**Interactive comment on "Evaluating the performance of coupled snow-soil models in SURFEXv8 to simulate the permafrost thermal regime at a high Arctic site" by Mathieu Barrere et al.**

**-Reply to Anonymous Referee #1**

This paper evaluates model skill in reproducing snow and soil characteristics at a permafrost site in Canada. At this site, a variety of in-situ data were retrieved: snow height, snow stratigraphy, temperature, and conductivity. The aim of the study is to find out how good a coupled soil-snow model is at reproducing the transient temperature signal at this site, during two consecutive seasons (2013-2015). Model runs have been performed with the Crocus snow model and the ISBA soil model. Sensitivity experiments are performed one snow property and several soil properties. In addition, the simpler ES snow model has been included, for comparison.

I think this study identifies important shortcomings in these snow models, some of which will apply to other models as well. The article is well structured and contains material that complements existing studies with Crocus. Unfortunately, the paper is not particularly well written and tends to 'jump to conclusions' which undermines its academic quality. I recommend publication after the following issues have been addressed.

*We thank the reviewer for his positive comments and for taking the time to help us optimize our writing. Our responses are embedded in blue italics.*

Major comments

The paper is too long in general. I believe it should fit on 3/4 of the current number of pages. Leave out unnecessary sentences, e.g. only summarize site instrumentation, and refer to full discussion in Domine (2016a). Rewrite long sentences. More use of academic language which is shorter. Focus on main results. Suggest to combine Results and Discussion. Perhaps make two new sections out of those: (1) Snow model results and (2) Soil model results.

*Thank you for the comment. The paper has been shortened following your suggestions. However, we chose to keep the Results and Discussion sections separated to facilitate the readability of the paper and also to avoid confusion as to what is actual result and what is discussion.*

Grammar: the paper could have a better use of linking words to create flow and thus make it easier to read. There are grammatical errors that need to be fixed before publication. I listed some at Specific Comments but I did not aim for completeness.

Suggest to present the experiments in a table, instead of in the text. This would make them easier to refer to and would help to clarify the text, e.g. Sect. 2.3. Then, introduce the experiments earlier in the text, e.g. 'litter' on page 7 line 1, 'SOC' on page 7, line 8-14, 'wind' on page 8, line 8 . What confused me at first is that the additions are additions with respect to other experiments, not 'base'. It would be great if this could be made more clear.

*Thank you for this useful suggestion. We accordingly changed the presentation of the different experiments to make them clearer. Experiments are introduced in the models description, P5, L12: "Based on a series of incremental runs, we particularly focus on the following model features: surface* *litter, soil organic carbon and density of the drifting snow." And then run 'base' on p.6 l.27, 'litter' on p.7 l.3, 'SOC' on p.7 l.17, 'wind' on p.8 l.8, and 'ES' on p.9 l.11. All the experiments are presented in Table 1. We added details about the iterative experiments (p.9 l.16): "[…] we performed a series of runs with incremental complexities from August 2012 to June 2015 (Table 1). The run wind integrates all our changes, it consists in the most detailed configuration tested here with Crocus. ES uses the same configuration as the run wind."*

One of the main results is Figure 3, that shows that snow density is not well reproduced by the models. It is hypothesized by the authors that this is due to a missing process: upward vapour fluxes. Yet they have no run with this process included so they cannot conclude that this process is fully responsible, only partially, or not at all. The abstract is therefore misleading (Page 1, line 20-22) and should be changed.

*As of today, there is no snow model that describes water vapor transport induced by the temperature gradient. Including this process is a significant research project but is in fact very complex because of the very nature of current snow models that separate the snowpack in several layers of different thicknesses. Efforts are under way but will not lead to a new model for quite a while. That the water vapor flux is important is discussed in detail in Domine et al. (2016a). It is also proven by photographs of shrub branches buried in snow, where depth hoar crystals grow on the underside, demonstrating the existence of a strong flux (Fig. 1). Granted, nothing today proves that this flux is responsible for 100% of the discrepancy between models and observations, but models have other approximations anyway that would make a possible demonstration no more convincing than the observation of intensive crystal growth under shrub branches. In any case, we have modified the abstract (p.1 l.20): "The simulated snow density profiles are unrealistic, which is most likely caused by the lack of representation of the upward water vapour fluxes generated by the strong temperature gradients within the snowpack in snow models."*

Figure 1. Picture of a shrub branch with depth hoar crystals stuck on the underside, demonstrating the existence of a strong upward water vapour flux within the snowpack.

