

Third round review:

The authors have made a few improvements to their manuscript and addressed a couple of major comments. Unfortunately, there are still major concerns about this manuscript, which were mentioned in the previous round of review but were not addressed. These concerns are detailed below. I would strongly suggest that the authors take more time to address all concerns and comments from both reviewers before submitting another revised manuscript.

One general issue is that I find that the amount of work put between the previous revised versions is not consistent with the concerns raised by the reviewers. The authors should realize that the time of the reviewers is valuable. When a reviewer strongly suggest major revisions before he/she can recommend the manuscript for publications, the authors should avoid responding as such: “if you still will request to change [...], we shall do it in the next revision”. How many revisions do the authors intent on going through?

For this reason, I hope the next revised manuscript will seriously address the many issues raised in this round of review, many having already been raised. Otherwise I will feel compelled to reject the manuscript.

Major issue:

- #1: As mentioned in previous rounds of review, the authors should start with the analysis of the longitudinal distribution of meridional and zonal wind, and the discussion of the two-core structure. I find this part interesting and well defended. I would suggest expanding the discussion to why it matters with respect to general stratosphere dynamics theory (troposphere-stratosphere interaction, wave dynamics...) since otherwise it doesn't add much to the scientific community.

- #2: The trend analysis need to be redone. There seems to be a major issue with Figure 1 at 100 hPa: the year-to-year variability seems wrong, especially when compared to Figure 2. I have attached a graph (Fig. 1) that I have created while trying to reproduce the analysis. They also show different behavior, leading me to believe that there is a problem with the authors' Figure 1. There are also questions about the choice of averaging (the quadrants). Why group the Pacific and North America together but the Atlantic and Europe separately? Surely the choice of averaging matter since only one quadrant shows a trend. By the way, why is only one quadrant showing a trend?

I suggest presenting maps of trends of total wind speed (pre-1990s and post-1990s) showing the trend at each lat/lon for 10hPa and 100hPa over the 20-60°N region (as the authors have presented for the longitudinal distribution of meridional and zonal wind). Then the same analysis should also be done for the meridional wind component and zonal wind component to determine the main driver. I have attached such maps as Fig. 2 (for NCEP/NCAR), for both Oct-Nov-Dec and Jan-Feb-Mar (there is a clear difference between the two seasons, in terms of magnitude of the trend and the sign of the trend for post-1990, as suggested in the previous round of review.

The Fig. 2 that I attached contradicts the statement that there are no trends outside of 50-55°N. It also shows trend outside of the Atlantic quadrant. Also the fact that trends are largest in Jan-Feb-Mar potentially indicate that ozone and troposphere-stratosphere interaction are involved.

- #3: The discussion of the results is “fuzzy”.

- Is ozone driving the trend in the Atlantic sector or is it there NAO?
- Why investigate how the QBO modulates the trend (which is not even properly explained), but not ENSO?

Maybe you should simplify the paper by simply investigating the presence of trends (if there really is a trend) and how they relate to the “most important result” of the manuscript, the two-core structure.

In light of the results shown in Fig. 2 (below), the discussion on trends should be redone:

- NAO is unlikely to explain the trends outside of the Atlantic quadrant
- trends in Jan-Feb-Mar are larger than in Oct-Nov-Dec potentially indicating that ozone troposphere-stratosphere interaction are involved

Less major issues:

- The introduction was improved. It is shorter, more to the point. However, it fails to provide context and relevance to the study. Why is it important to analyze the longitudinal distribution of stratospheric winds in the midlatitudes? Why would changes in the longitudinal distribution of stratospheric wind matter? Has this been studied before? For example, does the longitudinal distribution of stratospheric wind play a role on stratosphere-troposphere interaction? You state from line 75 to 77, that you study the longitudinal distribution of stratospheric winds (meridional) because it's never been studied. I don't think that's good enough of a reason. Certainly, one cannot expect to obtain funding on the simple basis of "nobody's investigated this topic, please fund me to look into it".

