Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2019-36-RC1, 2019

Earth System Dynamics Discussions

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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.

Anonymous Referee #1

Received and published: 19 August 2019

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 the extent to which our cold extremes are getting warmer,

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Discussion paper

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.

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, ...)

Line 17: potential (sp.)

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.

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.

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.

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

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.

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.

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.

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)111<0046:SFSAID>2.0.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)012<1280:eosdof>2.0.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.

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.

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.

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.

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

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.

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.

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.

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.

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