The Cryosphere Discuss., 7, C1515–C1522, 2013
www.the-cryosphere-discuss.net/7/C1515/2013/

The Cryosphere Discussions
Open Access

TCD
7, C1515–C1522, 2013

Interactive Comment

Interactive comment on "Evaluation of the snow regime in dynamic vegetation land surface models using field measurements" *by* E. Kantzas et al.

E. Kantzas et al.

e.kantzas@sheffield.ac.uk

Received and published: 22 August 2013

We would first like to thank the 2 reviewers for their comments which helped us identify weaknesses but also omissions in our submitted manuscript. Here we provide details on how we intend to strengthen and correct our study.

The 3 main concerns produced by the reviewing process were:

1) When the 4 models were compared against the snow transect measurements, they were driven them with 3 different climate drivers which could have diluted our inter-comparison conclusions. We will improve that by running 3 of them (SDGVM, LPJ-WM and JULES) with WATCH as a driver while, since we used off-the-shelf runs for CLM4CN (driven by CRU+NCEP), we intend to provide comparisons between WATCH

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

and CRU+NCEP.Even though we believe that our conclusions regarding the model inter-comparison will remain as presented in the original manuscript, we have already commenced the model runs.

2) The fresh snow density retrieval has been modified according to the recommendations of the anonymous reviewer to exclude forest transect measurements and the uncertainties they bring due to the heterogeneity of a shallow forest snowpack. Furthermore, we now filter the transect data and exclude days of very low temperatures. Thus the algorithm now produces values that suit a fresh snow density during the early parts of the snow season while the uncertainties of the regression analysis have significantly decreased.

3) Finally, we acknowledge our omission of not considering negative values of sublimation (condensation) in our original analysis as pointed out by the anonymous reviewer. The algorithm was completely revamped to account for condensation and our results offer a basis for model evaluation of the relative processes which we carry out.

We therefore request to be allowed to submit a revised version of our manuscript for re-evaluation. As these land surface models seek to add more sophisticated soil temperature, organic soils and permafrost processes, our analysis sheds light on implications linked to uncertainties in the simulations of various snow parameters.We now provide more detailed answers to the reviewer's specific comments and recommendations.

Reviewer #1: R.L.H Essery

Kantzas et al. present an interesting comparison between land surface models and field measurements of snow. My major concern is that the four different models are used with three different climate drivers, so there will be differences between the simulations that have nothing to do with differences between the models. Ideally, all of the models would use the same driving (the WATCH and CRU + NCEP datasets both contain all of the necessary variables), but at the very least the temperature and precipitation inputs need to be compared to see how they contribute to differences in snow simulations. A recent paper by Brun et al. has used the same field data and also considers simulations of snow density, sublimation and soil temperature; this paper is referred to, but needs to be more completely acknowledged.

»Reviewer's main concern is that the 4 models are driven with 3 climate drivers thus diluting the value of comparing them against the snow transect measurements. We are already correcting that by running JULES, SDGVM and LPJ-WM with the same driver (WATCH). As we obtained the CLM4CN data from an off-the-shelf run, we intend to provide comparison between its driver (CRU+NCEP) and WATCH.

L2334, 6 "the system's processes"

»Corrected.

L2334, 8 "across the extent"

»Corrected.

2334, 18 "the discussion section"

»Corrected.

L2335, 15 wouldn't regard a model which did not have coupled land, ocean and atmosphere components to be an "Earth system model".

»Replaced "Earth system models, either land, ocean, atmospheric or coupled,. . ." with "Land, ocean, atmospheric or coupled Earth models,. . ."

L2337, 2-5 "the number of stations in the dataset reduced" "the majority of them were located" "measurements were usually taken"

»Corrected.

L2337, 18 The utility of GlobSnow for testing land surface models has been examined by Hancock et al. (2013)

»Added.

TCD
7, C1515–C1522, 2013

L2338, 11 Snow albedo in CLM4 is based on the microphysical model of Flanner and Zender, but density is not.

»Removed.

L2339, 17 GlobSnow SWE data are provided at 25 km resolution.

»Corrected.

L2343, 20 Some studies have estimated much larger magnitudes for blowing snow sublimation and transport in continental northern latitudes, e.g. Pomeroy and Essery (1999).

»The sublimation section was considerably revised and the reference with its importance is now included.

L2347, 23 The fresh snow density and maximum density in CLASS were revised by Bartlett et al. (2006).

»Added the reference and how snow density has changed in CLASS 3.x but maintain in our algorithm the equation used in CLASS 2.x.

L2349, 14 “are also shown”

»Corrected.

L2349, 26 “This is consistent with”

»Corrected.

L2350, 24 “As data is too sparse”

»Corrected.

L2354, 11 Spurious behaviour in calibration has been demonstrated for this type of model by Kavetski and Kuczera (2007).

»We intend to provide a comment regarding this observation.

TCD
7, C1515–C1522, 2013

L2355, 3-11 “Similar effect will have . . .” – this sentence is mangled.

