SC on “Trends in the tropospheric general circulation from 1979 to 2022” by Adrian Simmons
Comments from Gloria Manney & Michaela Hegglin

We read your paper with great interest and most enjoyed the thorough and comprehensive analysis, especially the lucid discussion of considerations in addition to confidence levels that should fold into interpretation of the robustness of trend results.

We find the discussion at the end of section 4.3 of differences in the present results from those of Manney and Hegglin (2018, hereinafter M&H) very interesting, and indeed the topic may be worthy of some future analysis of how the differences in methods fold into the results, and what we can learn from that about physical interpretation of those changes. We do have a few additional suggestions related to these differences in methods that might also fold into the appearance of different results, which you may want to consider and, if you feel it is appropriate, mention in your discussion of this point in the paper. Regardless of any changes you may make, we would look forward to further offline discussion of these points in the future.

One obvious difference, related to the point you emphasize about our definition of “subtropical” and “polar” jets, is that we are defining these jets at all whereas you are analyzing the full wind fields – particularly in regions / periods (such as the ones discussed in this paragraph) where the distributions of jets (aka windspeed maxima) is not well characterized by a single jet (or in some cases even just two jets), it is not clear to us that trends in the full wind fields would always necessarily be colocated with those in the windspeed maxima.

We also note that because the altitude of the jet cores (even within a given type) varies significantly in pressure at different times/locations (and indeed M&H found robust trends in both subtropical and polar jet core altitude), your method using a single pressure level could potentially “translate” a shift in jet core altitude/pressure as a weakening or strengthening of the jet.

Regarding our definition of “subtropical” vs “polar” jets, our identification of a jet as the "subtropical" jet is based not simply on latitude but on identifying the westerly jet across which a “tropopause break” occurs – thus it is a physically-based definition, and the transition between a “subtropical jet” at one longitude and a “polar jet” at another would necessarily require a change in this character. In this sense, we did not (for the analysis of the subtropical jets) show results that may “mix up” jet types because of simple geographic considerations.

On the other hand, we did make a choice in M&H to analyze only one “polar” jet at each longitude, that being the strongest jet poleward of the subtropical jet (or of 40 deg latitude if there is no subtropical jet). The region / season in question here is one where the complicated jet structure may not be well-described even by just **two** jets. Our jet identification method does identify and characterize up to five jets at each longitude in each hemisphere (after that we identify just one of those as the subtropical jet and one as the polar jet), so future analysis accounting for more westerly jets, where present, could be illuminating.

Finally, a couple of suggestions on very minor details in the paper:

Re the discussion (re Fig. 7) around lines 345–356 regarding the results on the 2PVU surface (used here to represent the dynamical tropopause) in the tropics. In the examples we've looked at (including some of ERA5), the 2PVU surface is above (lower pressure than) 100hPa (which is near 380K, both being common levels used to represent the tropical tropopause) in much of the region between about ±10 deg latitude, so presumably at lower pressure than 200hPa in a substantially broader latitude region. The dynamical tropopause is, in fact, commonly taken to be 2PVU (or another PV value in that range with strong gradients) in the extratropics and 380K in the tropics (defined simply as wherever that PV value is above 380K). While this does not imply anything wrong with the discussion in this section of the paper, it does appear to complicate interpretation of the results, and it might be good to be a bit more explicit about where 2PVU does and does not represent the tropopause well.

It would be helpful, for those of us who are "geographically challenged", to give approximate latitude/longitude values for regions mentioned by name in the paper that might not be obvious to everyone on the maps (e.g., Barents Sea, line 47; Svalbard, line 317; Somali jet, line 671).

If it is easy to implement, it would be appreciated if the globe figures could include meridians and parallels to ease comparison of the study's results with other studies. It would also be helpful to label the color bars with the quantity and units they represent.

The caption for Fig. 10 seems a little confusing in how it describes the types of dashed lines that are overlaid, could this be reworded so that the two types of dashes are mentioned together so the reader doesn't start out wondering why the first dashed contour that meets their eye clearly doesn't match the value they are reading in the caption, e.g., something like "Short- and long-dashed contours in panels…show the 25 and 35 ms^-1 contours, respectively, from panels (a) and (c)"?

Figure 17, two panels are labeled (d) and none (c).

Line 699, "arctic" should be "Arctic".

Line 883, it would be clearer if you bounded "however" by commas.