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Comment

## ***Interactive comment on “Impacts of snow and organic soils parameterization on North-Eurasian soil temperature profiles simulated by the ISBA land surface model” by B. Decharme et al.***

### **Anonymous Referee #2**

Received and published: 23 February 2016

#### General comments:

This well written manuscript presents how development of parametrisations with respect to snow and soil processes leads to significant improvements in SURFEX model behaviour for climate regions represented by snowy winters and/or high soil carbon content. As SURFEX represents a widely applied land-surface model covering modelling communities in both NWP and climate plus site specific studies this manuscript represents an important documentation of SURFEX development.

#### Specific comments:

Page 6744, lines 2-6: I acknowledge the fact that your documentation of the current  
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ISBA physics should be limited to those aspects that are improved and presented in Section 3 of this paper. But, one important aspect, or sensitivity, of the Douville type of snow schemes, which is not mentioned in the paper now, is the reset of albedo to  $\alpha_{\max}$  at a certain snow-fall intensity. Is that aspect of the snow albedo still the similar or is it also changed?

Page 6744, line 14: You state that thin snow layers at the bottom of the snow pack ensure a good computation of the snow/soil heat conduction. However, the snow/vegetation/soil interface is often represented by quite a complex layer of vegetation/litter/soil which is difficult to represent well. Thus, isn't the uncertainty in heat conduction due to the uncertain characteristics of this layer so large that the gain given by very thin bottom snow layers is questionable without carefully also looking into the interface layer itself?

Page 6744, line 21: What is the argument to chose exactly 12 layers and not e.g. 10 or 14 or...?

Page 6748, Section 3.1.3: The snow age plays an important role in the albedo and extinction coefficient formulations so please consider to add the expression for the snow age itself.

Page 6749, line 8: Do you have any information on how sensitive your described parametrisation may be for uncertainties in the soil organic carbon content hear given by HWSD?

Page 6753, line 9: A very low local roughness for an open snow patch (0.001 m) but surrounded by forest is always a bit suspicious to me. Are you convinced that the local snow roughness is representative for this site?

Page 6756, lines 7-10: Brun et al. (2013) concluded that “we recommend scaling ERA-Interim precipitation with GPCC only for those models that do not simulate the blowing snow sublimation”. So, just to understand precisely, in this study you do not consider

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blowing snow, right?

Page 6758, lines 3-4: In permafrost climate studies it is usually concluded that a very deep soil column is needed to correctly simulate permafrost dynamics. Something like on the order of 100 m. The time scale of your study is not so long but what do you think, would a 12 m column only have any noticeable implications on the results?

Page 6767, lines 6-7: Isn't default value of  $w_{snv}=5$ ?

Figures 3 and 4: I find it very hard to understand the performance of the different simulations from the daily-value sub-figures. Any interesting short period deviations just drown in the huge annual cycle. Please consider this suggestion: Use CTL and NEW results only in the daily absolute figures to illustrate the variability. Could you linearly interpolate the observations and show the bias as continuous time series (maybe also apply a short running mean). Then any interesting short-period deviations will be easier distinguishable. Ok, the annual cycle figures in Figure 5 are more informative.

Technical corrections:

Page 6736, line 20: At the end, replace “us” with “is”.

Page 6736, line 21: Space missing in “anda”.

Page 6745, line 18: Please correct reference to equation, i.e. Eq. (9) should be (11), right?

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Interactive comment on The Cryosphere Discuss., 9, 6733, 2015.

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