The authors have made a few improvements to their manuscript but several major comments made in the previous round of reviews have not been addressed. I do not believe the improvement have been substantial enough to reach the high quality requirements for publications in ACP at this time. While the paper presents some interesting and relevant results, the manuscript needs further revisions in order to be considered for publication in ACP. These revisions would essentially require improving/expanding the analysis, reorganizing the manuscript and improving the discussion section. These are major revisions, but the manuscript would greatly improve in quality.
I have listed several major revisions (by order of the paper not importance) that the authors need to make before I can recommend their manuscript for publication:

- The introduction was clearly improved. It remains however quite long (around 1200 words or a little more than 25% of the paper) so it should be more concise. It also generally lacks proper organization. I would suggest aiming preferably for closer to 15% (or around 800 words). For example, I do not see how the paragraph on reanalysis products (line 90 to 100) is relevant to the introduction: analyzing reanalysis data for stratospheric dynamics studies is certainly not novel and most people are familiar with reanalysis products and their general shortcomings (at least, this might be more relevant in the methodology section).

- I am not convinced by the explanation for using only one particular re-analysis dataset, in this case the NCEP/NCAR.
    - First, the Kozubek et al. (2014) study only compares to three observation stations and only after 1989 (only Prague-Libus is mentioned on line 138). I don't see how it is possible to generalize the quality of (or even rank) the three reanalysis products based on so few samples.
    - Second, the authors claim that the data is more reliable from 1957 onwards without citations to back it up. I am not sure I would consider reanalysis data before 1979 as "reliable", especially in the stratosphere.
    - I think it would be preferable to simply write that you chose the NCEP/NCAR reanalysis, even though you could have chosen others (including the JRA55 data, the NCEP/DOE reanalysis 2…). That this reanalysis product shows reasonable agreement with observations (for example, based on Kozubek et al., 2014), cite some studies that have analyzed the representation of the QBO, SSW… in the NCEP/NCAR reanalysis, an state as a limitation the fact that this study uses only one reanalysis and thus could lead to different results if using another reanalysis product.

- I am not convinced by the explanation for the choice of the three latitude bands.
    - The authors claim that the lack of ocean makes the data "better than in majority of other latitudinal band" without any citation to back up that claim. I certainly don't think it would be an issue after 1979, since satellite data is assimilated. But if it were an issue before 1979, it would make sense to simply start the analysis in 1979 then and analyze a larger latitudinal band. In addition, since the authors analyze longitudinal sectors, some with more ocean than the others, if the ocean does indeed lead to a degradation of the quality of the data and thus of the analysis, that could be a very worrisome issue for this study (although I don't believe it is the case).
    - I think it is necessary that the analysis be extended to more than the three chosen latitudes (see further down). *This comment was already made during the first round of review but not addressed by the authors*.

- The biggest flaw of the manuscript is its lack of consistency in three different aspects of the analysis:

1) <u>The region of study</u>: three latitude bands for the study of trends, QBO, SSW and solar cycle (only one for the NAO impact) but the mean of the 20-60ºN for the analysis of the longitudinal distribution of the meridional wind, the analysis of tide and geopotential height. It is unclear to me how the authors can make general concluding statement of the impact of NAO, QBO… on stratospheric winds based on just three latitudes (that are next to each other, thus only covering a 7.5º band).
This issue was pointed out in the previous round of review but not addressed by the authors.

2) The total wind speed versus meridional wind: the analysis of the longitudinal distribution of wind is focusing only on the meridional wind, even though it only contributes a small part to the total wind speed. There is some inconsistency in several places: the authors analyze the trends in total wind speed in Table 3 to identify trends in the " two-core" pattern of meridional wind even though the trend in the total wind might not be related at all to the meridional wind (it is most likely driven by changes in the zonal wind). The authors also state that Monier and Weare (2011b) "found some strengthening of northern jet in October-December and weakening in January-March" but that the authors' analysis of "meridional wind at northern higher middle latitudes reveals similar trends for October-December and January-March". The jet is mainly driven by zonal wind, not meridional wind, so the analysis of Monier and Weare relates to changes in zonal wind, not meridional wind. The authors should analyze the total wind separately between Oct-Dec and Jan-Mar, not the meridional wind… It is also unclear how to link trends in total wind speed to the meridional wind component.