On page 8 line 8 the authors explain that compaction by drifting snow can now reach up to 600 kg/m3, compared to 350 kg/m3 before. The argumentation for this change is anecdotal ('we observed densities of 450 kg/m3'). Should the reader therefore regard this change as just a sensitivity test, rather than a real physical process that was misrepresented? Moreover, doing this you may be compensating for other biases / missing physics in the model, such as the missing upward vapour transport, and, my hypothesis, early melt and refreezing? This potential caveat is not discussed.

*Thank you for these questions. First of all, as stated p17, l. 23, "Numerical models can be viewed as descriptions of a set of complex processes where error compensation is optimized". This is true for Crocus, for ISBA and any other snow or land surface model. And this is why a model used in conditions very different from those it was designed for can lead to large errors: because error compensation is not optimized anymore. At first, as indicated p8, l.6, increasing the maximum density reached by drifting snow concerns a misrepresented physical process, because Arctic snow densities regularly exceed 350 kg $m^{-3}$. In essence, yes, by changing snow density, we are trying to improve error compensation for Arctic conditions, while it is clear that this will in fact attempt to compensate for vertical water transport, which increases upper layer densities. This change was then used as a sensitivity test to assess the impact on snow and soil properties. Changing this parameter may affect other processes, in particular snow driftability and early melt (see next point).*

*This is now stated on p11, l32: "[…] increasing the maximum density reached by drifting snow helps to reduce the underestimation in upper layers." And in the discussion (P17, L1): "This change also partially compensates for vertical water transport, which increases upper layer densities."*

I guess the goal of simulation 'wind' is to simulate a hardened top wind slab. Rather than changing the upper limit to 600 kg/m3 in simulation 'wind', would it not be more effective to decrease the characteristic time scale in the drifting snow compaction (parameter Tau in Vionnet, 2012)? Looking at Figure 3, I see none of the model results exceed 300 kg/m3 at the top, so I wonder if 600 kg/m3 is ever reached at all.

*Changing the characteristic time scale (parameter Tau) was another concern of our work. The results show only slight differences (less than 10%) in snow densities between runs 'wind' (maximum density changed from 350 to 600 kg $m^{-3}$) and 'windTau' (characteristic time scale changed from 48h to 12h), see Fig. 2. Consequences are colder soil temperatures during winter with the run 'windTau', because of the overall more conductive snowpack. Even if the theoretical maximum density of 600 kg $m^{-3}$ is never reached in our simulations, we found interesting to discuss the impact of compensating errors resulting in an apparently well-simulated soil temperature by the run 'wind'. The value 600 kg/m3 is used to change the shape of the relationship between wind speed and snow density, but it does not mean that this value should be reached in the simulations.*

