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Interactive comment on "Enhanced warming of seasonal cold extremes relative to the mean in the Northern Hemisphere extratropics" *by* Mia H. Gross et al.

Mia H. Gross et al.

mia.gross89@gmail.com

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*Note RC=reviewer comment; AR=author response

RC: Review of 'Enhanced warming of seasonal cold extremes relative to the mean in the Northern Hemisphere extratropics' by Mia H. Gross et al. The submission has the potential to make a significant contribution to the literature, but it is not quite there yet. For the most part the standard of English in the submission is quite good. However, there are some instances where the expression is awkward or that the meaning is unclear. It would be valuable for the authors to seek the help of a colleague who is proficient in written English when preparing their revision. This manuscript explores

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the extent to which our cold extremes are getting warmer, in both absolute terms and relative to the local environmental warming. Investigation makes use observations and also simulations of the future in 6 CMIP5 climate models (RCP8.5 scenario). A valuable part of the investigation is that the authors suggest the physical processes and drivers which lie behind the changes they document. Before acceptance could be recommended, there are a number of issues which need to be addressed.

AR: Many thanks to the reviewer for their very helpful comments and suggestions. We have addressed each comment and will be changing the manuscript accordingly. Some comments require substantial changes to the text and figures. We believe the changes strengthen the manuscript and the revised manuscript will make a significant contribution to the literature.

RC: Line 1: I think the word 'Enhanced' in the title is redundant (and potentially confusing). (I have a similar issue with this wording at numerous places in the manuscript (e.g., Lines 44-45, ...)

AR: Line 1: We can see how this word may not be the most appropriate word to use and have decided to replace 'enhanced' with 'amplified', both in the title and in several instances throughout the text (e.g. lines 44-45; line 51; line 66).

RC: Line 17: potential (sp.)

AR: Line 17: This will be corrected.

RC: Line 25: It is not clear to me that the paper uses a 'novel' approach. Lines 95, . . . indicate the authors are following earlier studies. Either justify this statement or remove the word here.

AR: Line 25: It is true that a similar approach has previously been used (e.g. Donat et al. 2017) in examining processes on the day in which the extreme occurs, however it has not been done for cold extremes as is done in this paper. Nevertheless, we will change the wording here to read "This study examines the day in which the extreme

occurs. . .”.

RC: Lines 54-60: With relevance to AA and the role of the northerlies in cold extremes it is worth referencing here the recent study of . . . Yuki Kanno, John E. Walsh, Muhammad R. Abdillah, Junpei Yamaguchi and Toshiki Iwasaki, 2019: Indicators and trends of polar cold airmass. Environmental Research Letters, 14, 025006, doi: 10.1088/1748-9326/aaf42b. They show that in the Arctic the loss of extremely cold air is happening at a faster rate than the loss of moderately cold air. Also reference Screen and co-authors, 2018: Polar climate change as manifest in atmospheric circulation. Curr. Clim. Change Reps., 4, 383-395 in this broader context.

AR: Lines 54-60: Thanks to the reviewer for the suggested references. We agree these references are certainly appropriate to reference the role of northerlies in cold extremes and Arctic amplification and adding these references helps to strengthen the argument put forward here.

RC: Line 63-64: Also include in this citation list the paper of Screen et al., 2014: Amplified mid-latitude planetary waves favour particular regional weather extremes. Nature Clim. Change, 4, 704-709.

AR: Line 63-64: Thanks to the reviewer again for the suggested reference which is indeed relevant to the text here.

RC: Line 88: How can we ‘improve’ future projections when we don’t know the future. Reword this phrase more appropriately.

AR: Line 88: Yes, this is a fair point. We will reword this to read “. . .may help to increase confidence in future projections of warming.”.

RC: Line 109-113: I am surprised that natural variability (as revealed in intra-model (or ensemble member) differences) is deemed to be small in this context of cold events. While the use of the first member of the six models is OK, the reader is entitled to some quantitative justification for this statement.

