Author's response to RC2 comments

RC2 (Richard L.H. Essery)

This is an interesting and generally well written paper, although it is unclear in parts. My comments are minor but numerous. The description of the simulation protocol needs to be improved; after reading the paper three times, I think there are three experiments – initial simulations, XP0 and XP1, also called "_new" – but I am rarely confident of which is being discussed at any time.

Thank you for the remark. The section 2.2 has been rewritten to make it clearer and better explain the protocol that has been used. Here is the new content of section 2.2:

The models were run offline, i.e. guided by the atmospheric forcing measured at Dome C, for a total simulation time of 15 days. Snow temperature was initialized from in situ measurements. First, each group had to provide the results obtained with the default settings of the surface parameters of their model. The models were run offline, i.e. guided by the atmospheric forcing measured at Dome C, for a total simulation time of 15 days. This set of simulations is called XP0, it includes one simulation for each of the 10 models and the name of an experiment is made up of the name of the model suffixed by "_xp0". From the simulations in the XP0 set, each participant could propose additional simulations. Only CHTESSEL and CLM4 performed calibration simulations, aiming at minimizing the root mean square error on the surface temperature. CLM4 calibrated the surface albedo and CHTESSEL calibrated the snowpack thickness and from this calibration then calibrated the dynamic and thermal roughness lengths. The names of the corresponding experiments are CLM4_cal1, CHTESSEL_cal1 and CHTESSEL_cal2 respectively. In addition, a rerun was proposed in order to better represent the diurnal cycle and to reduce the dispersion of the surface temperature results, but also to see if this dispersion was reduced or not in the coupled single-column simulations (this last point is not addressed in this study).
Snow albedo depends on zenith angle, but also on grain size and cloud cover. At Dome C, because the sky is generally clear, the effect of solar zenith angle is prominent compared to the typical diurnal cycle. Warren (1982) showed that the albedo of snow was maximum when the sun was low while its effect was less when the sun was at the zenith, it then enabled the surface to warm, or at least cool less by radiative effect. Most models do not consider the variation of albedo with the zenith angle, a fixed average value is proposed in the experimental protocol, corresponding to the average value of the ratio between the incident and reflected radiation measured at Dome C, over the period considered. Concerning the thermal emission of snow, the value of 0.98 is within the range of values commonly used for this type of medium. For the dynamic and thermal roughness lengths, values of 1 mm and 0.1 mm were chosen respectively. The dynamic roughness length is close to that established by Vignon et al. (2017a) who studied the effect of sastrugi on flow and momentum fluxes and proposed using a thermal roughness length that is one order of magnitude smaller than the dynamic roughness length. This ratio of 10 is classically used in many models calculating fluxes at the surface-atmosphere interface. Snow density at the surface can range from 20 kg m^{-3} for fresh snow to 500 kg m^{-3} for old, wet snow. Measurements at Dome C during the summer of 2014-2015 (Fréville, 2015) show that the snow density profile varies between 250 kg m^{-3} and 310 kg m^{-3} between the surface and 20 cm depth (Gallet et al., 2011).

Therefore, all participants were asked to run a new simulation with an albedo of 0.81, an emissivity of 0.98 (which corresponds to the average emissivity of the hemisphere (Armstrong and Brun 2008)), dynamic and thermal roughness lengths of 0.001 m and 0.0001 m, respectively, and for single-layer schemes, to impose a snow density of 300 kg m⁻³, as well as the snow thermal coefficient c_s=3. 166 ×10⁻⁵ (K m² J⁻¹). This coefficient is directly involved in the temperature evolution equation along the vertical:

$$C_{snow} \times \frac{\partial T(z,t)}{\partial t} = \frac{\partial}{\partial z}\left(\lambda(z)\frac{\partial T(z,t)}{\partial z}\right) \qquad (1)$$

$$c_s = (h_{snow} \times C_{snow})^{-1} \qquad (2)$$

Where $\lambda(z)$ is the heat conductivity of snow, h_{snow} the snow depth (m) and C_{snow} the volumetric heat capacity of the snow (J K⁻¹ m⁻³).
Moreover,