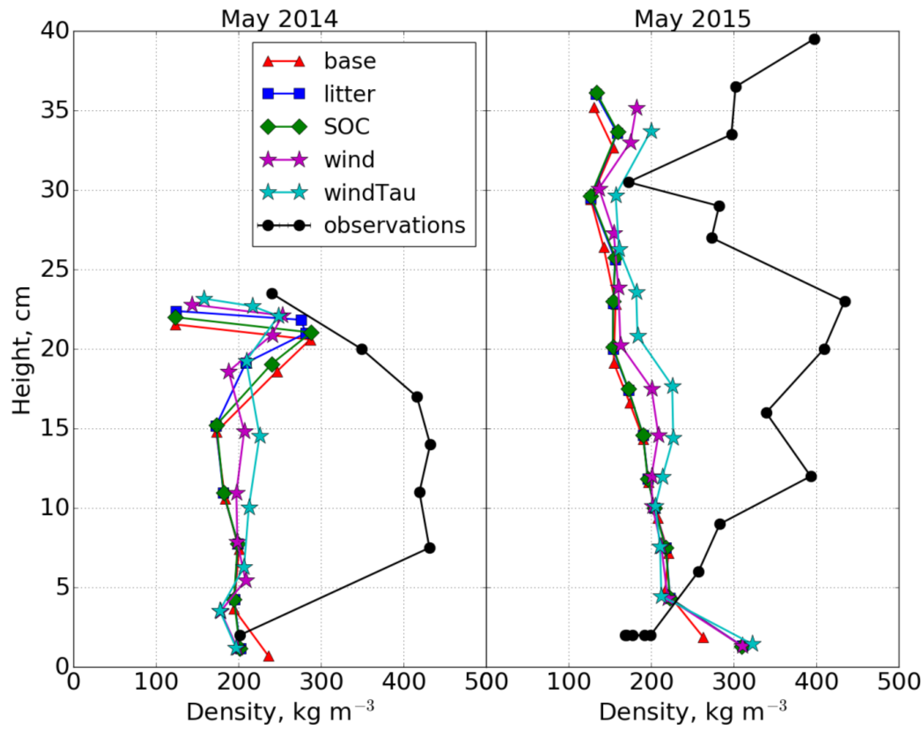


Figure 2. Vertical profiles of density measured on 14 May 2014 (left) and 12 May 2015 (right), and simulated with Crocus on 14 May 2014 and 6 May 2015.

In Figure 3, density in May 2014, the 'wind' experiment simulates lower density at the top than the others. This is counter-intuitive, as you would expect always higher densities in this simulation. Could you provide a possible explanation to why this is?

*Good point. The increase in density of the sub-surface snow layer during spring is due to melt, not to wind-induced compaction. Spring melt is slower in run 'wind' compared to previous experiments, because snow temperatures are lower. This is explained by higher surface thermal conductivities, allowing cold waves to propagate easier to the bottom of the snowpack during winter. In spring, heat waves are also more easily transferred through the snowpack in run 'wind', while in previous experiments heat accumulates in low-conductivity surface snow layers, accelerating their melt-out. A brief explanation is now given on page 11 line 30: "In May 2014, the wind experiment simulates yet lower densities in sub-surface snow layers than the other Crocus runs. This is attributed to early melting, which occurs slower in this run because of colder snow temperatures compared to previous experiments." And in the snow temperature description (P13, L4): "The run wind simulates colder temperatures than previous experiments. This is attributed to the higher conductive upper layers, allowing cold waves to propagate through the snowpack during winter."*

The analysis of snow temperature completely omits the effect of latent heat by rain and meltwater refreezing. What do the authors think is the importance of refreezing on temperature and how do the models simulate this?

*Crocus computes heat fluxes by taking into account latent heat exchanges, including contributions from evaporation of liquid water and sublimation, and melt-refreeze cycle. It also includes a precipitation heat advection term when it is raining. This is detailed in (Vionnet et al., 2012), to which the reader is referred, and it therefore will not be repeated here.*

*Snow melt and water percolation take place in a manner which is spatially very variable. Percolation follows channels in snow, water lenses form very irregularly at discontinuities in capillarities (such as at the base of wind slabs). Simulating this with a 1-D model is a challenge. Observing this using point measurements carries the problem of data representativity. Hence we feel that no interesting discussion on this point can be made here. If we had made an addition, it could only have been "Latent heat exchanges and percolation during snow melt are expected to modify the temperature profile and this is observed in the simulation on 19 May 2015, with the snowpack become almost isothermal. From our measurements, this happens on 10 June. However, given the spatially highly variable nature of percolation, 1-D models and point measurements make any meaningful comparison difficult and of limited interest."*

*It is pretty clear to us that this adds little value to the paper, and for the sake of concision (recommended by the reviewer) we prefer not to develop this point.*

Another key result is Figure 5, that shows that a simple density relationship for thermal conductivity is not sufficient to reproduce most observations. That said, it does not deserve the qualification 'totally inappropriate' (P18, L30).

*Changed to 'inadequate' (P18, L17).*

P17, L5-6: the effect of missing effect of solar zenith angle is stated like a fact. Yet you have no results or reference to support this. Make clear that this is a hypothesis, not a given fact.

*Please see response to referee #2, point 2.*

The two previous comment exemplify a general critique that I have on this article: the wording is not precise enough. In the article, there are sentences without such modifiers that read like facts, but are in reality claims or beliefs of the authors. This must be addressed in the final version.

The authors do not mention whether the model changes they did (litter/SOC/wind) have officially been incorporated into SURFEX.

*Litter and SOC have been integrated to SURFEX v8 (Decharme et al., 2016). Changes we made (maximum snow density value for wind compaction, litter thickness and SOC content) can be easily reproduced.*

Specific comments

P1, L19: soil and snow thermal regime. Simulated soil and snow properties.

