Dear Editor, dear Reviewers,

We first want to thank all of you for the very constructive comments that you provided following your reading of the manuscript (they testify for careful examination and insightful knowledge of the field), and for the enhanced time that you allocated us to submit this response: it was really welcome and we thank you for your understanding.

To help the further review process we also submit a corrected version of the manuscript where changes are highlighted.

Point-by-point response to comments by Referee # 1:

We are deeply grateful to reviewer 1 for its thorough examination of the paper and sharp, helpful comments that encompass both the wide areas of… physics and grammar!

Response to « General Remarks »

(i) About « sharpening the focus » and « concentrating on innovations »

We completely understand your feeling. Structuring the paper in its current form was actually a long debated choice: should we prefer a sharp and short paper just focused on the added-value of the 2-layer model? Or a more comprehensive one, that would also include a never-published (and hard to dig out) exhaustive documentation of the canopy module, that would help further generations of PhD students and postdoc understand the module in its present state and build on it with loads of innovative, great ideas ? We obviously chose the second option; the choice of GMD naturally ensued, as the journal fits that purpose.
For the revised version, we stand to that choice.
However, your comment means a lot to us as the feedback of a very advanced reader. You convinced us that clear efforts had to be made to make the reader aware of the particular structure of the paper, and help him better find his way to his key interests (model documentation, or state-of-the-art developments based on physics and applied on field data).
We therefore performed the following changes (that are highlighted in the corrected version attached to this Response) :

- Clearly state our documentation purpose in the Asbtract. (« *As a by-product of these new developments, an exhaustive description of the canopy module of the SNOWPACK model is provided, thereby filling a gap in the existing literature.")*
- We thoroughly revised our introduction and among others, insisted on the paper's structure :

"Our contribution is hence structured in the following way:

1. an exhaustive documentation of the new model and the canopy module it is embedded in, is proposed, for the sake of clarity and knowledge dissemination. Earlier versions of the canopy module had been only partially described in Stähli et al. (2006) and in appendix A of Musselmann et al. (2012).

2. existing simultaneous observations of sub-canopy radiation, snow evolution and meteorological conditions from Alptal (Switzerland) are used to validate the new model and demonstrate its robustness and improvement over simpler canopy formulations and with respect to observations.

3. model validity and transferability is finally tested against observations of components of the canopy energy balance taken from a different coniferous environment (Norunda, Sweden)."

(ii) The methods applied are appropriate and valid. However, concerning the related work, some additions should be included. Balance of what is presented and discussed can be improved, too.

The Discussion was enhanced, especially with respect to the role of longwave radiation in the spring melt dynamics below forests. The discussion about the mass-balance and the relative importance of processes affecting it was ill-placed in the previous version and now belongs to the Discussion too.

We feel that the first part of your concern (« additions should be included ») has to do with the possibility of including additional simulations and comparisons to observation, as you suggest later in your detailed remarks.
We followed your advice and tested the new canopy model at BERMS and Fraser, two sites from the SnowMIP2 project.

