Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2022-19

Theodore Shepherd (Referee)

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-RC1, 2022

This paper is a very detailed and comprehensive analysis of the long-term trends in a number of variables representing the tropospheric general circulation over the period of the ERA5 reanalysis, 1979-2022. With over a 40-year timeseries, these trends are now long enough to become interesting in themselves. It is increasingly recognized that for documenting long-term changes in the tropospheric general circulation, direct observations tend to be problematical because of sampling and representativeness issues, and reanalyses ultimately provide the best way of obtaining a reliable record of the changes. It is of course necessary to be vigilant for potential sources of temporal inhomogeneity, as is exemplified in the present work which continually reminds the reader that nothing should be taken for granted. The uncertainty quantification presented here, which is a combination of standard statistical significance (essentially, a measure of the signal-to-noise ratio) to capture the effects of natural variability, and detailed comparisons (e.g. with other reanalyses, or with monitoring of the background fields) to assess potential systematic uncertainties, is interpretable and transparent, and far preferable to the oft-heard but meaningless mantra of a “full uncertainty budget”.

The paper is very descriptive but the overall philosophy is to carefully document what the data show, which I very much respect, and which will be useful for the community. It can be unhelpful to try to create a narrative where one does not really exist (the real world can be like that). Indeed, the paper questions some of the current narratives that exist around circulation changes, and raises some new questions to be explored. Both will be useful.

I can therefore recommend publication of the paper in essentially its present form.

My one scientific comment concerns the standard narrative of the poleward shift of the SH summertime (DJF) jet. The author criticizes its representation as a changing polarity of

the SAM. In a similar vein, Byrne et al. (2017, doi: 10.1175/JCLI-D-17-0097.1) argue that the phenomenon is more usefully seen as a delay in the seasonal equatorward shift of the SH jet, induced by the delayed breakdown of the stratospheric polar vortex. That argument seems to me to be a temporal analogue of the argument made here concerning the latitudinal shift of the NH eastern Pacific jet, with time in place of longitude. The author might wish to make that connection, if he agrees with it, since it seems part of a more general point that looking at circulation changes from too local a perspective (either in seasonality, or in longitude) can miss the bigger picture.

Minor points:

Fig 1: Why is the black dot in panels a and b located at 2010 rather than 2005?

Fig 7 and discussion in text. Shouldn't it be PV=-2 in the SH? And what happens at the equator, when PV changes sign?

Fig 10 and Table 3: I'm not sure what is the logic for discussing the annual-mean changes in upper tropospheric wind speed in such detail, when as Fig 11 makes clear, there is quite a seasonality to the changes.