Weather Clim. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/wcd-2022-19-RC2, 2022

Comment on wcd-2022-19

Anonymous Referee #2

Referee comment on "Trends in the tropospheric general circulation from 1979 to 2022" by Adrian J. Simmons, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-19-RC2, 2022

This is an interesting paper which is more in the forma of a somewhat-descriptive survey rather than necessarily developing new physical insights. It can be seen as an update on, or complementary to, a few aspects of the recently-released IPCC/UNEP Working Group I AR6 report. It has some focus on the comparison of two modern reanalysis sets (and makes remarks as to where and, in some instances why, these reanalysis products differ.

The manuscript has potential to be an important contribution to the literature. However, it requires revision as outlined below.

As an overarching comment on the paper some of the explanations resented are based on the basic dynamics and thermo-dynamics of the atmosphere. For example, from line 36 comment is made … 'Changes in upper-tropospheric winds are linked to changes in surface flow and horizontal temperature gradients through the tendency of the atmosphere to remain close to thermal-wind balance. Where hydrostatic and geostrophic balance apply, the vertical shear of the wind is proportional to the temperature gradient across the direction of flow. The proportionality factor is larger at low than high latitudes'

Even though such statements are true, they don’t really belong is a ‘scientific’ paper. In many cases these remarks are obvious and the implications will be clear to the reader. I strongly suggest culling such remarks; this will make for a better paper, and also makes a valuable contribution to reducing the length of quite a long submission.

Lines 30-32: Reinforce this message by referencing the more recent paper of Screen, Bracegirdle, and co-authors (2018), Polar climate change as manifest in atmospheric circulation, Curr. Clim. Change Reps., 4, 383-395, doi: 10.1007/s40641-018-0111-4.

Lines 36-43: The remarks made a few lines earlier I the paper point to the compelc association and interactions between the thermodynamics and dynamics. In these introductory comments it would be very helpful to refer to the paper of

Theodore G. Shepherd, 2014: Atmospheric circulation as a source of uncertainty in climate change projections. Nature Geoscience, 7, 703-708, doi: 10.1038/ngeo2253

and his insights of ‘…

‘nearly everything we have any confidence in when it comes to climate change is related to global patterns of surface temperature, which are primarily controlled by thermodynamics. In contrast, we have much less confidence in atmospheric circulation aspects of climate change, which are primarily controlled by dynamics and exert a strong control on regional climate’

Lines 92-94: For easy reading, it would be beneficial if these trend values and corresponding p values were presented as inserts into the four boxes within Figure 1.

Line 100, caption of Figure 1: Even though it is perhaps obvious, it would be worth alerting the reader here that the y-axis ranges are different in the four time series. (This is mentioned in the text at lines 108-109, but should be emphasised here.)

Lines 117-120: Showing the (ERA5) trends that are significant (in part (b)) is interesting and helpful. It, of course, shows the same structure as in Figure 2a over the non-white areas. I appreciate that the author is showing these two plots (with much common information) for clarity. However, I'm wondering if the same purpose could not be achieved by presenting these two in ONE part of the Figure. For example, some subtle stippling could be added to Figure 2a indicating where the trends are NOT significant. This could save showing one map, and perhaps would be easy to absorb the information.

Incidentally, I presume that by 'one-signed' the author means 'one-sided' – please use this more conventional terminology. Also, a justification is required by using such a test, given that there are regions of cooling over the globe. More appropriate to use the two-sided test?

Line 158: 'SST' has already been defined (at line 142).

Lines 181-182: On this S/N issue worthwhile to reference recent paper of

Luke J. Harrington, 2021: Temperature emergence at decision-relevant scales. Environmental Research Letters, 16, 094018, doi: 10.1088/1748-9326/ac19dc.