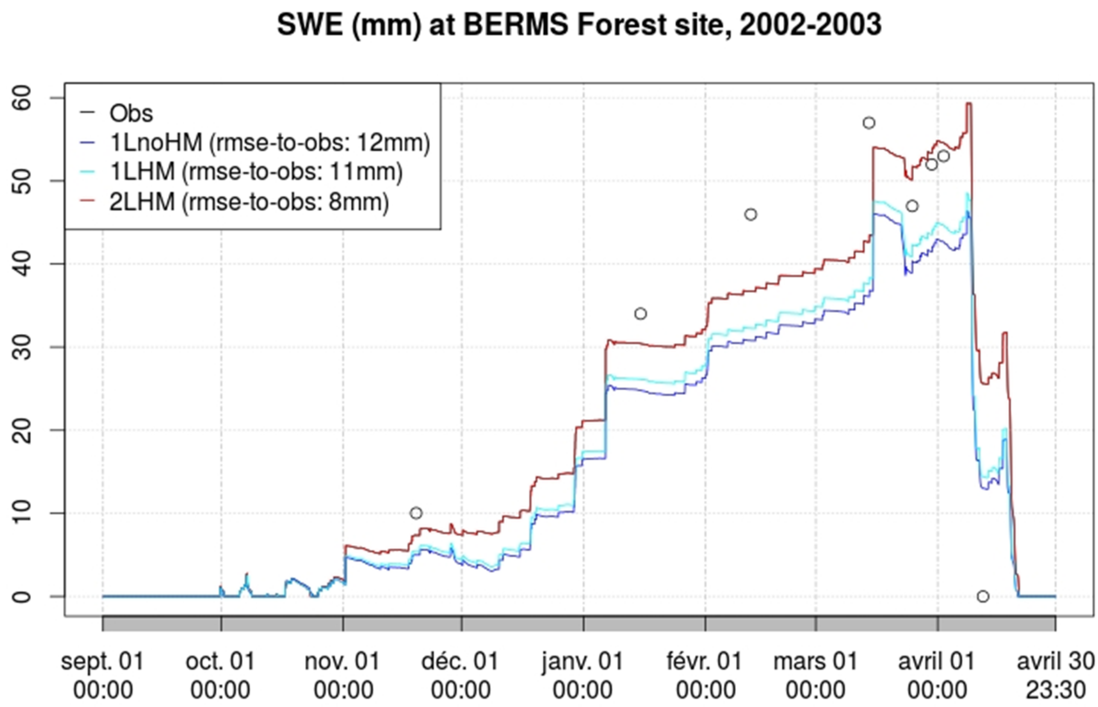

Fig1. SWE observations and simulations at the BERMS site.

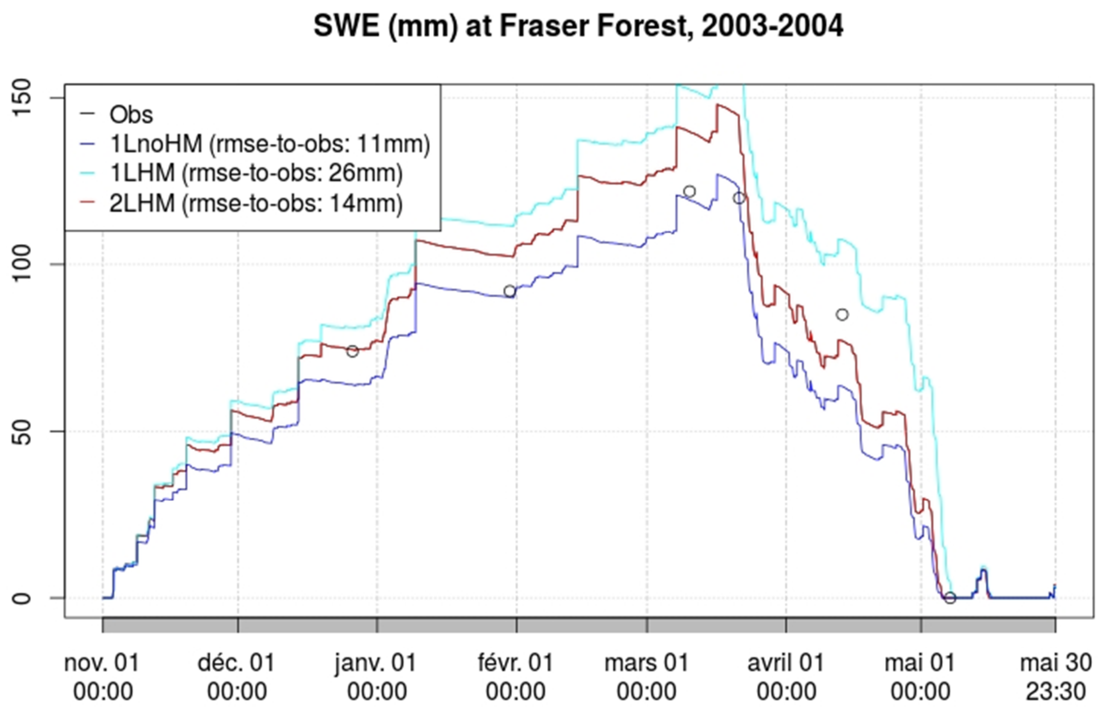

Fig 2. SWE observations and simulations at the Fraser site.