*Done.*

P1, L20: compared with → compared to, add comma after 'temperature'

*Done.*

P1, L21: suggest to change ‘erroneous’ to ‘unrealistic’

*Done.*

P1, L29: climate change.

*Done.*

P2, L31: ES is introduced as an intermediate complexity snow model. I would classify this as a simple (yet, multilayer) model, whereas Crocus is of intermediate complexity. A complex model is SNOWPACK (Lehning, 2002).

*Two distinct methods are frequently used to simulate snowpack evolution: the degree-day method used in hydrology and the energy-balance method used in land surface schemes of Numerical Weather Prediction (NWP) and climate models. In the following, we only discuss the complexity of snowpack models using the energy balance method.*

*While there is no official categorization of snowpack models, it is generally agreed that several key components make the delineation between simple, intermediate complexity and sophisticated snowpack models. Simple models are generally single layer schemes used for numerical weather prediction or climate model land surface schemes (see e.g. Essery et al., 1999, Avanzi et al., 2016). Most operational NWP and climate models still use such snow components, while a few of them are gradually moving toward using multi-layer "intermediate complexity" models, with improved representation of heat storage and intrinsic snow processes, which requires multi-layer approaches (see e.g. Essery et al., 2013). ES clearly belongs to this category of models (Decharme et al., 2016).*

*Beyond, there is still a higher class of sophistication for snow models, which in particular includes explicit representations of snow microstructure and sophisticated approaches for handling e.g. solar radiation budget and liquid water percolation. Crocus and SNOWPACK feature approximately the same level of detail for the representation of snow microstructure and its evolution (through the use of semi-empirical variables such as dendricity, sphericity, sometimes replaced by specific surface area - see Carmagnola et al., 2014, and the associated evolution parameterizations), and both handle a large number of snow layers in a Lagrangian way. Liquid water treatment was recently improved in SNOWPACK using the Richards equation (Wever et al., 2014), also more recently implemented in Crocus (D'Amboise et al., 2017). The original version of Crocus uses three spectral bands to compute solar light radiative transfer and albedo, based on physical properties of snow and snow age (Vionnet et al., 2012), while SNOWPACK currently uses a broadband albedo parameterization. Following recent developments, Crocus is now equipped with a spectrally-resolved radiative transfer scheme (Libois et al., 2015), and a multi-physics version of Crocus was recently published (Lafaysse et al., 2017- which includes parameterizations initially implemented in SNOWPACK). Clearly, SNOWPACK and Crocus belong to the same category of snowpack models, with comparably similar levels of sophistication depending on the process.*

*As a consequence, we do not think it is necessary to change the description of ES and Crocus in the manuscript.*

P4, L30: snow pits are two words

*Corrected.*

P5, L12: SURFEX v8, as in title?

*Added.*

P5, L11: why did you not do bias correction on the radiation data?

*As indicated in the paper (p5 l.21), the radiometer shifted from its horizontal position. Therefore, as we added on page 6 line 9: "ERAi radiation data were kept unchanged for lack of reliable measured values"*

P8, L8: 'we increased this value to 600 kg/m3' → only in simulation wind!

*Indicated.*

P9, L8: models not model

*Done.*

P9, L28: units of thermal insulance are m**2 K / W , see your Figure 7. Units of thermal resistance are K/W. Rename to insulance, or change units to K/W.

*Changed to thermal insulance, thanks for this comment.*

P10, formula 5 and 6: parts are missing

*Corrected.*

P10, L20: this made me wonder, does SURFEX have a representation of snow cover fraction?

*SURFEX can indeed represent the surface cover fraction of snow, vegetation and bare ground. Here, we simply used a snow cover fraction of 0 when the surface was snow free, and of 1 when at least 1 mm of snow was covering the ground. We thus do not think it is relevant to add details about it in the paper as what we did on this aspect is pretty clear.*

P11, L1: Snow height was not well reproduced in 2013-2014 so a 30% reduction to precip has been applied in the model runs. The authors do not discuss the phase of precip. Did you experiment with the temperature threshold for snow?

*Changing the temperature threshold of ±1°C for the ERA-interim precipitation phase recalculation lead to changes of 9-10% in cumulated snowfall between August 2013 and August 2015. Because of the high uncertainty on the precipitation rate, and given that observed snow height is quite well reproduced in winter 2014-2015 without changing the snowfall amount or the temperature threshold for precipitation phase, we chose to arbitrarily reduce the snowfall amount for the winter 2013-2014.*

P11, L2: "in good agreement with the snowpits". How about automatic gauge?

