could be performed over a larger time period, and at distinct places to complement our results. Furthermore, additional models could be used, in particular snow models with similar physical complexity.

Figure S2.1 in L5 of the caption: "represents" instead of "coincides for".

We clarified the sentence:
PSWE is equal for z_0 is 1 mm and 1 cm and thus only the red line is visible.

Figure S3.1: mention in the caption what K and TI mean.

The caption now reads:
Observed cumulative precipitation, PSWE and precipitation corrections (MacDonald and Pomeroy, 2007; Smith, 2007; Wolff et al., 2015). The two SWE observations with potassium (K) and thallium (Tl) gamma rays are also given.

The editor noted the following typos:

- L9: "directly".
- L90: "unshielded".
- L138: "MicroMet is a ...".
- L190: "sensitivity tests".
- L236: "z0 of 1 cm and 1 mm are displayed".
- L299: "Switzerland".
- L365: "one of the most".
- Table 2: "1 default option and 2 from SNOWPACK".
- Fig 5: For consistency, "SNOWPACK" instead of "SnowPack" in the caption.

All typos have been corrected.

Response to the anonymous reviewer #3

Main comments

The authors provided a thoroughly revised manuscript, with most issues properly addressed. There are a few remaining issues though, which I think the authors should take into consideration before the manuscript can

be accepted for publication. Please find those below.

We would like to thank the reviewer for his/her second evaluation of our work, the valuable feedback and the positive comments.

The most important issues are with the new section "Idealised simulations". First, I'm not sure if idealised is the best term, since it mainly concerns the reconstruction of precipitation from SD and/or SWE. So maybe the title should reflect that. The main confusing part for me here is what is considered optimal or idealised. In Fig. 4, the thick line indicates the most optimal settings. I noticed in that figure for the SNOWPACK results that the drop in SWE towards the end of September in Fig. 4c is much smaller than in Fig. 3c. So there are clearly other settings being used, and it is unclear to me how the results can be so different when the manuscript suggests that both figures are created with a kind of optimal/best settings. It has an impact on the precipitation reconstruction from snow depth, since SNOWPACK will add precipitation to match the observed snow depth after this drop that occurred at the end of September.

The term "idealised" was chosen as the most "perfect" set of input variables was used as input. This includes indeed a precipitation assimilation, which has a significant (and main) effect on the simulated snow depth and SWE. But it also includes other important variables such as the observed albedo (for SNOWPACK). Indeed, this is the cause of the bigger drop of the SWE (and SD) at the end of September in Fig. 4. In Fig. 4 only the $S_{\downarrow}$ was used and the albedo is simulated with the available parametrizations in SNOWPACK. During field observations we have seen that the patchy snow areas sometimes get covered with dust, probably causing the drop in observed albedo and thus the faster melt. This can, of course, not be modelled with the implemented parametrizations. The term "idealised" is therefore chosen and corresponds to all the "best" input data possible. However, first, all this data are not always available and second, it is not possible to use SD or SWE and observed albedo as validation data, if it is also used as input.

We therefore consider and 'optimal' case, allowing to (i) represent the set up as more ususally used (i.e. Not assimimation alsedo and P), and evaluated with the combination of an albedo and snow density parametrization agree the most with the observed SWE and SD.

We agree that this can be confusing in the manuscript. Therefore, to clarify this point:

- we have added the legend title "Input variables" to Fig. 3 to show which variables were used as input. These simulations resulted in the data with assimilated precipitation (Fig. 3e,f) and we have adjusted the caption.

- the term "optimal precipitation" has been replaced by "corrected precipitation" in Sect. 3.2.2 (line 168) to avoid confusion.

- a sentence at the end of Sect. 3.2.2 has been added to better explain this idealised case: *This idealised case corresponds therefore to simulations using the best possible combination of input data. As such observations are not always available or used to evaluate models, the idealised simulations are not used for the sensitivity study and model comparisons, which are based on optimal simulations (i.e. without assimilating precipitation and albedo, see Sect. 3.2.3). The simulated SWE and SD are compared to the observed SWE and SD and the assimilated precipitation data sets are shown.The precipitation data set (P_{cor}) that leads to a simulation with best correspondence to the observed SWE and SD is used in the further study.*

Minor comments regarding Section 4.1

L237: If possible, please provide some details of the crashes (temperature related, model bugs?)