Lines 209-211: This significant winter cooling over Eurasia and the northeast of the United States of America are important regional aspects of the complexity (and the consequences) of remote influences on the T2 trends. This warrants more attention than is presented here. Strongly suggest, for example, pointing out the role of teleconnections from the Arctic, high latitude blocking, Pacific SSTs etc. Making Reference to following will help on this:

Overland et al., 2019: Weakened potential vorticity barrier linked to recent winter Arctic sea ice loss and midlatitude cold extremes. J. Climate, 32, 4235-4261,

Luo, Xiao, and co-authors, 2016 - Impact of Ural blocking on winter Warm Arctic–Cold Eurasian anomalies. Part I: Blocking-induced amplification. J. Climate, 29, 3925-3947,

Dai, A. et al., 2020: 'Combined influences on North American winter air temperature variability from North Pacific blocking and the North Atlantic Oscillation: Subseasonal and interannual time scales'. *J. Climate*, **33**, 7101-7123, doi: 10.1175/JCLI-D-19-0327.1,

Rudeva, and coauthors, 2021. "Midlatitude winter extreme temperature events and connections with anomalies in the Arctic and tropics". *J. Climate*, **34**, 3733-3749.

Lines 265-270: Paper should make clear the physical/dynamic reasons why the near-tropopause winds are of great relevance here. Also, to make clear in the text here that Figure 6 is associated with the 200 hPa wind – the reader is finally told this at line 295, which is a bit late.

Lines 274-275: On this jet perspective consider citing the more recent works of …

Dong B, Sutton RT, Shaffrey L, Harvey B (2022) Recent decadal weakening of the summer Eurasian westerly jet attributable to anthropogenic aerosol emissions. Nature Comms. 13: 1148 doi: 10.1038/s41467-022-28816-5,

Hallam S, Josey SA, McCarthy GD, Hirschi JJM (2022) A regional (land–ocean) comparison of the seasonal to decadal variability of the Northern Hemisphere jet stream 1871–2011. Climate Dyn. doi: 10.1007/s00382-022-06185-5, Liu X, Grise KM, Schmidt DF, Davis RE (2021) Regional Characteristics of Variability in the Northern Hemisphere Wintertime Polar Front Jet and Subtropical Jet in Observations and CMIP6 Models. J. Geophys. Res. 126: e2021JD034876 doi: 10.1029/2021JD034876.

Line 301: The term ‘variation’ is used thru the manuscript to mean different things, from the qualitative concept and also to the mathematical variance. This can become a little confusing for the reader. Here the author is referring to ‘… the total sub-seasonal variation, the sum of the variances of the zonal and meridional wind’. This is clearly defined, but it should be given a more precise and informative name, such as ‘summed variance’. In general use ‘variance’ when the statistical concept is being examined.

Lines 380-384: It would be helpful here (and other places where relevant) to relate these WC changes to the geographical distribution of tropical SST changes. This need not be comprehensive, and a few words will probably suffice. Worthwhile to not merely present a description of the changes, but also link them physically to other (driving?) processes.

Line 432: I am a bit confused by the use of the word ‘nominal’ here. Reference to Webster’s did not help me greatly (e.g., ‘existing or being something in name or form only’, ‘of, being, or relating to a designated or theoretical size that may vary from the actual’, …). Please to use more conventional terminology here and below (using, e.g., ‘de-trended’, ‘anomalies’, …)

Line 686: Just confirming the paper is referring to ‘surface pressure’ rather than ‘sea-level pressure’? If so, comment on why this choice was made.

Clearly space considerations have prevented the author from showing the SEASONAL trends in surface pressure, but the annual mean hides a lot of interesting seasonal behavior. To highlight this make reference in the paper to the study of Li et al. (2021 - Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity, Ann. NY Acad. Sci., 1504, 167-186, doi: 10.1111/nyas.14673) who show that the strong midlatitude N Pacific increases are dominated by the DJF trends, while the annual deepening in the Amundsen-Bellingshausen Seas are predominantly due to large reductions in the intermediate seasons.