The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-254-AC2, 2020

The Cryosphere Discussions

Interactive comment on "Unprecedented atmospheric conditions (1948–2019) drive the 2019 exceptional melting season over the Greenland ice sheet" *by* Marco Tedesco and Xavier Fettweis

Marco Tedesco and Xavier Fettweis

cryocity@gmail.com

Received and published: 13 February 2020

Received and published: 22 January 2020 The authors show that summer 2019 was an exceptional melt season for Greenland, with record or near-record values in runoff, snowfall, and SMB. They find that summer 2019 was characterized by persistent anticyclonic conditions and melting was enhanced by melt-albedo feedback and warm air advection. Comparing 2019 to the extreme melt season of 2012, they show that although the two years have similar values of runoff and SMB, these exceptional conditions were driven by different atmospheric circulation patterns. This study provides

valuable insights into this latest Greenland melt season within the context of long-term trends. Overall, the analysis is thorough and well done, and the manuscript is well written. âĂŤ Main Comments âĂŤ In the SOM analysis, I'm concerned that the persistency of atmospheric patterns is defined somewhat arbitrarily and the exceptional persistency of 2019 may be overstated. In lines 246-249, 257-259, and Fig. 14c, the cumulative number of days for the top 5 most frequent classes of 2019 is compared to to the cumulative number of days of these same classes in other years and is found to be much higher. But this seems to be an inevitable result by construction, since the top 5 classes vary from year to year, so we're comparing 2019's top 5 classes to lesser ranked classes in other years. If these 5 classes were all adjacent on the SOM map, and represented some broader category of circulation pattern, then I could see how grouping them together is phys- ically meaningful for comparison across years. But in this case, the grouping seems artificial and perhaps not a very robust approach to comparing across years. For example, in Fig. 14c, the cumulative number of days for the 5 classes appears to be approximately 25 in 2012, much lower than the 55 days for these classes in 2019. However, we can see from Fig. 14b that 2012 had high persistency in atmospheric pat- terns as well, just with different classes than those in 2019. From Fig. 14b, I estimate the cumulative number of days for 2012's top 5 classes is approximately 47, which is pretty close in magnitude to the 55 days for 2019's top 5 classes. Thus, I would conclude that both 2012 and 2019 probably had high persistency, contrary to the analysis presented here. If you were to repeat the analysis but instead compute the cumulative number of days for each year based on the top 5 classes in that specific year, then how do the results change? Is the persistency of atmospheric conditions in 2019 as exceptional as stated? It would be interesting to see if both 2012 and 2019 stand out as exceptional in this approach.

R: thanks for the comment. We have added the following text at the end of the Results to highlight the similarities between the 2012 and 2019 summers: Nevertheless, we observe from Fig. 14b that 2012 also had high persistency in atmospheric patterns though such patterns belong to different classes than those in 2019. For example, the

cumulative number of days for 2012's top 5 classes is 47, being similar in magnitude to the 55 days for 2019's top 5 classes. Moreover, similarly to 2019, the top five classes in 2012 were all characterized by high GPH anomalies and strong anticyclonic conditions (though different in temrs of spatial distribution of the GPH anomalies). In this regard both 2012 and 2019 can be assumed to be exceptional from an atmospheric point of view. Concerning the other years: we performed the analysis suggested by the reviewer of looking at the top 5 (or even 4 or 6) patterns (in terms of frequency) and found that 2012 and 2019 weree the years with the highest occurrence of the top 5 patterns. Moreover, we looked at the top 5 nodes (e.g., patterns) for the remaining years and used the mean GBI (as well as the cumulative sum) to quantify the anticyclonic conditions and found that, also in this case, the top 5 patterns were not characterized by anticyclonic conditions as strong as the ones in 2012 and 2019. Said this, we do acknowledge that there is extra work that needs or can be carried out to identify "extreme" atmospheric patterns or their persistency and we plan to carry out a deeper analysis in our future work and are thankful to the reviewer for his/her suggestions.