*Snow gauge data are shown in Figure 2. As stated in the text, automatic snow gauges give just one point measurement while snow pits yield several data points and are therefore more representative. We feel this was detailed enough in the text, e.g. page 4 line 21, page 10 line 16.*

P11, L6: "it seems to be " is not academic English

*Changed for "which could be".*

P11, L12: I'm missing the causal relationship here. Restate like belief or hypothesis.

*Please see response to referee #2, point 2.*

P11, L30-31: suggest to restate: this partially compensates for the underestimation of density in the upper layers

*Done (P11, L28), thanks for the suggestion.*

P12, L5-6: why is the mean density in ES higher than in Crocus? Is this due to the discretization only, or are there differences in the physics?

*This is due to the faster compaction rate in ES, because it does not account for the snow microstructural property whereas Crocus reduces compaction rates for crystals of depth hoar, frequently found in the Artic snowpack.*

P12, L9: Suggest to restate: thermal conductivity is primarily controlled by density.

*Changed to (P12, L4): "Since thermal conductivity is totally (in Crocus) or mostly (in ES) controlled by density".*

P12, L25: 'gross' is not academic English.

*Changed to "very large".*

P13, L9: suggest to start the sentence with 'In winter, . . .'

*Done.*

P13, L21-22: suggest to move this to Methods.

*We prefer to let it here to facilitate the comprehension.*

P13, L23: unclear

*Changed to (P13, L17): "Litter and SOC additions have little effect on snow properties, the most noticeable being a reduction of less than 1 cm in snow height."*

P14, L14: unclear

*Answered to referee #2, point 5.*

P14, L17: spelling error 'sate'. Suggest to check entire Latex document using aspell.

*Done.*

P15, L1: 'which confirms' too strong?

*Changed to "which supports".*

P15, L7: warm temperature -> high temperature, 'and at the' → 'at all'

*Done.*

P15, L20: suggest to change 'It is because' to 'This is attributed to'

*Changed to "This may be attributed".*

P15, L27: Rˆ2 is used, yet Table 1 lists R.

*We are sorry, but Table 1 clearly states r2.*

P16, L7: suggest to remove ‘but’, and start sentence with ‘Although’

*Done.*

P16, L22-26: discusses temperature, not soil water content.

*Removed.*

P16, L29: suggest to drop ‘important’

*Removed.*

P16, L31: suggest to drop ‘considerable’

*This is what shapes the Arctic snowpack. We do confirm that the impact is considerable.*

P17, L1-3: need reference

*There are already 2 references. The fact that density errors propagate to thermal conductivity is because thermal conductivity is determined solely by density, as already discussed in detail in many places in the paper. We feel there is no need to repeat this here.*

P17, L5-6: need reference or rewrite.

*Rewrote as (P16, L23): “Further, downwelling shortwave absorption is lower in the Arctic because of the large zenith angle in late winter, and this is not accounted for in the original version of Crocus. Therefore, solar warming of the snowpack is exaggerated, resulting in incorrectly simulated melting episodes.*

P17, L8: move TARTES discussion to Methods.

*Done (P8, L21).*

P17, L17: maximum snow density reached by drifting snow

*Done.*

P17, L22: ‘main factor influencing soil properties’. Do you mean thermal and/or hydraulic properties?

*Both, changed accordingly (P17, L7).*

P17, L28: suggest to change ‘increases’ to ‘enhances’

*Done.*

P18, L19: suggest to use ‘ERA-Interim’, not ‘ERAi’

*Done.*

P19, L2: provide reference to SNOWPACK, Lehning et. Al. (2002)

*Done.*

P19, L12: Crocus is no longer ‘most sophisticated’. It is intermediate complexity.

*See the previous answer.*

P19, L15: to GCMs

*Done.*

P19, L19: Figures.

*Done.*

P22, L6: Reference Elberling et al. should start on new line.

*Done.*

Figure 7: units for ksnow are misspelled. Suggest singular form in caption (snow height, thermal conductivity). Rename resistance to insulance.

*Done.*

Figure 9: Depth has positive value in the soil. Switch to height if you prefer negative values. No model output between 10 and 20 cm depth, why not include all points that the model has?

*Done. No points between 10 and 20 cm depth.*

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