AR: Line 109-113: We will include a figure of the intra-model differences in supplementary material for reference. In the figure, the intra-model differences are small, suggesting that natural variability is relatively small for cold extremes relative to mean temperature changes.

RC: Line 148-150: Please write this equation in more conventional form, and note that 'advection' should have a negative on the right side of the equation. Also, the associated text needs to be expressed better.

AR: Line 148-150: The equation for temperature advection will be amended as $\partial T/\partial t = -(u\ (\partial T/\partial x) + v\ (\partial T/\partial y))$ and the associated text will similarly be amended to describe this better i.e. "where $\partial T/\partial t$ is the horizontal temperature advection in °C/s, u and v are the zonal and meridional wind components (uas and vas, respectively), and $\partial T/\partial x$ and $\partial T/\partial y$ are the temperature gradients in the zonal and meridional direction."

RC: Lines 160-163: I can understand a three-day average prior to the day the cold extreme occurs is employed here as it can be seen as representing the cumulative (in time) effect of the relevant processes. However it is not clear to me why it is applied just to Tadv, and not to snow, albedo etc. The influence of these last would also be imagined as relevant in the days leading up to the event (rather than just consider synchronous conditions). Some extra rationalisation/explanation is warranted here.

AR: Line 160-163: We agree with the reviewer that it might be odd that we only applied the 3-day averaging to Tadv. We did look at the 3-day average for snow and albedo and found that there was no real difference in the result (compared to looking at the variable on the day the extreme occurs). In addition, our reasoning was that the radiative effects would be more important on the day in question, although we acknowledge that there could be some cumulative effects in certain weather conditions. However, given that the results are so similar between the 3-day averaging prior to the day and results on the day of the extreme itself, we prefer to keep the results as they are in the paper, but will add further explanation of why the Tadv and other variables are treated slightly

differently (at line 164): “Days leading up to the cold event were also examined for snow cover and albedo, but results showed no clear difference compared to using values on the exact day of the event. In addition, the radiative effects influencing surface temperature would be more relevant on the day the extreme occurs. For these reasons, we show results of snow cover and albedo on the day the extreme occurs.”

RC: Line 184-208: This section should be rethought. The pattern correlation coefficients of models with HadGHCND are quite small. At the very least it should be established whether these r values differ significantly from zero (field significance) when appropriate allowance is made for spatial autocorrelation. Need to convince the reader that we are not just looking at noise here. See, for example, LIVEZEY, R.E. & CHEN, W.Y. 1983. Statistical field significance and its determination by Monte Carlo techniques. Monthly Weather Review, 111, 46-59, doi: 10.1175/1520- 0493(1983)1112.0.CO;2. Wang X, Shen SS (1999) Estimation of spatial degrees of freedom of a climate field. J. Climate 12: 1280-1291 doi: 10.1175/1520-0442(1999)0122.0.co;2. Bretherton CS, Widmann M, Dymnikov VP, Wallace JM, Bladé I (1999) The effective number of spatial degrees of freedom of a time-varying field. J. Climate 12: 1990-2009. Also in this section one must be careful of making a posteriori judgements of the geographical locations where the models seem to be in concert and might be of use (in random data such regions can always be found). Any argument for specific regions should be backed up by some physical reasoning. A related issue is the whether these models have proved their worth (or otherwise) in present climate to be trusted to examine cold extremes under future scenarios.

AR: Lines 184-208 (Section 3.1): We agree this section needs to be rethought. We have since decided that the pattern correlation coefficients shown in the figures are not an adequate method of showing the similarities/differences between the individual models and HadGHCND, because in these figures, we want to know regions where models agree/disagree, rather than a regional average that the pattern correlations infer. We have decided to show the multi-model mean instead of individual figures with

conditional stippling added to show agreement between models as well as with observations. This also helps to keep figures more concise and reduces the overall number of figures in the paper (though we will include individual model results in supplementary material). In the amended figure 1, agreement/disagreement is much clearer allowing a more logical flow of this section into the next, discussing future changes. This section will be re-written to first discuss observational results, then the regional similarities/differences between the models and observations. This then leads logically into the next section, future projections, where we discuss those regions where there is model agreement (similarly now showing the multi-model mean rather than individual models).