3) The authors claim that the "well-pronounced longitudinal structure of wintertime meridional wind at 10 hPa to be the most important result" of their study. Yet, because the trend analysis is done before the analysis of the longitudinal distribution of meridional wind, the authors do not really discuss how this structure has evolved in the last few decades (see point above about inconsistency between analysis of meridional wind and total wind speed).
This issue was pointed out in the previous round of review but not addressed by the authors.

   I suggest the authors make the following revisions to their manuscript in order to bring it to the high quality requirements for publication in ACP:
   Present the analysis of the longitudinal distribution of the meridional wind (section 3.3) first, and add to it an analysis of the longitudinal distribution of zonal wind (essentially redo Figure 3 and Figure 4 but for the zonal wind). Then present the trend analysis of the total wind speed (section 3.1), but on the average over latitudinal bands similar to section 3.3. Averaging over 20-60ºN might be too large, so 30-40, 40-50 and 50-60 might be more appropriate, following the same "three latitudinal band" structure of Figure 1, Table 1 and Table 2. You should then discuss how the trends affect the dipole pattern of meridional wind in more details, using Table 3. Finally present the role of the solar cycle on the trends (section 3.2), once again on the same latitudinal bands (30-40, 40-50 and 50-60). This would provide an improved flow and add much needed consistency in the analysis.

- The authors mention that the reversal of trends in winds in the mid-1990s is "in accord with change in trend of ozone" on line 364. However, the authors state in the response to reviewers that "the total ozone trend change in the mid-1990s was caused mainly by dynamics (e.g. Harris

et al., 2008), therefore role of factors like NAO, QBO etc. might be really important." This last statement warrants a more complex discussion (and expanding analysis) on the role of ozone on the trends in total wind speed that the single sentence on line 364. What is the role of ozone versus NAO/QBO/SSW… in controlling the trends of total wind? No particular explanation is given on how ozone is affecting trends in the total wind speed, and in particular, why it only affects the atlantic sector (as shown in Figure 1 and mentioned in line 187). The authors could, for example, plot the ozone concentration along Figure 2, showing that the different trends in wind speed are not only explained by NAO, QBO… but also by ozone concentrations. In any case, the discussion and analysis of the impact of ozone is seriously missing, and that's a clear shortcoming of this manuscript.
This issue was raised in the first round of review but not addressed by the reviewers (except to add a few general statements on the importance of ozone for stratospheric dynamics in the introduction).

- Generally, the manuscript is too descriptive (Fig X shows that such and such is occurring) but lacks explanations (such and such is occurring because of this potential mechanism…). There are a lot of questions that are neither answers nor even raised:
By what mechanism are the NAO, QBO, solar cycle impacting trends in the wind speed? What is causing the trends in wind speed to start with? Is it changes in ozone? Is it decadal variability in NAO? Why would the phase of the QBO, solar cycle, impact these trends? Are NAO trends driving ozone changes? Are ozone changes driving NAO changes? How? Why are there different trends pre-1995 and post-1995 in NAO if the trends in ozone are driven by dynamics? Why is there also a reversal of trends in 1970 (based on Figure 2)? The manuscript would benefit significantly from a better discussion/conclusion section, instead of just simply presenting the results without any real discussion.

Minor comments:

Figure 1: the top panel (100 hPa) starts in 1968 while the bottom panel (10 hPa) starts in 1958. Both should start at the same time. In addition, the difference between sectors is difficult to see. The different markers should be replaced by different colors instead.

Figure 2: The artificial discontinuity between the three time periods should be removed. Please plot a single continuous line. This was a comment in the previous round but was not addressed by the authors.