- Since the authors have added two reanalysis datasets (MERRA and ERA-Interim) for their analysis of the longitudinal distribution of stratospheric winds, I don't think their discussion on the "superiority" of NCEP/NCAR reanalysis over ERA-40 and ERA-Interim is needed. I will reiterate the comment that an evaluation of reanalysis based on three observational sites is not enough to make generalization about the quality of 3 reanalysis datasets for stratospheric winds over the entire Northern Hemisphere mid-latitude (which is your region of interest). In addition, you can certainly compare dataset mutually but that cannot provide any insight on which one is "better". On lines 96-97, I would replace "we use in the paper reanalysis data, namely NCEP/NCAR..." with "we use three independent reanalysis datasets, namely NCEP/NCAR1, MERRA and ERA-Interim". Then provide a brief overview of each (levels, resolution, temporal coverage) and their use in previous studies of the stratosphere.

- The authors claim that since the 3 reanalysis datasets show similar long-term climatology of longitudinal distribution of meridional and zonal wind, they can use only one for the trend analysis. I have a few issues with that:

1) the authors present the analysis of the consistency of the long-term climatology of longitudinal distribution of meridional and zonal wind between the three reanalysis after the trend analysis, so that point is not clear until later in the manuscript. I have a specific comment on the lack of logic of the structure of the paper later on...

2) the climatology might be consistent, that does not mean the variability and possible trends are.

Please expand the trend analysis proposed in "major issue #2" to all three reanalysis datasets.

Minor comments:

Line 67-68: I would suggest you change "we analyze some trends separately for time periods before and after the mid-1990s" to "trends in the stratospheric dynamics are expected to altered by the ozone recovery and thus periods before and after the mid-1990s should be analyzed separately."

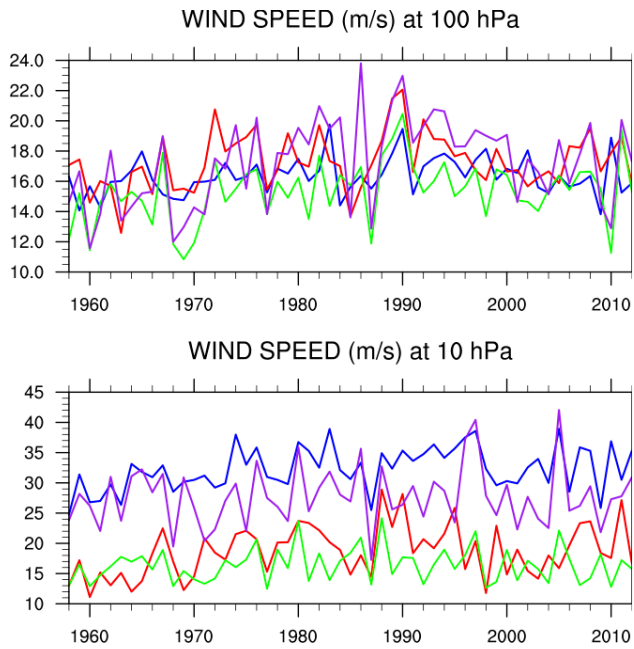


Fig. 1 Same as Fig. 1 in manuscript (52.5°N), except using daily mean instead of 00UTC.

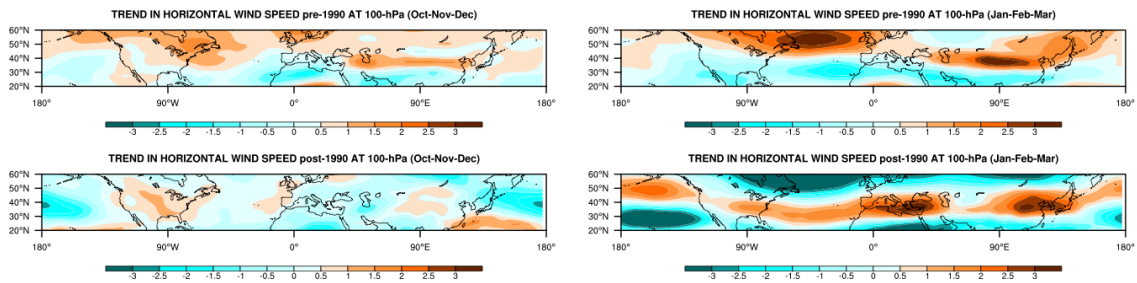


Fig. 2 Trends in horizontal wind speed (m/s per decade) pre-1990 and post-1990 at 100-hPa for Oct-Nov-Dec and Jan-Feb-Mar for the NCEP/NCAR reanalysis.