Response to interactive comment from Referee #1 (Richard Essery) :

Authors responses are shown in blue. Proposed changes in the manuscript are reported in bold.

I think that this is an important paper. Several recent studies have also used multiphysical snow models, but they have been rather exploratory in nature, e.g. investigating sensitivity to missing or simplistically represented processes. The different approach of this paper in seeking to construct an ensemble of equally plausible models is a necessary step towards being able to use mulitphysical ensembles to characterise model error for data assimilation.

On behalf of all authors, we thank Richard Essery for the value he found in our work as well as for his detailed and relevant suggestions.

I have some minor questions, correction and suggestions:
p8, Figure 2

Because there is only one option used for snow drift, and that is not to have snow drift, it doesn't seem worth having a box for it in this figure.

We agree and removed this box in Figure 2.

p11, Table 2
Where does the parameter value l f = 0.05 m come from? Why not just add dry deposition to the surface layer?

The e-folding depth parameter l_f comes from the fact that impurities are deposited preferentially at the surface but some may also be deposited below the snow surface (a few cm) because of air circulation and adsorption of impurities on the snow microstructure. Because the thickness of snow layers vary in time, rather than specifying a fixed deposition rate for a given number of upper layers, we assign a characteristic length for the penetration of impurities at and below the snow surface.

This parameter was first introduced in Charrois et al, 2016 and set to 0.05 m. Although rather arbitrary, it was not modified in this paper. From the literature (Clifton et al, 2008) it might be that values around few mm are more physically consistent (characteristic scale of wind pumping effect) However, as illustrated in Fig C1 below, snow depth or snow albedo simulations are almost not sensitive to the e-folding value within [1 mm – 10 cm] range.

