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.

I am grateful for this comment, and open to further discussion and possible additional future analysis. I have made some changes to the text, as indicated below.

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.

I have expanded the initial discussion of the differences between our approaches in Sect. 4.1 to include a little more on M&H’s identification of subtropical and polar jet streams, including jet-core heights and their changes. I already note in the context of Fig.10 that changes in the monthly mean wind speed are qualitatively similar to changes in the speed of the monthly mean wind, notwithstanding the former being larger in magnitude than the latter.

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.

Yes, but only to a small degree. In responding to a point made by reviewer 2 I have noted that the extratropical trends in the speed of monthly mean winds have much the same structure at 500hPa as at 200hPa, though smaller magnitude. Something of the same can be said for 850hPa, as already noted in the paper. The changes at 200hPa are related to the changes in temperature gradient lower in the troposphere through the thermal wind relationship, and the similarities with the changes lower in the troposphere suggest that changes that result from changes in tropopause height or structure can be only a small fraction of the net 200hPa wind changes. The similarity between mid-latitude wind trends at 200hPa and |PV|=2 bear this out. The point does need to be made that I am not presenting the changes in peak jet speed, however. I have accordingly revised text in Sect. 4.2.

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.

I believe the changes to Sect. 4.1 and Sect. 4.2 noted above are almost all that is needed. I have made only small revisions to the final paragraph of Sect. 4.3.

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.

Reviewer 1 made a related comment, and I have amended the text to note that the pressure to which winds are interpolated, though first evaluated using a calculated value of PV, is not allowed to be lower than 89hPa. What I did not include, but can be found in the public documentation to which Hersbach et al. (2020) refer, is that all fields are fitted with spherical harmonics before archiving, so that pressures retrieved from the archive can have local (small-scale) values less than 89hPa. But these low values were not used in the interpolation of fields to the nominal |PV|=2 surface.

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).

I have made some minor re-wordings which may help. A balance has to be struck in matters such as this, as it is easy to irritate those who know well what one is talking about – as I once found out after giving a talk where I deliberately avoided referring to the Barents Sea by name. For the geographically challenged, which includes all of us to some extent, Google provides a quick way of finding what one wants, and helps in the process of becoming less-challenged.

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.

I prefer not to make these changes. Adding latitude and longitude lines would inevitably make it a little more difficult to see the detail displayed in the maps, particularly near the poles. The maps include coastlines and discussion in the text tends to refer to placenames rather than latitude and longitude, which makes for an easier read for those who are not geographically challenged (see above response). As to adding units to all the colour bars, I can of course do this quite quickly, and will do so if an editor requests it. But adding units would cause the maps to be smaller for a given figure dimension. In most if not all cases the unit is stated in the first line of the figure caption, so should be picked up quickly by the viewer.

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)"?

The caption has been revised. The result is shorter and I hope clearer. Thanks for pointing this out.

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

Thanks for spotting this. The correction has been made.

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

Thanks also for spotting this one. The correction has been made.

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

I have made this change too.