For these simulations we blindly used the forest and site parameters from the SnowMIP instructions and did not calibrate the model.
These simulations confirm that the new canopy module does not deteriorate the model's ability with respect to SWE modelling, and sometimes improve it (like at BERMS).
However, none of the authors of this study is truly familiar with these sites, and we feel that this is detrimental to a proper analysis of these results. At the local Alptal site, we could use ancillary, stand-scale radiation data to calibrate some model parameters properly (like the shortwave extinction coefficient) and evaluate other aspects of the model in the best possible setup with respect to the variety of available data. Furthermore, we relied on local knowledge for improved precipitation and radiation inputs to the model.
None of this was easily feasible for the BERMS or Fraser sites.
From Rutter et al. (2009), we trust that the harsh snow-rain threshold at 2°C at Fraser limits the reliability of the precipitation fields, and hence the strength of the conclusions that could be drawn from Fig. 2. The 2LHM model outperforms other model versions at BERMS, which we are happy about, but the simulation exhibits inconsistencies in both the accumulation and ablation phases, and it is difficult to assess whether the model performance are induced by the improved parameterization or randomly by better error compensations, for interception deficiencies for instance.
We therefore chose not to elaborate on this in the corrected version of the manuscript, because an analysis as detailed as what we did at Alptal would require much data digging and site knowledge that we are currently able to put together. The use of the SnowMIP sites combined with local knowledge is though mentioned in the Discussion as a rich perspective.

(iii) We elaborated about the structural choices made for the manuscript as an answer to (i) ; efforts were also made to sharpen the focus and make it easier to navigate through the structure depending on the reader's interests. We hope that this fulfills your demand.

(iv) We took your helpful suggestions into account and also rephrased some ambiguous paragraphs. Many thanks for this detailed help !

<u>Response to « Remarks in Detail »</u>

(i) None needed

- about missing references : your bibliography suggestions have been taken into account and the discussion of the SnowMIP2 case study has also been enhanced with a view of sharpening the precise focus of our paper, among other possible issues like mass-balance.
- About the coniferous vs deciduous, summer vs winter (snow) processes, mass balance vs energy balance:

- **Coniferous vs deciduous :**

The Reviewer is right in pointing out that the domain of validity of the model has not been specified properly. Our new canopy module is indeed in its current form designed for **needleleaf, evergreen** forest. We strove to explain this in the manuscript:

- The paper's title is now: « A two-layer canopy model with thermal inertia for an improved snowpack energy-balance below <u>needleleaf</u> forest".

- We now specify in the abstract and in the text that our modelling is up to now only valid for <u>needleleaf, evergreen</u> forests.

- It is stated in the discussion that « *The two-layer formulation furthermore builds a suitable basis for a future model adaptation to deciduous forest environments*"

- **Summer vs winter (snow) processes :**

The SNOWPACK model was originally designed for snow applications. However, as an energy-balance model featuring a conservative soil scheme with water balance and heat diffusion, it is equipped with all the necessary elements to be used as a Soil-Vegetation-Atmosphere-Transfer model. This precise quality made it possible to use summer data like the ones collected at Norunda for a more thourough validation of the model : indeed, we couldn't find any record of tree temperature measurements performed in winter environments that would be as long (3 months) and as exhaustive as the Norunda data. The latter include the monitoring of tree temperature and air temperature at different heights, and a careful calculation of biomass and biomass+air heat fluxes based on measurements and theoretical considerations. In comparison, the tree temperature and long-wave irradiance data by Pomeroy et al. (2009) only cover two 3-to-4 days periods.

