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Interactive comment on "Eurasian snow depth in long-term climate reanalyses" *by* Martin Wegmann et al.

Martin Wegmann et al.

martin.wegmann@univ-grenoble-alpes.fr

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The manuscript addresses an important topic, which fits very well to the scope of the journal. There has been a lot of uncertainty in the recent trends in Siberian snow cover in autumn, and the manuscript to some degree reduces this uncertainty, by showing that the observed trends strongly vary in space (Figure 2a). Moreover, interesting results are presented on the centennial time scale, showing major differences between the U.S. and European reanalyses until about 1940. The manuscript has, however, also weaknesses, and I suggest that major revisions should be made before publication.

Response: We thank the reviewer for his insight and for raising a few key questions regarding the way we describe and discuss the results. We present below a detailed reply.

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Discussion paper

Major comments: 1. A lot of results are presented on the performance of reanalyses in various months and regions, evaluated using various skill scores. The manuscript is, however, lacking analysis on the reasons for the better of worse performance of reanalyses. For example, major differences are found for the period 1901-1940 (Figures 3 and 4), and a reader is certainly interested in understanding the reasons for the differences. The differences can originate from (a) different data assimilated or different methods ap- plied in assimilation of the same data, (b) different model results for precipitation and its phase, (c) different model results for snow melt, and possibly (d) different parame- terizations (if any) applied for snow metamorphosis causing changes in snow density and, accordingly, thickness. The authors should pay at least some attention on these issues. If it is too difficult to find answers to issues (b) to (d), at least the snow schemes applied in the models should be compared. There may be major differences in the schemes for snow thermodynamics, which may explain the different results in early years when the role of data assimilation was probably smaller.

R : Thank you for pointing out some key elements. In the discussion part we investigate several options why the difference might occur, namely temperature and sea level pressure differences between the datasets. However, as you rightly pointed out, an outline of differences among the snow scheme was missing, which is added now at the end of section 2.1 . Moreover, we added Table 1, where it is more apparent what data assimilation is used and what boundary conditions are used. That said, we can dig only so far into technical details. Our investigations still show that assimilation and snow schemes are very similar, and we still support the idea of dynamical reasons for the changes in snow. We added a plot for vertical integrated mass of atmosphere, which points out a problem in ERA20C, namely to much high pressure over the Arctic in the first half of the 20th century. With this we hope to give enough initial ideas as to why the snow states diverge. Future studies need to check this feature in more detail.

2. The arguments for conclusions presented in Sections 5 and 6 are not clear. Why do you write in the beginning of Section 5 that the results indicate a good performance

of reanalyses (change “datasets” to “products”) and that climatologies are well represented? All figures presenting comparisons against observations include considerable errors, and Figure 3 only comparing different reanalyses includes huge differences. Also, most of the correlation coefficients presented are not “very high”. A correlation of 0.6 only explains 36% of the variance. If you consider the results good, did you have reasons (in addition to Khan et al. 2008) to expect worse results? Do you have arguments to set relevant thresholds for “good performance”?

R : Indeed, the wording here is not correct. We clarified the section and added arguments as why we see the performance as “good”

3. In general, the text is not particularly clearly written. See Minor comments below. Minor comments: Lines 31-34: unclear text

R : clarified

Line 51: alter . . . modulate

R : changed

Line 59: has severely impacted

R : changed

Line 60: “From 1979 to 2011” or “Between 1979 and 2011”

R : changed

Lines 62-63: I am not sure, if Park et al. (2013) also report regional snow cover increase associated with low sea ice concentration. The main message of their study is, however, the opposite, given by the title of the paper: “The role of declining Arctic sea ice in recent decreasing terrestrial Arctic snow depths”.

R : Indeed, they only report regional specifics. Deleted the citation at this point

Line 76: climate models

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R : changed

Lines 79-81: Global reanalyses have at least equally large spatial coverage as satellite products. So, the work “compromise” is perhaps not the best.

R : Clarified

Lines 85-86: not all reanalyses listed here extend further back in time.

R : Clarified

Line 98 and analogously in many other places: Brun et al. (2013)

R : corrected

Line 124: Medium-Range

R : corrected

Line 130: assimilating synoptic observations of atmospheric surface pressure

R : corrected

Line 144 delete “model”

R : deleted

Line 146: tell the resolution also in km.

R : We added resolution information in Table 1

Line 150: “follows exactly the CMIP5 proposal” is unclear

R : Not sure what is unclear at this point. Added explanation as to what is CMIP5.

Line 186: perhaps “exceeding”

R : changed

Lines 279-284: the text is unclear and appear contradicting. Be clearer to which seasons you refer to in the beginning. On lines 283-284 the ECMWF is considered excellent in 1901-1940, but in Figure 4 the ECMWF appear excellent only in 1901-1910 and 1980.

R : We tried to clarify that part. However note that each point represents a 30 year long climatology, which is shifted by 10 years from point to point rather than a 10 year long climatology.

Lines 419-421: Snow drift may indeed generate differences between observations and reanalysis products. In addition to resolution, however, the differences may simply originate from a lack of snow drift parameterization in the reanalysis snow scheme (see Major comment 1).

R : We added information about snow schemes in Section 2.1, see above.

Lines 427-428: The differences in input data should be quantified in Section 2.1.

R : Input data is now part of Table 1

Lines 438-443: The cause and consequence related to sea ice melt remains unclear. Without clarifying this, the processes at play in the pre-1950s sound very speculative.

R : We clarified this part. We do not want to tackle sea ice feedbacks here. This example should just be used as a dynamical reason as to why high pressure can lead to less snowfall.

Line 449: Why do you think that ERA20C is most probably much too warm in April?

R : Our best guess are dynamical reason, like temperature advection, due to pressure differences.

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