Reply to both referees of “Simulation of permafrost and seasonal thaw depth in the JULES land surface scheme” by R. Dankers et al.

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We thank both referees for their constructive and helpful comments. Below we will address the issues that were raised by the referees.

1 Comments by Andrew Slater

Andrew Slater made the following comments and suggestions:

(1) Lawrence & Slater (2008) also include organic content varying with depth.

Response: this is true, but the *properties* of organic material are held constant. At least, this is our understanding of Lawrence & Slater (2008, Table 1) who also state (p.149) that “*for the sake of simplicity and because we do not have information on the extent of decomposition of the soil carbon material, the parameters used in this model and listed in Table 1 are standard literature values for undecomposed organic material (also known as fibric peat).*” This seems to suggest that only a single set of parameter values was used. In our setup, both the organic content *and* the parameter values of pure organic soil vary with depth (cf. Table 2).

(2) It would be informative to include information about the skill of the meteorological forcing at the observation site.

Response: this is a good point. Please have a look at the figure below, which makes a comparison between 2.0 m temperature in the WATCH forcing data and the observed air temperature at 1.5 m at Toolik in 2001. It will immediately be clear that differences between the two cannot be the reason for the much colder soil temperatures in the simulation. Although WATCH is indeed colder than the observed air temperature at this site for short periods of time (e.g., mid-January), it is also warmer at other times (e.g., early March). As suspected by Andrew Slater, the WATCH forcing is indeed somewhat warmer than observed in the weeks preceding snowmelt, and the daytime temperature frequently climbs above zero degrees Celsius from early May onwards. However, the daily average temperature remains close to or below zero until the end of May, in accordance with the observations. We propose to include this plot in an updated version of Figure 7, possibly replacing panel (e) which at present does not include a comparison with observations. Note also that the two variants of WATCH (WATCH-GPCC and WATCH-CRU) differ only in their precipitation input, meaning that differences in soil temperature between the two runs are not due to differences in temperature forcing.

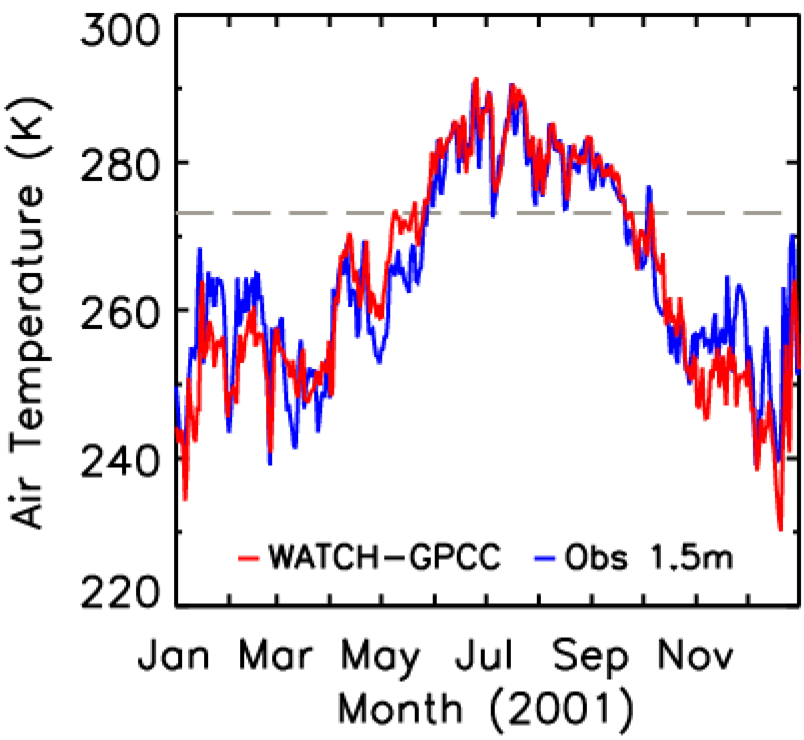

Figure: comparison between 2.0m temperature in the WATCH forcing data, and observed air temperature at 1.5m at Toolik, Alaska in 2001.

(3) It may be informative to include data from GSWP2 in Figures 3 and 4.

Response: The figure below shows the comparison with annual soil temperatures from the GSWP2 run. Fundamentally it shows a similar picture as our Figure 3. The bias appears somewhat less but note that the number of data points (years) is also considerably less. We propose to discuss this in more detail in the text.

