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The Cryosphere Discussions

# *Interactive comment on* "Simulation of permafrost and seasonal thaw depth in the JULES land surface scheme" *by* R. Dankers et al.

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This paper looks at the diagnosis of permafrost from the JULES model when forced by a couple of global scale data sets (GSWP and WATCH). It is a good straight forward analysis of the state of high latitude soils in JULES and there is not much to say as a reviewer. Making comparisons between models and available observations is a tough prospect – if one wanted to be difficult there are many instances where these comparisons are less than ideal. However, the authors have presented most of the data in a manor that provides the reader with an overall impression of how the model is functioning (i.e. what its strengths and weaknesses are) and is about as good as can be done within the limitations of the data. The paper is in pretty good shape and there is little that needs work. In short, this paper should be published but would hope



that the authors would consider the few matters I have listed below prior to publication – suggestions are mostly minor in nature.

* The statement "The properties of organic soil are based on Letts et al. (2000) and Oke (1987) and, in contrast to Lawrence and Slater (2008), were allowed to vary with depth" needs to be corrected. It is stated rather clearly in Lawrence and Slater (2008; Section 2.2) that the organic content varies with depth. Figure 1c of our paper shows sample profiles.

* Making detailed comparisons at a single point when forcing the model by reanalysis scale data is, in my opinion, somewhat uninformative without knowing the relative skill of the forcing at the observed point. For a 'big picture' perspective, using multiple locations, an overall bias or error may be discernible (e.g. your Figure 6 or Figure 2 despite its limitations), but the detailed comparison of soil temperatures at Toolik (Figure 7) is less useful. Air temperature data is available at all the SCAN sites – how does it compare to the WATCH forcing data? Obviously there are some considerable differences in energy at Toolik because the WATCH-GPCC data ablates snow several weeks early. A quick look at air temperature in 2001 at Toolik shows that 0°C is only achieved in very late May (consistent with the SSM/I snow removal date) – WATCH suggests there was enough energy to ablate 120mm of snow in early May. Conversely, WATCH data could have been much colder in mid-winter – I don't know. Hence, it would be good to plot the observed air temperature from the sites with those from the WATCH data set – Figure 7c and 7d essentially tell the same story, so one of them could be replaced.

Given the excessive permafrost area in your Figure 1c as well as Figures 3 and 4, WATCH data will produce a cold bias in the soil (probably dependent upon location). Hence a look at how it compares at the point specific sites (Toolik, Barrow etc) would be informative. Also, despite there being fewer soil temperature observations for the post-1982 period than from 1959, it would be (in my opinion) more useful to include data from the GSWP runs in Figures 3 & 4 and limit the time period to post-1982. The data used to drive JULES will have considerable influence upon its results.

Another item to remember is that there is no such thing as an “observation” of snow depth or snow water equivalent from a satellite. The SSM/I data are passive microwave brightness temperatures that are converted to an estimate of depth via an (often poorly performing) algorithm (i.e. a model!). The depth is then multiplied by an estimated density (a single value for the whole earth in the old Chang algorithm) to give a SWE value. Using the standard (Chang algorithm) SSM/I SWE or depth for “validation” is fraught with danger and in general shouldn’t be done. SSM/I data is useful for detecting if snow existed or not. As an alternative, I would suggest snow measurements at Toolik as of May 2nd 2001: (http://data.eol.ucar.edu/codiac/dss/id=106.ARCSS910). I will tell you that the mean snow depth was 66cm with a StdDev of 9cm and when converted into SWE (using 285 or 310 kg m-3 – see Sturm and Wagner, 2010) gives values of 188-205mm with a StdDev of 28mm. These values are above the SSM/I and model estimates.

Overall, the results suggest that JULES does not have the correct amount of seasonal damping in the permafrost regions – it’s too cold in winter and thaws too deep in summer. I am surprised that the addition of a SOC and DEEPer column had very little impact as both of these items should provide some damping. For the deep column simulations was the hydrology “active” down to 60m? If so, how does this change the soil moisture in the upper layers? I am reluctant to ask the authors to make additional fugures or carry out further experiments for the purpose of publication so will not do so, but I would be interested to know whether they have compared time series of thermal diffusivity for particular soil layers at specific locations where they would have expected a larger impact from SOC. For example, I would have expected a bigger signal in the area west of Hudson Bay where there are large deposits of organic rich soils – Figure 5a shows only a little change in this area. In any case, I look forward to seeing future progress in the JULES model on this difficult topic.

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Interactive comment on The Cryosphere Discuss., 5, 1263, 2011.