RC: Lines 238-: I think it is confusing to speak of cold air temperature advection (ll. 238, 247-266, 359, 385, . . .) and cold air advection (ll. 265, caption of Fig. 7, . . .). Referring to just 'temperature advection' (its sign, magnitude etc.) makes the argument much simpler. As a more general (and serious?) comment here, I am a little confused by this Section and what Fig. 7 is actually telling us. At line 243 the authors use the word 'actual' for the first time. I first thought this meant, in this case, the changes in the climatological T advection (left panels in Fig. 7). The right panels show 'excess changes' (where 'the difference between changes in the seasonal minima and changes in the seasonal mean is then calculated, hereafter referred to as "excess changes" ' (lines 135-7). My interpretation seemed to be borne out when the authors stated, in connection with the similarity of the left panels and right panels in Fig. 7, that this . . . 'suggests the changes are related to a change in the overall mean state of cold air temperature advection, rather than changes associated with the days directly prior to the day the cold extreme occurs'. However, if this last statement is true one would have expected the 'excess changes' would be close to zero. All this might be just associated with their choice of words. It is important that this issue is addressed and clarified, as the temperature advection argument is central to the paper.

AR: Lines 238-267: We understand the reviewer's confusion here and have decided to

use ‘negative temperature advection’ throughout, with a sentence added to the Methods section describing what is meant by this (line 151 – “We refer to cold air temperature advection hereafter as ‘negative temperature advection’”). The term ‘actual’ refers to the average of only values of temperature advection on the 3 days prior to the cold extreme, as opposed to the “excess Tadv” (Tadv on extreme days – mean Tadv). We will add a sentence to the Methods section to explain that ‘actual’ refers to this for simplicity at line 174 – “For simplicity, we use the term ‘actual values’ to refer to the seasonal mean calculated from values taken on the day the cold extreme occurs.” This indeed required clarification, thanks to the reviewer for pointing this out.

RC: Line 254 (and Fig. 7): I have a little trouble understanding the units here. They are stated as degC, whereas the units of T advection are degC/sec. Please clarify.

AR: Line 254, Fig. 7: Yes, the reviewer is correct, the units are degC/sec. We will correct this in text and figures.

RC: Lines 365-380: Some useful discussion pertaining to the complexities is presented here. Thru here worth reminding the reader of the considerable regions on NEGATIVE excesses over parts of Eurasia (Figs. 1 & 4) and the resemblance of these to the ‘Warm Artic-Cold Eurasia’ pattern. This warrants some extra comment and reference to recent work of Overland and co-authors (2019) Weakened potential vorticity barrier linked to recent winter Arctic sea ice loss and midlatitude cold extremes. J. Climate, 32(14), 4235-4261 and Luo et al. – 2019: The winter midlatitude-Arctic interaction: Effects of North Atlantic SST and high-latitude blocking on Arctic sea ice and Eurasian cooling. Clim. Dyn., 52(5-6), 2981-3004.

AR: Lines 365-380: Thank you to the referee for this very useful suggestion. We agree that this section warrants some extra discussion on the negative excesses in some regions and the ‘Warm Arctic-Cold Eurasia’ pattern, and in addition, the suggested references are relevant to the current discussion here and will be cited appropriately. Some extra discussion will be added at line 380 as follows: “There are still small re-

gions which show negative excess changes in Eurasia, such as in central-eastern Asia and northern parts of Siberia. This is consistent with the 'warm Arctic, cold Eurasia' pattern and relates to a substantial decline in sea ice concentration in the Barents-Kara seas and high-latitude blocking associated with a positive phase of the North Atlantic Oscillation (Luo and Chen, 2019; Luo et al., 2019)."

RC: Line 418: Would make more sense to join this paragraph up with the one preceding it.

AR: Line 418: Agreed, we will join this paragraph with the one preceding it.

