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Interactive comment on "Evaluation of North Eurasian snow-off dates in the ECHAM5.4 atmospheric GCM" *by* P. Räisänen et al.

P. Räisänen et al.

petri.raisanen@fmi.fi

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We thank Referee #2 for his/her constructive comments on the manuscript. Below, the referee comments are written in *italic* font, and our responses in normal font.

General Comments

This paper utilizes in situ snow course measurements and satellite passive microwave estimates of snow off date to evaluate the ECHAM4.5 atmospheric GCM. Because neither the in situ measurements, satellite data, nor model simulations provide direct values of snow off date, clear explanations and justifications are provided for the

derivation of snow off date from these three sources. A set of historical ECHAM4.5 sensitivity simulations were utilized to show the model response to nudged parameters related to atmospheric circulation, and changes to the parameterization of surface albedo in the model. In situ measurements from Sodankyla, Finland provide convincing evidence that early snow melt in the simulations, despite a cold temperature bias, are due to the failure to calculate the energy budget separately over snow-covered and snow-free fractions of the grid cell. Explanation for the regions with a late snow melt bias are somewhat less convincing, but the attribution to the lack of vegetation canopy shading in the model seems sound. I have a number of suggestions that will hopefully improve the final version of the manuscript.

Comment: *1. The introduction provides clear information on the background and context for this study, but some fundamental citations on simulated versus observed snow albedo feedbacks are missing. I suggest consideration of the following:*

Qu, X., and A. Hall. 2007. What controls the strength of snow-albedo feedback? Journal of Climate. 20: 3971-3981. DOI: 10.1175/JCLI4186.1

Qu, X., and A. Hall. 2014. On the persistent spread in snow-albedo feedback. Climate Dynamics. 42:69-81. DOI 10.1007/s00382-013-1774-0.

Fletcher, C., H. Zhao, P. Kushner, and R. Fernandes. 2012. Using models and satellite observations to evaluate the strength of snow albedo feedback. Journal of Geophysical Research. VOL. 117, D11117, doi:10.1029/2012JD017724.

Response: We will add a short paragraph on snow-albedo feedbacks in the Introduction in the revised manuscript. These references will be mentioned there.

Comment: *2. Page 3676 lines 26–27: "The ECHAM5 snow scheme considers both SWE intercepted by the canopy and SWE on the ground, the latter being more interesting for this study." Recent work with the Community Land Model has shown the importance of snow-canopy processes as a source of simulation error in snow albedo (http://onlinelibrary.wiley.com/doi/10.1002/2014JD021858/abstract). While the importance of these processes are certainly model dependent, the role of snow-vegetation interactions can be significant.*

Response: The reason for including this sentence in the original manuscript is that obviously, snow-off time depends *directly* on SWE on the ground only, so it is more relevant to describe the latter in detail. It was, however, not our intention to give the impression that snow-canopy processes are unimportant in general. To avoid this impression, we will delete the words "the latter being more interesting for the present study", and add a reference for the canopy snow scheme, should the reader be interested in its details:

Roesch, A., Wild, M., Gilgen, H., and Ohmura, A.: A new snow cover fraction parametrization for the ECHAM4 GCM, Clim. Dynam., 17, 933–946, 2001.

Comment: *3. Page 3678 line 6: what is the depth threshold for determining 100% snow cover in the model?*

Response: In fact, the snow cover never reaches 100%. The snow cover fraction is parameterized using a *tanh* function which approaches asymptotically 95% with increasing SWE, and also depends on the subgrid-scale standard deviation of surface elevation, as described in Roesch et al. (2001; reference provided above). This will be

discussed in the Section 2.1 of the revised manuscript.

Comment: *4. 1978–2006 covers the CMIP5 historical simulation time period. Rapid reductions in spring SCE, including northern Eurasia, has occurred between 2007 and 2012, as described in:*

Derksen, C., and R. Brown. 2012. Spring snow cover extent reductions in the 2008–2012 period exceeding climate model projections. Geophysical Research Letters. 39: L19504 doi:10.1029/2012GL053387

Are there any implications on the results of this study related to the 1979–2006 time period? Most CMIP5 models do not capture the observed spring snow reductions over the past 7 years, but the radiometer derived snow off dataset would allow evaluation of model performance during this recent period of rapid change. It is not necessary to add these years to the current paper, but some statements on this issue could be added to the Discussion.