<u>The suitability of SNOWPACK for use in summer context is now specified in Section 4.2 in the revised manuscript version.</u>

- **Mass balance vs energy balance**

As the Reviewer mentions, there has been a lot of modelling effort dedicated to the representation of snow in beneath-canopy environments. Lots of them (like Pomeroy et al., 1998) investigated relevant ways to parameterize the interception of solid precipitation and the sublimation loss of the intercepted snow, because they are first-order matters for the snow mass balance and subsequent hydrological applications.

However, at the time when the present study took place, we didn't dispose of new data that would enable to make an important breakthrough in that delicate matter. We therefore chose to revisit data collected earlier (mostly during the SnowMIP2 campaign), part of which could benefit from dedicated additional analyses.

We agree that the model refinements we performed and the model improvement that we diagnosed, are of second order for the snow mass balance when compared to interception issues. We however think that gradually improving model features will, with time, reduce the number of parameters likely to be flawed during a model experiment, so that more relevant conclusions can be drawn as to other critical model parameters like the ones governing interception. In the end, model accuracy should be increased, too.

We try to make the motivations and the philosophy underlying our study more obvious in the introduction and discussion of the revised manuscript.

Since the beginning of our investigations, work has been carried out at SLF-Davos with respect to the parameterization of interception. The first conclusions have just been published this month by Moeser et al.: they bring up new material that will help modellers better constrain interception issues and hence consolidate the model on aspects complementary to the ones we improved. This is now mentionned in the discussion too.

- **« No explicit simulation of snow melt or snow densification in the canopy is included in the model" (p217, ls18-20): why? At least the former is very important, and literature provides ways to do so. You should clearly figure out the effect of the necessary simplifications in the parameterization of Your model contrasting with its physical orientation, mainly cf and the unloading coefficient. »**

We are sorry for the misleading description that made you so critical about our model and dubious as to its physical consistency. Melt of intercepted snow actually occurs as soon as air temperature reaches the snow/rain threshold: intercepted snow then unloads as liquid water, and part of if remains on the leaves as « intercepted liquid water ». The density of intercepted snow also evolves following the conditions of the air surrounding, as described in Lehning et al., 2002a. We reckon that this is not a proper densification process. But because unloading occurs as an excess of the interception capacity (which decreases when the density of intercepted snow increases), warm and humid conditions can naturally trigger snow unloading before the snow-rain threshold in temperature is reached. These processes circumvent some of the limitations that you mentioned.
We modified the manuscript to clarify these processes, as a feedback on your remark.

- **About efficiency criteria:** we thank the reviewer for the enlightening paper by Krause et al. It underlines pretty well the diversity of criterion among which hydrologists can choose or be lost, and their respective strength and weaknesses. Our position in the current paper was to keep things as simple as possible while enabling the reader to build his judgment on the basis of fair and rigorous information.

- For the model calibration and evaluation at Alptal we chose a combined criterion based on mean bias MB and root mean square error RMSE :
  - MB is crucial to modellers because it can help detect flawed energy or mass-balances, and most more sophisticated efficiency metrics (like Nash Sutcliff Efficiency NSE) are poorly sensitive to such flaws (as underlined among others by Krause et al. 2005).
  - We insist that although RMSE does not specifically pinpoint errors in synchronicity between signals, it is still sensitive to them, while accounting in a great way for errors in magnitude.
  - Additionally, we confess that we also performed our calibration/validation experiments at Alptal using NSE criteria for $LW\downarrow_{BC}$ and $SW\downarrow_{BC}$: this resulted in the selection of the same model parameters than the calibration based on our combined criteria, while the difference in numerical values between the different model versions was less striking and the main effects brought by the new model versions (reduction in LW bias) was harder to identify.
  - Absolute errors (in radiation budgets for our case) matter more to us than relative ones du to their impact on the energy balance, hence no use of relative efficiency index in our study.
- For the model evaluation against biomass heat fluxes at Norunda, we kept the same line of simplicity and fairness in the evaluation metrics provided to the reader. With respect to the metrics provided at Alptal, we provided the correlation coefficient (corr) because the RMSE errors were on the order of magnitude of the model standard deviation, questioning more directly the appropriate periodicity of the signal. The use of « corr » provided insight into that. Furthermore, model evaluation against the combined biomass+air heat flux lead to a desirable improvement in the model-to-data synchronicity, which « corr » also illustrates.