»To be rephrased.

“Both of these changes reduce”

»Corrected.

“As was demonstrated earlier”

»Corrected.

“readily-available optimization techniques”

»Corrected.

L2355, 16 “Interactions Soil-Biosphere-Atmosphere” Note that it was specifically the Crocus snow model that Brun et al. evaluated (ISBA has two less sophisticated snow model options)

»Corrected.

L2355, 24 “the extent of the FSU”

»Corrected.

2356, 26 “As shown”

»Corrected.

L2357, 14 Did anyone previously doubt the importance of systematic field measurements?

»To be rephrased.

L2357, 6 Dankers et al. used an unrealistically high value for fresh snow density in JULES, which gave too high a thermal conductivity for fresh snow. The new snow module in JULES has actually been found to substantially reduce cold soil temperature biases compared with the old version.

»We intend to provide a comment and rephrase this section.

L2357, 22 It is not correct to say that SWE and snow density have not been benchmarked in JULES. The snow component in JULES and the precursor MOSES model has been evaluated for many sites, including through participation in the GSWP2, PILPS2d, PILPS2e, RhoneAGG, SnowMIP, SnowMIP2 and WaterMIP intercomparisons.

»To be rephrased.

Reviewer #2:Anonymous

The 2 main results claimed by the authors both suffer from serious misconceptions which make them either irrelevant or wrong.

»Both of the algorithms mentioned by the reviewer (fresh snow density estimation and sublimation) were considerably revised especially the latter one which was completely revamped. Furthermore, these two algorithms are not, as stated by the reviewer, the main results of the paper as it mostly focuses on evaluation of snow water equivalent and snow density evolution of land surface models and the effects of their inaccuracies.

Density estimation The estimation of fresh snow density is based only on SWE observations recorded a few days after a period with no snow on the ground. It does not make sense for the following reasons: - the method samples only early or late season snowfalls and hence provides fresh snow density values which are absolutely not representative of the very cold conditions prevailing in these regions during mid-winter. The authors do not mention any possible issue related to this aspect.

»We have now enhanced the retrieval with climate data to tackle with this issue. As fresh snow density can be affected by extremely cold temperatures, our results now only take under consideration values that were collected when air temperature is in the range of -10C<T<0C thus providing an estimate expected to be found in the beginning of a snow season. This approach actually decreased the variance of the sample and consequently the range of the confidence interval.

- the method uses only SWE measurements from very shallow and light snowpacks not older than a few days. Consequently, the measurement itself should be very inaccurate since it is quite difficult to extract a very shallow snow core, especially in the forest where snow is very heterogeneous. The authors do not provide any estimation of the accuracy of such measurements.

»We now exclude forest transect measurement which actually accounted for just 2 measurements out of a total of 60.

- there are several hundreds thousands daily records of both precipitation and fresh snow depth available from NCDC which should be more appropriate for such a study (provided wind effects on rain gauges are taken into account), instead of only 60 pairs of measurements extracted from 600 000 SWE survey records.

»This algorithm, as the entire study, focuses of the Former Soviet Union. The NCDC daily records for the region are actually the transect measurements we are using.

Monthly sublimation estimation The sampling method retains only consecutive non-increasing values of SWE measurements. It undoubtedly leads to a wrong estimation of the sublimation for the following reasons: - the authors do not consider any cases where SWE is increasing between 2 measurements because of higher condensation than sublimation during the considered period! Hoar formation or riming is a common phenomenon during mid-winter which is neither considered in the method nor even mentioned in the results interpretation. - the authors do not consider any possible effect of bottom melting which cannot be excluded in the south-western part of Russia. - cases where SWE is decreasing in spite of light snowfalls (cases where sublimation would be higher than precipitation) are neither excluded nor mentioned. - even without these limitations, the method is very inaccurate since the sublimation/ condensation between 2 measurements (5 or 10 days apart) is probably much smaller than the SWE observation accuracy.

»We have revamped this algorithm, according to reviewers comments, which now also considers condensation,precipitation events and other ablation processes and invite the reviewer to provide comments.

References

Bartlett, P, M MacKay and D Verseghy, 2006. Modified snow algorithms in the Canadian Land Surface Scheme: model runs and sensitivity analysis at three boreal forest stands. Atmosphere-Ocean, 43, 207 – 222. Hancock, S, R Baxter, J Evans and B Huntley, 2013. Evaluating global snow water equivalent products for testing land surface models. Remote Sensing of Environment, 128, 107 – 117. Kavetski, D, and G Kuczera, 2007. Model smoothing strategies to remove microscale discontinuities and spurious secondary optima in objective functions in hydrological calibration. Water Resources Research, 43, W03411. Pomeroy, J, and R Essery, 1999. Turbulent fluxes during blowing snow: field tests of model sublimation predictions. Hydrological Processes, 13, 2963 – 2975.

Interactive comment on The Cryosphere Discuss., 7, 2333, 2013.