

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

### **Anonymous Referee #2**

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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.

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 or worse performance of reanalyses. For ex-

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ample, 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 applied 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 parameterizations (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.

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”?

3. In general, the text is not particularly clearly written. See Minor comments below.

Minor comments:

Lines 31-34: unclear text

Line 51: alter . . . modulate

Line 59: has severely impacted

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

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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”.

Line 76: climate models

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

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

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

Line 124: Medium-Range

Line 130: assimilating synoptic observations of atmospheric surface pressure

Line 144 delete “model”

Line 146: tell the resolution also in km.

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

Line 186: perhaps “exceeding”

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.

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).

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

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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.

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

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-253, 2016.

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