We consider that the mention of the MB, RMSE and corr provides the reader with quite complete and complementary information as to the model quality, equivalent to intercept (linked to MB), slope (indicating goodness in magnitude like RMSE) and R2 (linked to corr) in a model-vs-data scatterplot illustration.
For more clarity we also specified in Table 5 that the mean modelled and observed biomass fluxes are null over a period between two equal thermal states.

(ii) Thank you for your detailed advice for the improvement of the English of the manuscript. We considered most of them, with the few following exceptions :

- « Physics-based » and « physically based » seem equivalent to us, and « physics-based » is currently in use in diverse papers even in high-rank journals (for instance : http://www.nature.com/nature/journal/v431/n7012/abs/nature02995.html)
- The use of Figure and Fig. within a GMDD manuscript is part of the editing rules.

Some questions were also raised in these « Remarks in Details », to which we provide the following answers:

1 .« P222 ls12-22: a bit unclear: do You mean the shadow of the trees at the edge of the forest? »
No. We reformulated our sentence as follows in the manuscript and hope that this is clearer now.

"An exception to that occurs for direct shortwave radiation which is collimated in the solar direction: when sun is not at the zenith, the sun beams are not parallel to the tree trunks and the projected surface occupied by the canopy along their trajectory is higher than (1-c_f)."

2. Ps223 onwards (mainly 2.4.2 and 2.4.3): You should point out clearer what is computed in summer for the atmosphere-canopy-soil continuum, and what in winter when snow is intercepted and/or on the ground. Does the model run continuously all through the seasons and close the water balance? Yes and yes**;** see answer to (ii) and related modifications in the manuscript.

3. **P241 ls12-19: does this explain why the model reacts faster? According to Your explanation it could be expected that the model peaks later but higher? Please reconsider.**

In our interpretation, it explains why the model reacts faster (i.e. earlier) and also higher. We now detail both mechanism in the manuscript (also copied hereafter), and we were careful to rephrase our original explanation for the sake of clarity. We hope that you understand our interpretation, and welcome any other interpretation that you could suggest or reason why you would disagree.
"We interpret this as an artefact of modelling the canopy with only one or two thermally homogeneous layers, whereas it is in reality a continuous medium experiencing thermal diffusion at scales smaller than our layers. In reality, the low thermal inertia of a bark surface layer provokes quick surface heating as a result of solar energy input (e.g. in the morning). This temporarily limits further heating from turbulent and radiative fluxes, until the surface heat has diffused into the trunk. Contrarily, the bulk, thermally inert trunk layer of our model heats up to a smaller temperature because the heat flux is accommodated by the whole layer and not only by its uppermost surface: further heating by turbulent and radiative fluxes is then still possible and the heat flux towards the biomass keeps being sustained and homogeneously distributed in the layer. As a result, our modelled canopy accommodates incoming energy more rapidly than a real one during the first part of the diurnal cycle. The aforementioned mechanism can also cause the accommodation of more heat energy by the modeled trunk layer than in reality: in reality, the capacity of the canopy to accommodate heat is somewhat limited by thermal diffusion, and heat uptake stops when available solar energy starts going down. At that time, the wooden medium has usually not reached an homogeneous (high) temperature yet (e.g. Fig 1 from Lindroth et al., 2010)."

Point-by-point response to comments by Referee # 2:

Thank you for your kind encouragements and enthusiastic welcome of our work!