We did not investigate the crashes any further, but the crashes are likely related to out-of-bound temperatures. We have added to the text:
The reason for these crashes has not been further investigated.

L249: It looks like that there was a high wind speed event mid-July. That certainly could have resulted in some erosion in reality, which is not simulated by the models. I think that that is as much an explanation as the overestimation of PSWE at the end of June.

We agree that a similar process occured, but also note that even if snow erosion was activated in SNOWPACK, this erosion was not simulated. We added in this section:
Likewise, high wind speeds and a strong SWE and SD decrease is observed mid-July, which is likely snow erosion and also not considered in our simulations.

L251-252: As I mentioned earlier, it looks like melt is overestimated (since there is a lot of runoff simulated around this time when looking at Fig. 6c,d). So then the models need to simulate precipitation to bring the SWE/SD back to observational levels.

We agree with your comment. Indeed, melt is very strong in September. It is likely due to a low albedo not simulated by the model (see Fig. 4e and f). It might be attributed to a snow covered by dust transport by some strong wind events at the end of September, often observed in the field. The presence of dust, by darkening the snow surface, lead to a decrease in the albedo and an accelerating melt, which is not considered in these simulations. To compensate this effect, models try to readjust the precipitation amount, in other words, the models try to compensate processes not considered in this study. This is clarified in the manuscript:
This is related to a strong melt in September, not simulated by the model, along with models trying to get the SWE and SD to the observational levels. The strong melt in September is caused by a sudden decrease of the albedo (observations in Fig. **??***), as it is likely that the snow got covered with dust after some days with strong wind at the end of September, but the simulations overestimate the melt caused by this albedo decrease.*

I struggle to understand the legend in Fig. 3, particularly when it comes to what the blue line represents, since it includes both "uncorrected precipitation" and SD/SWE, where I assume it's actually corrected precipitation by using the model's ability to assimilate SD/SWE?? I think at least the figure caption should be improved here. The figure caption should also explain why SnowModel has multiple blue and red lines.

The blue line shows the assimilated precipitation, if the observed precipitation and SD is given as input. Along with adding the title "Input variables" to the legends and changing "Uncorrected precipitation" into "Observed precipitation" in Fig. 3, we changed the caption of the Fig. 3:
a-b) SD, c-d) SWE and e-f) the cumulative assimilated precipitation for the simulations with SNOWPACK (a,c,e) and SnowModel (b,d,f) and observations (black). The different input variables are given in the legenda. The dotted line of SWE indicates the less reliable (lower) SWE measurement from thallium rays (See Sect. 2.3) and the dotted line in e-f) is P_{cor}. The models have assimilated the observed precipitation (black) to the output (red/blue) given in e-f). Only one red and one blue line is shown for SNOWPACK as the other eight simulations crashed. The simulations for $z_0 = 1\,mm$ are found in Sect. S2.

I strongly recommend to add the undercatch correction from Wolff et al. (2015) in Panels 3e and 4f.

We think the reviewer meant to say Fig. 3e and 3f, as 4f is a figure with albedo, but we have added P_{cor} to Fig. 3e and 3f and clarified this in the caption of the figure.

Minor comments

L3/4: "While many studies focus on evaluating these uncertainties, issues still arise, especially in environments where sublimation is the main ablation process." "issues still arise" is very vague language, please improve this sentence with some concrete examples.

We clarified the text:
While many studies focus on evaluating these uncertainties, no snow model comparison has been done in environments where sublimation is the main ablation process.

L60-65: Instead of only listing those studies, please provide details of what those studies found (particularly the results relevant to this study).

The studies in this section are only mentioned to show that an accurate assessment of different snow models' sensitivity to parameterization choice or input forcing is currently missing, although it is expected to have a large impact. Furthermore, some of the mentioned studies (Gascoin et al., 2013; Réveillet et al., 2020) are elaborated in the discussion.