Response: The extent of the model runs was determined by the availability of input and validation data at the time that the work related to this paper was started (which, unfortunately, was a few years ago). It is pertinent to point out that the simulation period excludes the years 2008–2012 with a rapid reduction in late spring snow cover, and this will be done in the revised manuscript. Other than that, we prefer not to speculate on this issue in the manuscript, (i) for brevity, (ii) because such a discussion would indeed be speculative, and (iii) because it seems very likely that including these years would not change the conclusions of the paper in any substantial way.

To expand a bit on this reply, it should be recalled that the paper deals with the evaluation of the *mean* snow-off date and related quantities over the period 1979–2006.

Extending the period to 2012 would most likely cause only small quantitative changes in the results. First, it would only increase the number of analyzed years from 28 to 34, so that the years 2007–2012 would have a weight factor of only 18% for the mean values. Second, the simplifications of the model physics, such as the rather unsatisfactory treatment of the surface energy budget in the presence of fractional snow cover, would probably have largely similar effects even for these years.

Of course, as Derksen et al. (2012) show that climate models in general fail to capture the rapid reduction in snow cover in 2008–2012, this could also be true of ECHAM5. In that case, ECHAM5's tendency towards too early snow-off would be less pronounced during these years than during 1979–2006. Even if it proved to be so, it may be asked how relevant this would be. While climate change is expected to result in reduced springtime snow cover and earlier snow-off, the observed acceleration of this trend might be, at least in part, a manifestation of internal climate variability. In general, climate simulations cannot be expected to match the observed internal variability.

Were this paper focused on trends in snow-off time, extending the period to 2012 would be of more interest. We opted to leave out the analysis of trends (i) to keep the length of the paper reasonable, and (ii) because uncertainties related to internal variability (both modelled and observed) would play a larger role than in the case of mean values for the whole period.

Comment: *5. This study utilizes a small number of model runs, 3 or 1 depending on the experiment. Was internal model variability with respect to snow parameters quantified at all? A small standard deviation in the 28 year mean snow off date from 3 model runs is used to justify the small number of members. But how does the model variability compare to the observed variability in snow off date? I suggest a panel be added to Figure 2 which shows the standard deviation in satellite derived snow off date as is provided in Figure 2d for the reference simulations.*

Response: As emphasized above, the focus of this paper is on the time-mean snow-off date averaged over 1979–2006. And, as noted in the referee comment, the effect of internal model variability on this is quantified by the standard deviation in Fig. 2d. The figure suggests that the 28-year mean value is a relatively robust quantity, even when derived from a single model run. We also looked at the differences in the mean annual cycle of SWE between the three runs in the REF simulation, with the same basic conclusion (figure not shown).

We will follow the Referee's suggestion and add the interannual standard deviation of snow-off date to Fig. 2 — not only for the satellite data but also for the REF simulation, since the interannual std. dev. is of course not comparable with the std. dev. of the 28-year mean values as shown in Fig. 2d. This shows that the interannual variation of snow-off date in ECHAM5 is similar to the observations, though with some regional differences.

Comment: *6. Page 3681 lines 5-11: I was confused by the terminology in this paragraph with respect to 'snow melt date' and 'snow off date'. 'Snow melt' is the onset of wet snow, which the radiometer measurements are very sensitive to. 'Snow off' date is the time when the land surface is free of snow, and occurs at some time lag after snow melt onset. The snow course data can be used to evaluate both of these terms in the radiometer dataset through the use of the snow status flag (for melt onset) or snow depth (snow off when snow depth = zero). It's not clear in this paragraph how the microwave snow off estimates were calibrated. It seems snow melt information was used for calibration but the microwave dataset also provides the snow off date. It's important to clarify this description since the in situ measurements, satellite data, and model simulations each provide indirect values of snow off date.*

Response: First, we note that on p. 3681, line 8 "snow-off date" should be used instead of "snowmelt date" (which in our opinion is an ambiguous term — it can refer to anything between the onset of snow melt and snow-off). This will be corrected in the revised manuscript.

Regarding the calibration of the microwave dataset, the terms “temporary melting” and “continuous melting” both refer to a situation where there is no snow left at the weather station to be measured, “continuous melting” indicating summer(!). While this terminology might not be the clearest possible, we prefer to use these terms to be consistent with the description of the microwave satellite algorithm in Takala et al. (2009; cited in the manuscript) and with the original documentation of the INTAS-SCCONE dataset. However, the meaning of these terms will be clarified in the revised manuscript, as follows: *Specifically, for the calibration data, the snow-off date was defined as the last event during spring when the station snow status flag changed from “snow depth is correct” to “temporary melting” or “continuous melting”, both of which refer to a situation in which there is no snow left at the station.*

Comment: *7. The potential differences in how the satellite radiometer and snow course datasets characterize 'snow off' is a source of uncertainty in the model evaluation. I suggest a plot be added which shows a comparison between the microwave and snow course derived snow off dates (i.e. as a scatter plot) for those grid cells where both datasets are available.*

Response: We will add a scatter plot showing the relation between snow-off date in the satellite and snow course datasets. To be most consistent with the model-to-observation comparisons in the paper, the scatter plot will be presented in terms of time-mean values for 1979–2006 (using only those years with available snow course data also for the satellite data), at T63 resolution. The comparison shows that on average, the snow-off date derived from the satellite data is 5 days later than that derived from the snow course data, although some of the grid cells feature substantially larger (positive and negative) differences.