1. As a response to your Point 1, please consider Fig 3 and 4 of this response, displaying the model-to-data RMSE (Root Mean Square Error) in SWE (Snow Water Equivalent) as a function of *laifrac* (fraction of LAI attributed to the leafy layer, f_{LAI} in the manuscript model description) and *krnt* (short-waves extinction coefficient of the whole canopy, k_{LAI} in the manuscript model description). These results are obtained using the 2-layer canopy model. We focused on snow-years 2004-2005 and snow-years 2006-2007, because there is high suspicion of flawed precipitation data in the other years with available data at Alptal, as stated in the manuscript. For comparison, the model-to-data RMSE obtained with the other one-layered model versions are given in the caption.

   These figures illustrate that there is quite a wide domain where parameters *laifrac* and *krnt* lead to a RMSE with respect to snow observations as low as possible, and that the chosen *laifrac* and *krnt* values (as a result of calibration against radiation data) are within this domain (for 2006-2007) or very close to it (for 2004-2005), where variability remains quite low. With these values, the model-to-data RMSE in SWE is definetly lower in the 2-layer canopy model than with the 1-layered versions.
   We insist here that calibration against radiation data enabled the use of more years due to higher reliability of the data (cf. the suspicion of flaws in precipitation data); it also enabled the calibration of parameters directly linked to the radiation transmission/absorption scheme of the model, whereas calibration against SWE data would have relied on additional hypotheses regarding snow interception and rain-snow partitioning.
   However, highlighting the relatively low sensitivity of the modelled SWE to variations of *krnt* and *laifrac* around their optimum values strengthens our confidence in the model robustness. We thank the reviewer for this helpful comment and mentioned this additional result in the revised manuscript.

2. We took your helpful comment into account and modified Fig.3 of the manuscript accordingly.

3. The expression has been changed.

4 and 5: the grammatical mistake has been corrected and the ambiguous sentences have been changed with a view of enhanced clarity.