L90: unshielded

Done.

L127: "includes *the* MeteoIO preprocessing library"

Done.

L143: "new snow depth" This is somewhat confusing, since new snow depth can also be interpreted as the increase in depth from precipitation. Maybe "remaining snow depth"?

The text now reads:
In SnowModel, the melted snow is redistributed over the remaining snow depth up to a maximum density threshold of 550 kg m^{-3}.

L157: "models were calibrated". The examples given indicate that it is not a calibration. If the soil albedo is set to 0.15 based on measured albedo, I would not consider that a calibration. I interpret calibration as trying different values and see what matches SWE or SD best.

We adjusted the text:
Initially, both models were set up using similar parameters to facilitate intercomparison.

L256: overestimation of what exactly?

The overestimation of SWE:
The overestimation of and the need for SWE as validation data are indications that the PSWE is not a valid precipitation dataset for our simulations, (..)

Fig 3: the x-tics in (a) do not align with the x-tics in the other subfigures. Please correct, since the figure suggests a common x-axis.

Done.

L275: I actually struggle to see this in Fig. 4a,g.

We agree that it is hard to see the compaction in Fig. 4g. We think this is better visible in Fig. 4a, as the thicker red line in this figure has a flat slope, whereas the other runs show bigger increases during snowfall and faster decreases afterwards. We have added an explanation to help the reader:
(i.e. the bold red line has a moderate slope, compared to the light red lines in Fig. **??***a, until July)*

L308: "e.q." -> "e.g."

Done.

L328: Since for SnowModel, you talk about clusters, I think it's appropriate to talk about clusters here too. Instead of using the language of "range between 1.41 and 2.96", since it's not really a range. When I interpret it correcly, SNOWPACK produces two clusters in sublimation rate, based on roughness length.

Indeed, for SNOWPACK it was also clustered based on the z_0. The text has been adjusted:
For SNOWPACK, the spread of the averaged sublimation rates corresponding to the ensemble runs from the first day of snow to 20 September have a minimum of 1.41 and a maximum of 2.96 mm w.e. d^{-1} *(Fig. 6a). During the cold period, when no melt occurs, the sublimation amounts mainly depend on the* z_0*, with sublimation rates ranging between 1.40 and 3.18 mm w.e.* d^{-1}*, but this is mainly clustered according to the implemented* z_0*. At the end of the season, the total sublimation ranges between 153 and 364 mm w.e. (corresponding to 36.2 to 86.0% of the total ablation, again stronly depending on the* z_0*).*

L339: "Z0" -> "z0"

Done.

L365: Since the main objective of the study seems to be to quantify sublimation, and sublimation is most strongly impacted by roughness length (see the dicussion on the clusters found in model results), I think that should be discussed before albedo.

The topics in the discussion have been reordered. The turbulent fluxes are discussed first, followed by the snow settling and albedo.

Throughout manuscript: instead of talking about with and without precipitation uncertainties, I suggest writing including and excluding precipitation uncertainties. I think that makes it more clear.

This has been adjusted throughout the manuscript and also been changed in Fig. 5.

L356: "such as grain size and snow surface area". It's actually called "specific surface area", but please note that SNOWPACK currently does not consider SSA specifically, since it's microstructure model is constructed based on totally different parameters. I suggest writing "grain size and microstructure".

The text now reads:
Also, SNOWPACK considers a more complex representation of snow physics, such as the grain size and microstructure, (..)

L379: "as observations are biased". It's bit vague which observations are meant here. I assume authors mean that deriving density from SWE/SD measurements is biased over direct density observations using manual measurements?

It seems that this statement was too vague. We therefore considered you comment in clarifying this statement as follows:
(..), but an improvement of snow density parameterizations in semi-arid regions shows a demand for snow density measurements, as deriving density from SWE/SD measurements is biased over direct density observations using manual measurements (Smith et al., 2017).

L495: Please update the SNOWPACK repository link. It seems to have changed recently.

Thank you for pointing out this. This has been updated in the revised version:
SNOWPACK is an Open Source model and can be accessed at https://gitlabext.wsl.ch/snow-models/snowpack.

References

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