RC: Line 439-443: Much of the analysis and discussion in the paper is focused (appropriately) on the role of AA in inducing these changes in our hemisphere. Notably absent in this exploration is the role that increased moisture load (and hence enhanced downward longwave radiation) plays in AA and its broader consequences. At the very least this should be mentioned and reference made to the investigations of Lee, Feldstein et al., 2017: Revisiting the cause of the 1989-2009 Arctic surface warming using the surface energy budget: Downward infrared radiation dominates the surface fluxes. Geophys. Res. Lett., 44, 10,654–10,661 AND Luo and co-authors (2017) Atmospheric circulation patterns which promote winter Arctic sea ice decline. Env. Res. Lett. 12, 054017, doi: 10.1088/1748-9326/aa69d0.

AR: Line 439-443: We will add a brief discussion here on other potential driving factors such as enhanced downward longwave radiation from increased moisture. Thank you for the suggestion. We did look into each of the surface radiation fluxes as well as net radiation, and found that downward longwave radiation was amplified on the day of the cold extreme, as well as projected increases in the seasonal mean downward longwave radiation. We will add some extra discussion at line 441 as follows: "... for example, increased moisture load and associated enhanced downward longwave radiation have been shown to play an important role in Arctic amplification (Lee et al., 2017; Luo et al., 2017)."

RC: Lines 444-446: Important to make clear here that the '1.7 days' referred to here was for the period 1954 to 2007. Also, the Stine paper is now quite old. Please to update this by citing new techniques (and measures of uncertainty). E.g., Qimin Deng, and Zuntao Fu, 2019: Comparison of methods for extracting annual cycle with changing amplitude in climate series. Climate Dynamics, 52, 5059-5070, doi: 10.1007/s00382-018-4432-8. Qimin Deng, Da Nian and Z. Fu, 2018: The impact of inter-annual variability of annual cycle on long-term persistence of surface air temperature in long historical records. Climate Dynamics, 50, 1091-1100, doi: 10.1007/s00382-017-3662-5.

AR: Lines 444-446: Thanks to the reviewer for pointing this out and for the suggested reference. We will clarify that the reference period for the '1.7 days' referred to in the manuscript is 1954 to 2007. We will update this with the suggested references which are more recent and useful for the discussion here.

RC: Lines 449-50: Reinforce and complement this comment on the shifts associated with the NAM by pointing to the paper of Luo, Dai, et al., 2017: Winter Eurasian cooling linked with the Atlantic Multidecadal Oscillation. Env. Res. Lett., 12, 125002, doi: 10.1088/1748-9326/aa8de8.

AR: Lines 449-450: Thanks again for the suggested reference and NAM information. This is indeed relevant to the discussion here and this will be amended.

RC: Lines 497-500: Please making minor corrections to author list . . . Chapin, F. S., III, M. Sturm, M. C. Serreze, J. P. McFadden, J. R. Key, A. H. Lloyd, A. D. McGuire, T. S. Rupp, A. H. Lynch, J. P. Schimel, J. Beringer, W. L. Chapman, H. E. Epstein, E. S. Euskirchen, L. D. Hinzman, G. Jia, C.-L. Ping, K. D. Tape, C. D. C. Thompson, D. A. Walker and J. M. Welker, 2005: Role of land-surface changes in Arctic summer warming. Science, 310, 657-660, doi: 10.1126/science.1117368.

AR: Lines 497-500: Thanks to the reviewer for pointing this out. We have gone through the author list and have made necessary changes to this reference: Chapin F. S. III,

Sturm, M., Serreze, M. C., McFadden, J. P., Key, J. R., Lloyd, A. H., McGuire, A. D., Rupp, T. S., Lynch, A. H., Schimel, J. P., Beringer, J., Chapman, W. L., Epstein, H. E., Euskirchen, E. S., Hinzman, L. D., Jia, G., Ping, C.-L., Tape, K. D., Thompson, C. D. C., Walker, D. A., and Welker, J. M.: Role of land-surface changes in Arctic summer warming, Science, 310, 657–660, doi: 10.1126/science.1117368, 2005.

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