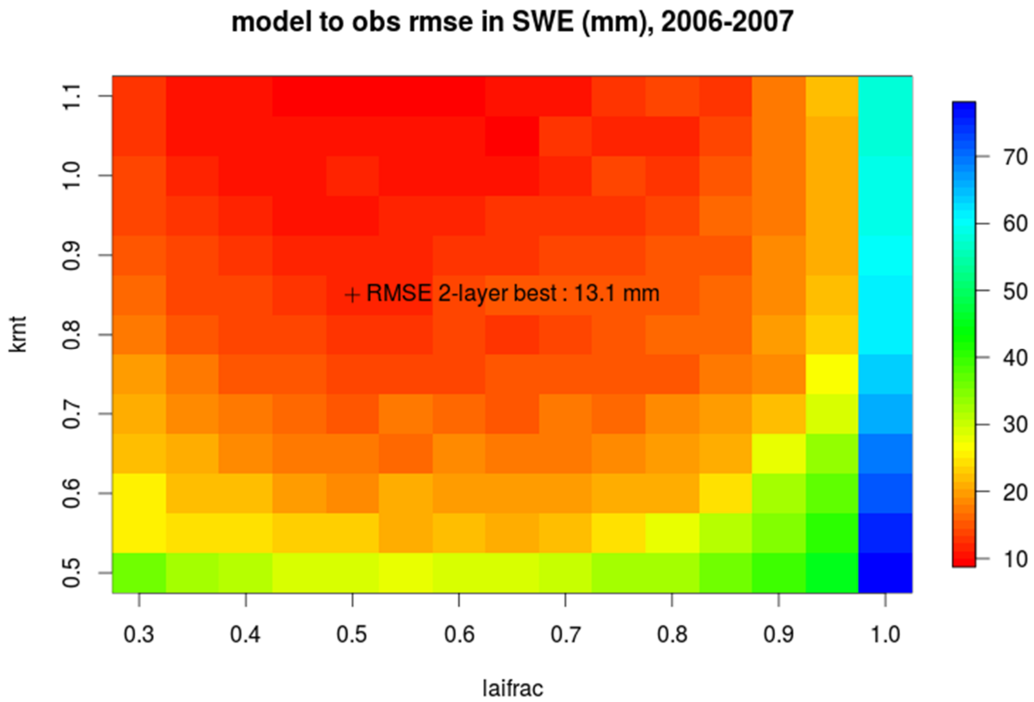

Fig 3. RMSE between modelled SWE and observed SWE at Alptal (2006-2007) as a function of 2 model parameters: *krnt* and *laifrac*. The cross over the plot indicates the result of the calibration of the 2-layer model for these parameters for the long 2003-2007 period (Table 4 of the manuscript). As an indication, the calibrated one-layer model versions (1LnoHM and 1LHM) yield RMSE to observations of 34.9 mm and 64.5 mm, respectively.

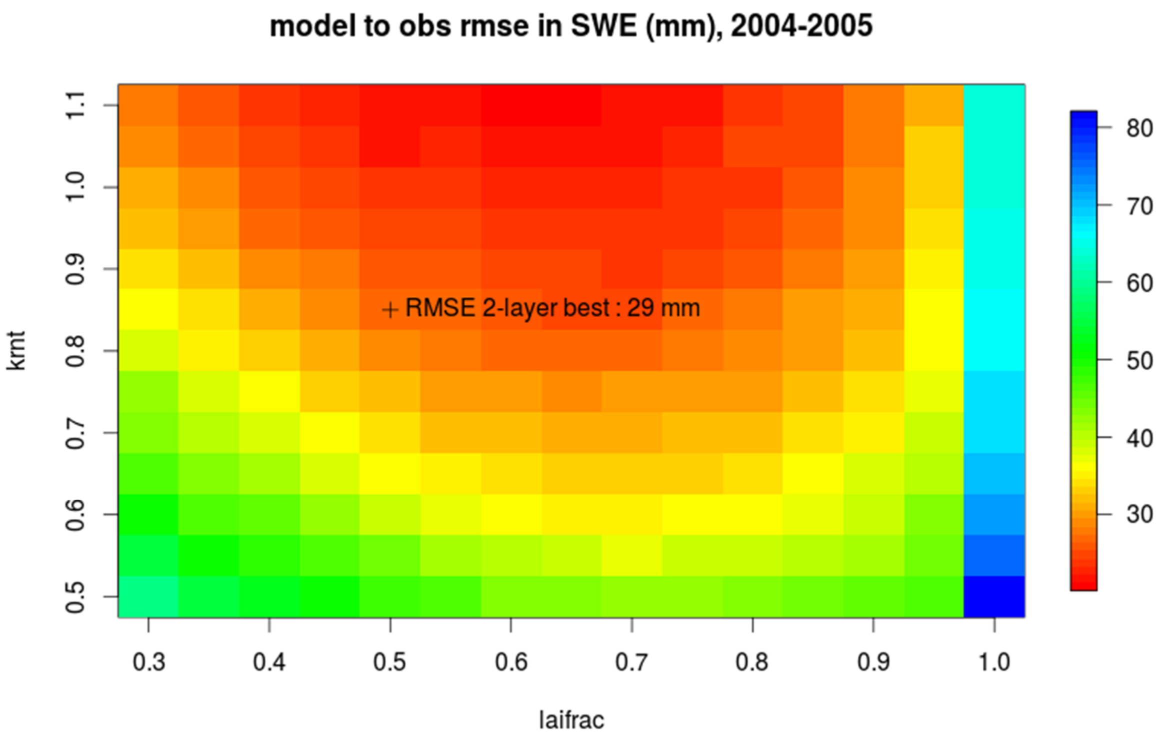

Fig 4. Same as Fig. 3 but for snow year (2004-2005). The calibrated one-layer model versions (1LnoHM and 1LHM) yield RMSE to observations of 39 mm and 77 mm, respectively.

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