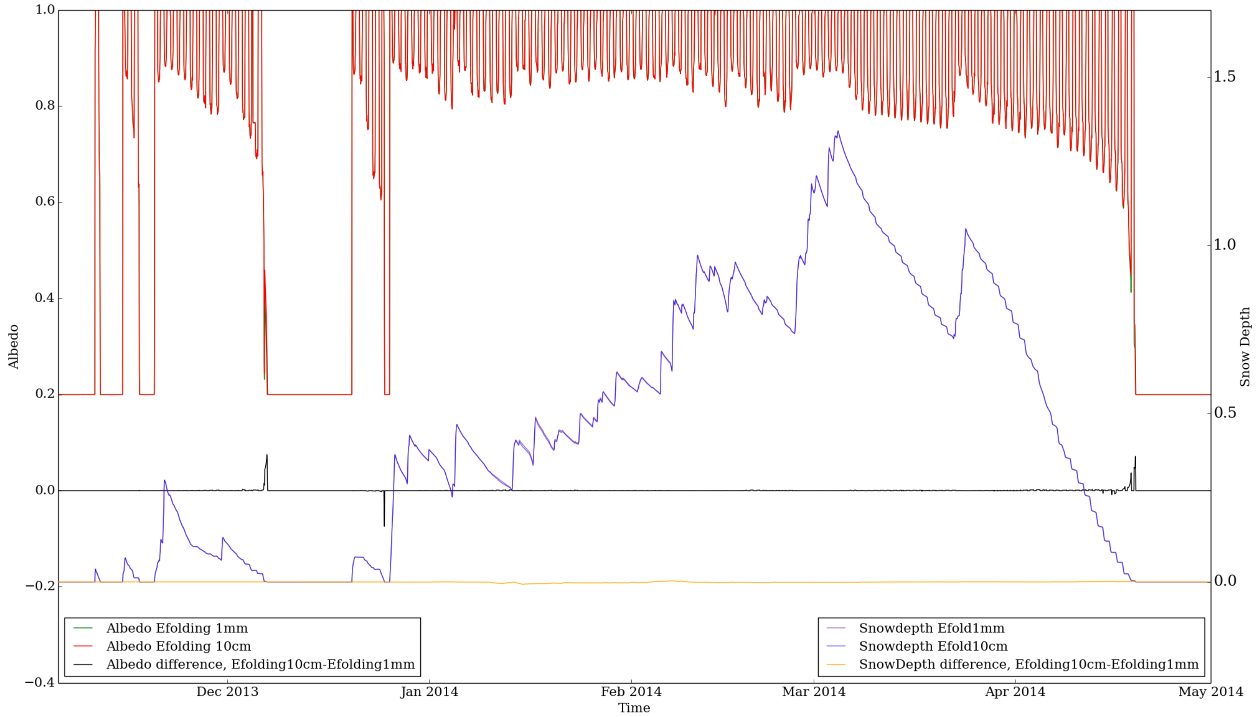

Fig. C1 Simulated snow depth and albedo for 2 simulations based on 10 cm and 1 mm e-folding depths. The simulations are so close that the lines are overlaid most of the time. The differences (black line for albedo and orange line for snow depth) always stay close to 0.

We modified the manuscript as follow:

« This formulation and its parameters are rather uncertain as it has not been specifically evaluated against observations. While ***the simulations are weakly sensitive to the e-folding depth l_f , the simulated albedo highly depends on the velocity of impurities deposition.*** *The typical magnitude of the parameters for black carbon [...] »*

p13, equation 8
It is fairly obvious what P is, but I don't think it has been stated anywhere. Same comment about ρ w and ρ i .

We apologize for not having defined these 3 variables. This is modified as follow in the revised manuscript:
Page 12, line 11: « ***ρ_w is the liquid water density (kg m^{-3}).*** »
Page 13, line 3: « ***P is the atmospheric pressure (Pa) and*** *parameter values are given in Table 4.* »
Page 14, line 7: « *where $\varphi = 1 - (\rho - w_{liq})/\rho_i$ is the snow porosity* ***and ρ_i the pure ice density (kg m^{-3})*** »

p14
The number of typos identified in previous papers concerning maximum liquid water holding capacity of snow is striking. This paper itself is not immune. Are the options W14 in section 3.7, S14 in Figure 6 and SPK in Figure 2 all the same thing?

Yes, they are. We apologize for this confusion and homogeneized the manuscript with the SPK abbreviation (for SNOWPACK model).

Plotting equation 11, I don't get the same curve for C98 as on Figure 6; please check.

The computation has been checked but we did not find any issue. To plot the liquid water holding capacity as a function of snow density, it is necessary to replace the porosity in equation 11 by equation 10 at saturation. The development gives a second degree polynom. One of the solution gives the liquid water holding capacity as a function of snow density corresponding to the C98 curve in Figure 6.

p16, equation 18
The dimensions of this equation are wrong, according to the units of the variables given in the text.

Thank you for noting this typo. We apologize for the incorrect unit given for the heat capacity which is expressed in **J m^{-2} K^{-1}** in equation 18. The unit was corrected everywhere in sections 3.9 and 5.3. This typo was due to the fact that we commonly assume a mass of 1 kg m^{-2} for low vegetation. In that case, the heat capacity value in J kg^{-1} K^{-1} is equal to the heat capacity expressed in J m^{-2} K^{-1}.

Incidentally, what are the thickness and heat capacity of the first soil layer, and is freezing of soil moisture allowed for?

The thickness of the first soil layer is now given in the manuscript (0.01 m). Its heat capacity is computed as the sum of the water heat capacity and the heat capacity of the soil matrix (Decharme et al, 2011). Freezing of soil moisture is allowed (Decharme et al, 2016) but for a better clarity equation 18 is given in the case without any phase change.