$$C_{snow} = c_i \times \frac{\rho_{snow}}{\rho_i} \qquad (3)$$

where c_i and ρ_i are the heat capacity and density of the ice respectively. Combining equations (2) and (3) gives finally equation (4):

$$c_s = \rho_i \times (h_{snow} \times \rho_{snow} \times c_i)^{-1} \qquad (4)$$

Taking a thickness of $h_{snow} = 5$ cm, densities of snow and ice of 300 kg m⁻³ and 900 kg m⁻³ respectively, and the heat capacity of ice $c_i = 1.895 \times 10^6$ J K⁻¹ m⁻³ we obtain according to equation (4) the value of c_s. This rerun is named XP1 and the name of the simulations that refer to it consists of the name of the model suffixed by "_xp1".
Not all models were able to perform this new experiment, either due to lack of time or because the results came from an operational model that did not allow for adjustment of certain parameters or variables in the schemes. Although not all of them participated, it is interesting to study the impact of the changes induced by the XP1 configuration on the simulations of the XP0 ensemble, considering, when they exist, the calibrations. We therefore calculated the daytime and nighttime biases, as well as the difference in RMSD between XP1 and XP0 (or the simulation calibrated from a simulation of the XP0 ensemble), and evaluated the impact on the model error.

The introduction includes an extensive review of previous snow model intercomparisons, but these are all for seasonal snow with a focus on snowmelt and runoff, and are not very relevant for this paper. If going into this level of detail, however, uncertainty in model outputs due to uncertainty in meteorological driving data should also be mentioned

https://hess.copernicus.org/articles/19/3153/2015/hess-19-3153-2015.html

The text was modified as follows:

Previous snow model intercomparison have focused on seasonal snow with an emphasis on snowmelt and runoff, but here we are dealing with a different climate where snowfall is low in annual accumulation, and the snowpack is dry, subject to strong wind transport. However, a common feature with other intercomparison concerns the uncertainty in model outputs due to the uncertainty in the baseline meteorological data (Raleigh et al., 2015).

15

The abstract should acknowledge that the intercomparison is for a single site on the Antarctic Plateau

Done:

The results of offline numerical simulations, carried out during 15 days in 2009, on a single site on the Antarctic plateau, show that the simplest models are able to reproduce the surface temperature as well as the most complex models provided that their surface parameters are well chosen.

22

Can it be said that the surface temperature errors are consistent with the magnitude of sensible heat fluxes being too great both day and night?

A sentence was added in the abstract:

The surface temperature errors are consistent with too large a magnitude of sensible heat fluxes during the day and night.

27

This is a standard way to start a snow modelling paper, but snow is not a water resource at Dome C (and it isn't a key element of the landscape – it is the landscape!). The snow cover does not vary considerably in time and space.

I wanted to keep the beginning of the introduction as general as possible, so I kept it as is. However, I have added some elements to highlight the specificities of the Dome C site.

In Antarctica, and more particularly in the interior of the continent, as at Dome Charlie (Dome C hereafter), conditions are very different, since snow is the landscape, and it shows relatively little spatial and temporal variation. In addition, the conditions are very dry and cold, which prevents the snow from melting and precipitation is rare. At Dome C, the small amount…

69

"snow patterns" suggest spatial distribution of snow cover, which is not considered in Schlosser et al. (2000).

Snow patterns was changed into snow model complexity

88

There are two sites in Etchevers et al. (2004).

a snow-covered low vegetation site was changed into two mountainous alpine sites

133

"that that water may freeze"

done (one "that" was removed)

150

The information in Table 1 is already provided in the text and the author list; it could be deleted.

done

154

All of the information in this sentence is repeated with more detail in the following sentences.

Section 2.2 was rewritten.

173

Rather than a list in the text, layer depths might be better presented in a table, which could then include the initial temperature and density profiles. How was initial temperature prescribed for single-layer models?

This information has been added to the end of the new section 2.2 and a table was added:

For the multilayer models, the snow density and temperature profiles were initialized from observations. Note that the single-layer models use a fixed density close to 300 kg m^{-3} which corresponds to a depth of about 10 cm in the initial profile, and their initial temperature was also provided from in situ measurements. Table 1 describes the vertical discretization and gives the initial temperature and snow density profiles. The LMDZ model is a special case. Indeed, it is a ground thermal model with the thermal inertia of snow that is used and not really a snow scheme, which is why there is no snow density as such.