âĂŤ Minor Comments âĂŤ L21-22 "the total number of days with the most frequent atmospheric pattern that characterized the summer of 2019 was 5 standard deviations above the 1981 – 2010 mean": This seems misleading, since the number of days referred to here is the cumulative total for the top 5 most frequent patterns of 2019, not the single most frequent pattern.

R: We changed the sentence as follows: The analysis of the frequency of daily 500 hPa geopotential heights obtained from artificial neural networks shows that the total number of days with the five most frequent atmospheric patterns that characterized the summer of 2019 was 5 standard deviations above the 1981 – 2010 mean, confirming the exceptional nature of the 2019 season over Greenland. L102-103 "When looking at the different summer months separately, the MI values in 2019 ranked 5th in June, 7th in July and 9th in August (Fig. 2b)": Fig. 2b only shows summer averages, not monthly values, so perhaps the reference to it should be removed?

R: this was removed. Thanks.

L109-110 "was also responsible for the cumulative 3-day melt event": Perhaps change "the cumulative..." to "a cumulative...", because this 3-day melt event hasn't been previously introduced. It would also be helpful to mention the specific dates of this event.

R: Done, thanks.

L112-118: Is this analysis of air mass trajectories all from the current study and details are not shown here? In particular regarding the 2012 summer, it's not clear if the discussion is summarizing earlier studies which should be cited here, or if it's referring to current analysis.

R: We have added a reference to the first sentence to clarify the fact that it referes to a published study. Thanks for the suggestion.

L167-168: The switch to positive albedo anomalies is confusing here, since the rest of the discussion centers around negative albedo anomalies. Also, looks like a typo here "In June, only 23%... was 23%."

R: Corrected, thanks.

L188-189: The text refers to "geopotential height anomalies", but the values listed (5567 m, etc.) seem to be actual geopotential heights, rather than anomalies.

R: thanks for catching this up. We have corrected the error. It should be simply "geopotential heights".

L197 "high pressure system centered near Summit over the whole 2019 summer (Fig. 5a)": I think Fig. 10b should be referenced here, rather than Fig. 5a.

R: That is correct. We have added the reference to the right figure. Thanks. L198: I think Fig. 5e should be referenced here, rather than Fig. 5d.

R: That is correct. Thanks.

L205-206: Should the area integrated anomalies be reported in units of W rather than W/m2?

R: We prefer to list our anomalies in W/m2 as using W only is very dependent of ice sheet mask/resolution used, which we want to avoid according to the recommendations of Fettweis et al. (2020): Fettweis, X., Hofer, S., Krebs-Kanzow, U., Amory, C., Aoki, T., Berends, C. J., Born, A., Box, J. E., Delhasse, A., Fujita, K., Gierz, P., Goelzer, H., Hanna, E., Hashimoto, A., Huybrechts, P., Kapsch, M.-L., King, M. D., Kittel, C., Lang, C., Langen, P. L., Lenaerts, J. T. M., Liston, G. E., Lohmann, G., Mernild, S. H., Mikolajewicz, U., Modali, K., Mottram, R. H., Niwano, M., Noël, B., Ryan, J. C., Smith, A., Streffing, J., Tedesco, M., van de Berg, W. J., van den Broeke, M., van de Wal, R. S. W., van Kampenhout, L., Wilton, D., Wouters, B., Ziemen, F., and Zolles, T.: GrSMBMIP: Intercomparison of the modelled 1980–2012 surface mass balance over the Greenland Ice sheet, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-321, in review, 2020.

L218 “We classify the daily 500 hPa GPH”: Should specify that it’s the GPH anomalies that are being classified.

R: we added “anomalies”here and in the successive sentence.

L220-221: This description of the training phase with “existing SOM nodes” seems a bit off. The SOM nodes are defined in an iterative process during training – they don’t exist prior to training.