Comment: *8. Figure 5 shows the differences in simulated versus satellite retrieved surface albedo. Is it possible to determine if these differences are driven by snow cover fraction biases or albedo parameterization uncertainties? I suggest adding panels to Figure 5 which show spatial patterns of snow extent or snow fraction bias in the model compared to an observational baseline.*

Response: There are numerous potential causes for the albedo differences, including the parameterization of snow albedo, snow cover fraction, vegetation effects, and inevitably, also observational inaccuracy. It would be quite difficult to disentangle comprehensively the role of these factors, but to shed some light on this issue, snow cover fraction and vegetation will be considered in the revised manuscript. (Please also see our response to the next comment).

Thus, we will add figure panels with snow cover fraction biases, along with related discussion, in the revised manuscript. It is of note, though, that the choice of an observational baseline for snow cover fraction is not a trivial issue. After considering a few alternatives, we chose to use the European Space Agency GlobSnow dataset, described in Metsämäki et al. (2015). The primary reason for choosing this dataset, rather than (e.g.) those described in Brown and Robinson (2011) or Zhao and Fernandes (2009), is that the fractional snow retrieval in GlobSnow uses the SCAmod method designed ecpecially to enable accurate snow mapping including forests, which cover a large part of the Northern Eurasia. A downside of GlobSnow is that (springtime) data is only available since 1997. However, this should not be a major issue for the comparison with the albedo biases in 1982–2006, because ECHAM5's albedo biases are similar from one year to another. In fact, the spatial correlation between albedo biases for 1997–2006 and 1982–2006 is $\approx$0.99 for March through May and $\approx$0.98 for June.

References:

Brown, R.D. and Robinson, D.A., Northern Hemisphere spring snow cover variability and change over 1922–2010 including an assessment of uncertainty, The Cryosphere, 5, 219–229, www.the-cryosphere.net/5/219/2011/, 2011

Metsämäki, S., Pulliainen, J., Salminen, M., Luojus, K., Wiesmann, A., Solberg, R., Böttcher, K., Hiltunen, M., Ripper, E., Introduction to GlobSnow Snow Extent products with considerations for accuracy assessment, Remote Sensing of Environment, 156, 96–108, doi: 10.1016/j.rse.2014.09.018., 2015

Zhao H. and Fernandes, R., Daily snow cover estimation from Advanced Very High Resolution Radiometer Polar Pathfinder data over Northern Hemisphere land surfaces during 1982—2004, J. Geophys. Res., 114, D05113, doi:10.1029/2008JD011272, 2009.

Comment: *9. Given the potentially important role of forest cover in this study, it would be helpful to provide an observationally derived forest classification and a dominant plant functional type map for ECHAM4.5 as extra panels in Figure 2.*

Response: It should be noted that ECHAM5.4 is run here without the land-biosphere module JSBACH (which is the default land cover scheme in ECHAM6 but not in ECHAM5.4). Thus, the description of vegetation is rather simplified. No plant functional type map is explicitly defined. Rather, the only relevant vegetation parameters for the current work are forest fraction and leaf area index. We will show these parameters, along with the forest fraction derived from the ESA Globcover 2009 dataset (http://due.esrin.esa.int/globcover/) in the revised manuscript. These will be

shown in a new figure in connection with the discussion of modelled vs. observed albedo, rather than as extra panels in Fig. 2.

Editorial Changes

Comment: *The term 'fields' is used throughout the paper to refer to non-forested areas. I suggest changing this to 'open' which better captures non-forested regions both above (i.e. tundra) and below the treeline.*

Response: The term “open-terrain” snow course will be used in the revised manuscript.

Comments:
Page 3687 line 14: change 'snow-off to occur' to 'snow-off occur'
Page 3689 line 11: change to 'The changes in snow-off timing ...'
Page 3690 line 23: change 'represented' to 'presented'

Response: Thanks for pointing out these. They will be corrected in the revised manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 7, 3671, 2014.