174

The sensitivity tests mentioned here are XP0 and XP1, not additional test deemed relevant by the participants?

No only 2 models performed calibration tests to minimize the surface temperature RMSD. It is explained in section 2.2.

179

No need for equations 1 and 2 to be bracketed with {

done

196

This is the only mention of calibration. It is important to know if some of the models have been calibrated for these simulations, and how.

It is now explained in section 2.2

205

The K&Z CM22 does not just measure visible radiation (and, in the Figure 1 caption, not just direct solar radiation).

The table caption has been changed to explain that the focus is on the forcing data and indicates which sensor was used to measure it (this does not mean that the sensor does not measure something else):

Presentation of the forcing data (name, unit and position on the measuring mast) and instruments used for the measurements.

Direct solar was changed into downward solar in Fig. 1

Table 2

Wind direction is not used. A Vaisala HMP155 measures relative humidity, which requires temperature for conversion to specific humidity. Genthon et al. (2017) consider humicap measurements to be biased low for the conditions of Dome C.

https://acp.copernicus.org/articles/17/691/2017/acp-17-691-2017.pdf

Wind direction was removed from table 1. Reference to HMP155 for air temperature was removed.

245

Could say when the temperature probes were installed.

It is now indicated in the text (26 November 2006)

Figure 2

Why are temperatures shown as not filled at four depths?

It was an error and the figure has been updated

Table 3

The caption should explain the use of square brackets.

The caption was modified into:

Table 2: Range of variation of model surface parameters for XP0. The values in square brackets indicate the values taken by a parameter when it is calculated by the model while the single values are fixed during the simulation.

316

I assume this is not a linear profile between the surface and 10 m depth.

Right, see answer to item 173 where a vertical description is given.

Figure 5

Adding air temperature to this figure would be an interesting comparison.

Done and the caption was updated.

341

The PDFs in Figure 6 have too many extrema to be cubic functions. Were they, in fact, fitted with cubic splines?

Functions was replaced by splines

389

What does "a root mean square error that varies from simple to double" mean?

The text was modified into:

a root mean squared error that varies from 0.4 K (CHTESSEL) to 1 K (JULES)

391

The more sophisticated model do not have to represent the evolution of albedo in XP0, as I understand it.

No, they do have to model it, in fact the albedo parameterization is not disactivated. The protocol is better explained in section 2.2.

400

Wind speed is always greater than or equal to zero, so taking its modulus does not add anything.

"Modulus of the" was removed from this sentence

401

The proportionality constant is not simply the surface exchange coefficient; air density, and heat capacity for sensible heat flux, are also required.

The sentence was replaced by:

For sensible heat flux, the proportionality coefficient is the turbulent surface exchange coefficient multiplied by the air density and the heat capacity.

417

Assumed overestimation of sensible heat fluxes in stable conditions is a longstanding feature of models, although it can prevent larger biases in surface temperature.

https://journals.ametsoc.org/view/journals/clim/10/6/1520-0442_1997_010_1273_votseb_2.0.co_2.xml

This comment and the reference were added in the text:

The assumed overestimation of sensible heat fluxes under stable conditions is a long-standing feature of the models, although it may prevent larger biases in the surface temperature (King and Connolley, 1997).

430

Equation 5 is how Qh is measured and equation 6 is how it is modelled.

Measured and modelled by were added in the text

442

This is the bulk Richardson number, not the gradient Richardson number. I would have guessed that GDPS and CLM4 are singled out because they characterize stability by the Obukhov length, but that is the case for JULES also.

Gradient Richardson number was changed into bulk Ri. Yes, that's right GDPS4 and CLM4 are singled because they characterize stability in a different manner than the other models using LMO. But that is not the case for JULES which is also using the Louis model based on Ri.

464

The vertical temperature profile is well initialized by construction.

Right, I wanted to mention that the initialization was identical and correctly set up.

The sentence "It can be seen that the vertical temperature profile is well initialized for the four models." was changed into:

It can be seen that the initialization of the vertical temperature profile is identical and correctly configured for all four models.