Weather Clim. Dynam. Discuss., author comment AC2
https://doi.org/10.5194/wcd-2022-19-AC2, 2022

Reply on RC1

Adrian Simmons

Author 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-AC2, 2022

I thank the reviewer for both his general comments on the paper and the specific issues he raises. I am of course pleased that he recommends publication of the paper in essentially its present form.

As regards his specific scientific comment, I am grateful to be reminded of the paper by Byrne et al. (2017). It is indeed relevant to the discussion of the SAM given in the concluding discussion of the present paper, and I have drafted an additional paragraph to follow the existing one on the SAM.

Concerning the three points:

(i) I apologise for the error in the placing of the black dots in panels (a) and (b) of Figure 1. The original version of this figure was produced by computer code that had placed the dots in the right position. In a final revision of the paper before submission I considered the dots to be too small. I decided to increase their size by manually editing the postscript graphics file, and in panels (a) and (b) but not (c) and (d) I inadvertently increased the separation between the dots as well as their size. As it happens, Figure 1 has since been remade to include 120-month running means (as has Figure 9). This is one of the changes I mention in https://doi.org/10.5194/wcd-2022-19-AC1. The dots are correctly positioned, and of the larger size, in this new version of the figure.

(ii) The reviewer is of course right: I was careless in referring to the PV=2 surface rather than the |PV|=2 surface. I have corrected this. The reviewer asks what happens at the equator. For the formal record I note that a link to the documentation of the ECMWF forecasting system used to produce ERA5 is provided in the Hersbach et al. (2020) paper to which reference is made in the present paper. But to answer the reviewer directly, the calculations of variables on the |PV|=2 surface are made on the model's Gaussian grid, which straddles the equator. Values are then fitted using spherical harmonics, from which values anywhere on the globe can be derived. This by itself does not avoid problems where PV is small through most if not all the atmospheric column, so the pressure of the nominal |PV|=2 surface to which variables are interpolated is not allowed to be lower than 89hPa. I have revised the text of the paper to provide the latter information, and took the opportunity to quote the average pressure and trend of the |PV|=2 surface. The trend is for a modest but statistically significant decrease in pressure (increase in tropopause height) over time of about 0.7hPa per decade. The global- and time-mean is 191hPa, close to the level of 200hPa for which the majority of results in the paper are presented.

(iii) I prefer to retain Figure 10 and Table 3. Yes, as the reviewer points out there is quite a seasonality to the changes, which is apparent from Figure 11 and its discussion. But there is also a fair deal of cancellation from one season to another, especially in the extratropical southern hemisphere, and it is difficult to assess quantitative aspects of this from Figure 11 alone. I think it is important to show that the net change over the years in jet-stream positions is quite small in many regions. Summaries such as presented in AR6 that mention only the largest seasonal changes, for which confidence is high, may give a misleading impression of net changes.

Figure 10 is also used to show the similarity between ERA5 and JRA-55, and that the same qualitative changes are found if one considers monthly-mean wind speeds rather than the speed of the monthly mean wind. Also, the "sanity check" provided by examining geopotential height (section 7) is illustrated for annual rather than seasonal trends (to prevent the paper becoming even longer). It links back to Table 3.