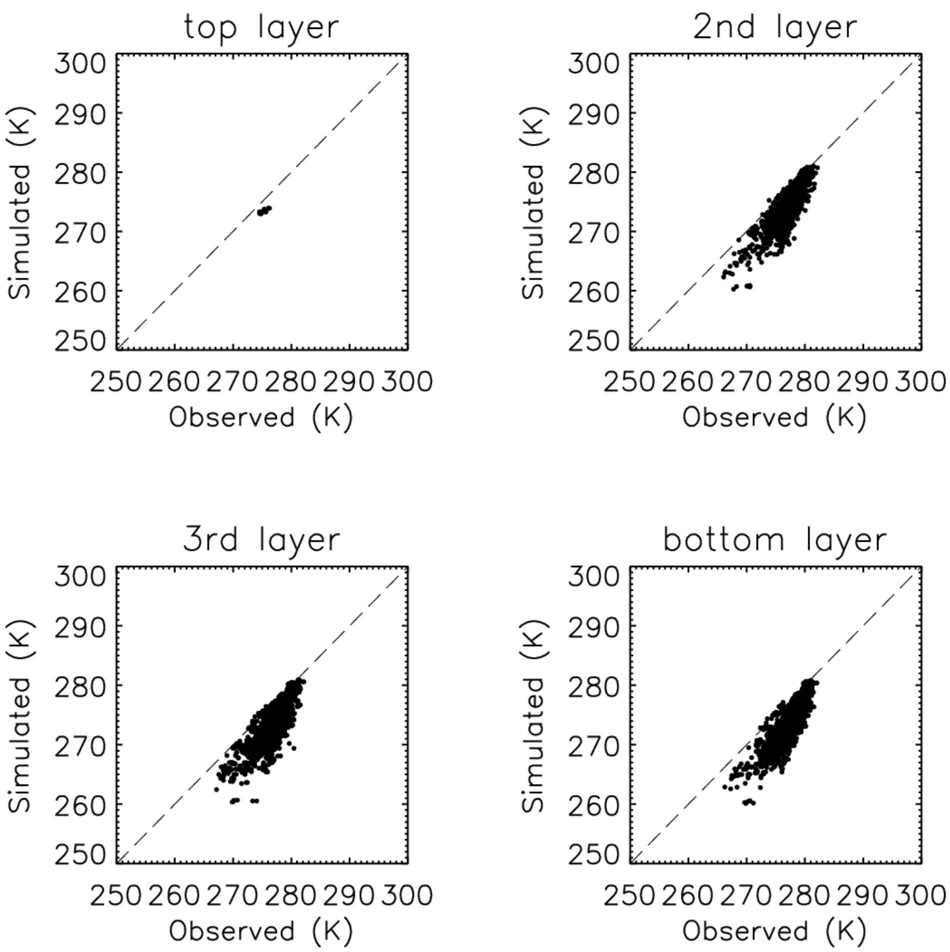

Figure: Simulated and observed mean annual soil temperatures at Russian meteorological stations, with modelled soil temperatures from the GSWP2 run (1983–1995)

(4) SSM/I data are not truly observations of snow water equivalent (SWE)

Response: we agree completely, and are of course aware of the limitations of the SSM/I data. We propose to emphasise this issue in the text. The suggestion of including the observed snow depth at Toolik (Oberbauer, 2003) is very useful. Averaged over 60 samples the observed snow depth at 2 May 2001 is 0.66 m with a standard deviation of 0.09 m. JULES, using a prognostic snow density, calculates a snow depth of 0.30 m (SWE = 115 mm). We propose to mention these values in the text.

(5) In the DEEP simulations was the hydrology active down to 60m?

Repsonse: yes. However, in permafrost regions these layers remain mostly frozen meaning that they are hydrologically not very "active". The differences in soil wetness in the upper layers are, on an annual basis at least, very small. When we performed the experiments, we did not report time series of thermal conductivity.

(6) Small signal of SOC in the area west of Hudson Bay.

Response: in the NCSCD database, there are no exceptionally high values of organic content in the area west of Hudson Bay in the top soil (0-30 cm). Since we are re-gridding the organic content to 1-degree, the values are furthermore averaged out over much larger grid cells (see also our response to Referee #2). As a consequence, the organic fraction is relatively low in this area, mostly not more than about 0.3-0.4 in the top soil, ~0.2-0.3 in the second layer, and ~0.4-0.5 in the third layer. In other words, the small signal is due to a combination of input data, re-gridding, and not accounting for sub-grid variability in soil properties in the model.

2 Comments by Referee #2

Referee #2 raised the following issues:

(1) Impacts of re-gridding the NCSCD to 1-degree.

Response: we believe this has quite an impact in the approach that we used in this paper. By re-gridding to 1-degree, sites with a high organic content are averaged out over a much larger area. As a consequence, the organic fraction (which in our approach determines the extent to which the final soil parameters are influenced by the organic parameter values) is nowhere higher than ~0.7, and averaged over the domain covered by the NCSCD more in the order of

0.3 (top layer). We agree with the reviewer that in this respect allowing for sub-grid variability in soil properties would be beneficial, but this is currently not yet implemented in JULES. Although this was mentioned in the discussion (p. 1286), we propose to emphasise this point further in the text.

(2) Use of grass as a surrogate for tundra

Response: at the validation sites in Alaska, we based our choice of vegetation and soil type on the available site descriptions. From the 5 vegetation types in JULES, C3 grass seemed the best choice to approximate the actual vegetation at these sites, in the absence of a true tundra plant functional type in the model. We performed an additional simulation for Toolik with shrubs as vegetation type instead of grass, but this had only a minor impact on the soil temperatures. Note that at the CALM sites (as shown in Figure 2) the simulations come from the northern hemisphere grid runs, i.e. the model vegetation varies between the sites, although it is unlikely to correspond with the actual vegetation type at all sites.

(3) A 1-degree resolution may be an impediment to simulating permafrost

Response: as mentioned above, we agree with the reviewer that allowing for sub-grid variability would be beneficial for simulating permafrost. It would presumably also lead to a larger impact of organic soils, and would allow for sub-grid variability in permafrost coverage in discontinuous and sporadic permafrost zones. We are not sure, though, that this can fully explain the cold bias we find in the simulated soil temperatures. In relatively complex models like JULES, there is always a trade-off to be made between the level of detail that can be included in representing the land surface, and the computation time it takes to run the model. This is especially important when used as a land surface scheme in climate model simulations.

References

Lawrence, D. M. and Slater A. G.: Incorporating organic soil into a global climate model. Clim. Dyn., 30, 145–160, doi:10.1007/s00382-007-0278-1, 2008.

Oberbauer, S.F.: Snow Depth Yearly Measurements at Toolik Station 1995-2001. Boulder, CO: National Center for Atmospheric Research, ARCSS Data Archive, 2003, available from: http://data.eol.ucar.edu/codiac/dss/id=106.ARCSS910