R: we added more explanation on the fact that the nodes are initially random structure in which the data is allocated.

L218-221: Were the input data fields weighted to account for grid cell area variation at high latitudes (for example, as in Mioduszewski et al. 2016)?

R: Yes.

L229-230: Can you explain in more detail how the 4x7 SOM shape was selected?

Were any sensitivity tests performed to determine the impact of SOM size / aspect ratio and analyze error metrics?

R: As we mention in the text, the selection of the number of nodes and the architecture of the SOM does not have specific rules but mostly directions. A first aspect to be considered is the computational time, as the network will have to train a large number of points (e.g., daily data for the past 60 summers gridded over the Arctic). One aspect also to account for is the fact that the number of elements to be classified are roughly the same within each node. We based the selection of our architecture on these two considerations by testing several configurations and building on our experience published in Mioduszewski et al, 2016. Once the architecture was selected, we trained 50 different SOMs and compare their outputs to assess the effect of the random initialization on the 50 different runs. We found that the differences in classification for the multiple SOMs was less than 0.1 %. We also tested our configuration using ad-hoc prepared training datasets with know shapes and patterns and tested the selected configuration. Also in this case, the error in terms of classification was smaller than 0.1 %.

L263 “frequency and occurrence of the atmosphere”: What does this mean?

R: Thanks for pointing that out. We rewrote the sentence as follows: In the future, we plan to analyse how the frequency and occurrence of GPH anomalies has been changing at higher levels (e.g., 300 hPa, 100 hPa) to eventually quantify potential missing links between the stratosphere and the troposphere that might be responsible for the exceptional conditions.

âĂŤ Figures âĂŤ Figure 1b: The colour scheme is inconsistent with Fig. 1a, Fig. 3a, and Fig. 5a, which all use red for more melting days and blue for fewer melting days. I recommend reversing the colour scheme in Fig. 1b to be consistent with the others.

R: we have reversed the colorbar.

Figure 2b: The caption describes the blue line as “Summer-averaged melt extent”, but this is not a summer-averaged quantity, is it? It looks like the blue line shows, for each summer, the overall area subject to at least one day of melting.

R: that is correct. We have rewritten the caption.

Figure 5: Captions for subplots (b)-(e) are mixed up (i.e., (b) is snowfall anomaly but caption says 2m temperature, etc.). It would also be helpful to add a bit more horizontal space between subplots, so that there is some space between the colour bar labels and the y-axis of the right-adjacent subplot.

R: we corrected the caption and added the space.

Figure 9: The annotation reads “Melt extent reaches âĹij97%”, whereas the main text reports this value as 96%.

R: we corrected the main text for the right value of 97 %.

Figure 10a: In the caption “Anomaly of the JJA 2019 averaged geopotential height anomalies”, extra “anomaly” should be removed.

R: Done, thanks

Figure 11: Typo in the y-axis label: “yStandardized”.

R: Adjusted, thanks

Figure 13: The subplots are very tiny. Can these be enlarged? Also, 3-4 decimal places in the average geopotential heights seems excessive - they could be rounded to 0 or 1 decimal place in these annotations.

R: We have removed the decimal place and enlarged the maps.

Figure 14c: Caption and y-axis label describe this data as anomalies in the cumulative number of melting days, but the values shown aren’t anomalies. Also, are they the cumulative number of melting days, or just cumulative number of days (melting or not)?

R: thanks for this. We changed the caption accordingly. âĂŤ Typographic Corrections âĂŤ Punctuation / spacing typos: - L45 “i.e. ,Kohonen” - L76 “.eg. Fettweis” - L77 “2011); .”‘ - L90 “2015 ,2018;” L115 “relative cold” ==> “relatively cold” L259-260 “Despite similar in terms of runoff and SMB” ==> “Despite being similar. . .”

- R: Done for all, thanks !

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-254, 2019.