Page 16 line 16:
« where G is the sum of radiative and turbulent energy fluxes at the surface (W m^{-2}), λ_1 the thermal conductivity of the first soil layer (W m^{-1} K^{-1}), Δz 1 its thickness ***(0.01 m)****, c_{G1} its heat capacity (J m^{-2} K^{-1})* ***depending on its water content****, and T_2 the temperature of the second soil layer.* ***Equation 18 corresponds to the case without soil freezing or thawing which are also represented in the model (Decharme et al., 2016).*** *»*

p23, Figure 8
The first two columns appear to be identical and both are labelled E2.
Thank you again, there was a bug in the preparation of subfigures. This is now corrected. The comments on this figure were not affected by this bug.

p27, Figure 13
Is it worth repeating the black triangles for heat capacity options? The conclusions about the dependency of B60 on heat capacity could be drawn from the same triangles on Figure 11, leaving the possibility of comparing options for solar radiation absorption and defaults in Figure 13.
We modified Fig. 13 to apply this good suggestion and improved in the manuscript the description of the dependency between both processes:

“The B60 option (blue points) could be associated with a positive bias of snow depth in the default Crocus version with a 10000 J m −2 K −1 surface heat capacity (down-facing triangle ***in Fig. 11 and 13), and the B10 or TA+ options preferred (left and right-facing triangles in Fig. 13). However, an opposite conclusion is obtained if the surface heat capacity option is changed: the positive bias of B60 disappears (right and up-facing triangles in Fig. 11) and a negative late snow depth bias appears in spring for B10 and TA+ (not shown).*** *Numerous similar dependences of the skill of a given option to the choice of other processes were found […]”*

p29, Figure 14
The argument that equifinality results from counteractions of the extreme TA+/B60 absorbed solar radiation options and RIL/M98 turbulent heat flux options is plausible. Do pairs of members differing only in these options exist within E1? The SD plot could include observations.

Yes, there are several pairs of members differing only in these options within E_1. At first, we thought it might be interesting to illustrate the behaviour obtained by a full set of different physical options. However, it is indeed probably easier to understand equifinality by limiting the differences to 2 processes in this illustration. Therefore, following your remark, we decided to modify this Figure by using 2 members illustrating the equifinality between these 2 processes only. We selected a different year illustrating better this behaviour for these members. Nonetheless, it is important to notice that equifinality can sometimes come from more complex interactions between more than 2 physical options. We added this remark in the revised manuscript. We also added the snow depth observations from the ultra-sound gauge and from the pits on this Figure. This illustrate that the small differences in snow depths between these two members are lower than model errors and than the uncertainty of the reference data. The corresponding paragraph was modified as follows:

"To illustrate how the different options create dispersion in the optimal sub-ensembles, we compare in Fig. 14 for one particular season (2003-2004) the energy fluxes of ***2 different members of the*** $\boldsymbol{E_1}$ ***sub-ensemble of optimal members. The two members were selected because they have different options for solar radiation absorption and turbulent fluxes (B60/M98 and TA+/RIL) but the same options for all other processes. The absorbed solar radiation is significantly higher in member with the TA+/RIL options than in member with the B60/M98 options, especially in February and March, whereas the turbulent heat fluxes are significantly lower in member with the TA+/RIL options than in member with the B60/M98 options.*** *As a result, the temporal variations of the energy balance differs between the two members, with a higher positive balance for member B60/M98 during some windy and mild events in winter and conversely a higher positive balance during some spring sunny days for member TA+/RIL. Therefore, there is a* ***slightly*** *different chronology of melting in these members although the final melt-out date difference is only 2 days.* ***These differences are lower than model errors and lower than the uncertainty range of observations.*** *This illustrates that very different contributions of energy fluxes to the energy equilibrium can result in a similar and optimal skill for all evaluated variables (both members are included in* $\boldsymbol{E_1}$ *sub-ensemble). This equifinality also exists between the other physical processes and options,* ***with some more complex interactions involving more than two processes.*** *It explains (i) the difficulty to select a single-model and (ii) the dispersion obtained at a given point in time by several members seen as equivalent and optimal from a deterministic statistical evaluation."*

Reference

Clifton, A., Manes, C., Rüedi, JD. et al., 2008, On Shear-Driven Ventilation of Snow, *Boundary-Layer Meteorol* 126: 249. doi:10.1007/s10546-007-9235-0