The paper is substantially re-structured with respect to comments of reviewer #2 so the changes (especially in abstract, results, discussion and conclusion section) are hard to highlight.

Response to comments of editor:

Abstract:

The abstract needs to be improved. It should be more concise what the purpose of your study is. What is the motivation? Why do you look at trends in winds. What does it mean for the atmospheric system is there is a trend in wind speed? Does it has an influence on NAO, QBO or SSW.

More specifically: Why do NAO and QBO or SSW have an influence on the trend? Shouldn't it be rather vice versa? If there is a trend in horizontal wind will it have an effect on wind driven phenomena as QBO, NAO or SSW?

L13: Add what the abbreviation NCEP is standing for.

L17: Seems? At this stage of your study it should be clear what the most important result is.

L21: What are the abbreviations ERA and MERRA standing for? Please add.

A: Abstract has completely been re-written. We hope it now responds to all above requirements.

L71: Same for ENSO. One of the reviewers already asked you during the previous revision to introduce all abbreviations, but this has obviously not been done.

A: The abbreviation has been explain in the text – see line 65.

L74: Which phenomena. Give an example.

A: The sentence was deleted.

L77: Of what? Please be more concise. Has the strong effect at 10 hPa been found in a previous study or in this study?

A: The introduction has been modified and this sentence is not in the text.

L80: Skip “other”

A: The introduction has been modified and “other” is not in the text.

General comments on the Introduction:

It becomes clear from the introduction what your analyses consists of, but not why you are doing this. For example, why do you think the latitudinal distribution of meridional wind could be interesting? To just state nobody has done that before is not enough. There should be a motivation for why you doing this study and this motivation should be clearly stated in the introductions so that it becomes clear to the reader what the purpose and benefit of your study is.

A: The results on longitudinal distribution of meridional wind could have an impact on Brewer-Dobson (B-D) circulation changes, which is very important for ozone distribution (Demirhan Bari et al. (2013) study shows the existence of such an influence over 2011-2006). The distribution of meridional wind is important also for wave propagation in the stratosphere (Matsuno, 1970; Kodera et al., 1990). Therefore here we investigate longitudinal distribution of meridional and also zonal component of stratospheric winds at northern middle latitudes.

L93: "Rufenacht" should read "Rüfenacht".

A: It is corrected in the text.

L92: Add also the study by Baron et al. (2013) who derive winds from SMILES.

A: It is added in the text.

L97: What is NCEP/NCAR-1 standing for? Add what the abbreviation is standing for.

A: The abbreviations has been explained (spelled out) in the text.

L120: Why do you choose 100 and 10 hPa. What dynamics and conditions are pronounced in these altitudes at middle latitudes? Why middle latitudes? Why don't you choose the tropics or polar regions for your study?

A: The 100hPa level has been chosen as a representative level of the lower stratosphere at higher midlatitudes. The 10 hPa level has often been used as a representative level for the middle stratosphere and major stratospheric warming determination. We choose the midlatitudes because in the past we studied various phenomena very predominantly in middle latitudes, we tested quality of stratospheric winds from several reanalyses in northern middle latitudes, long-term (several decades long) data series seem to be of better quality at northern middle latitudes, and Europe and Czech Republic are area where we are living (our funding agencies prefer research with some relevance to our country). In future we want to make comparison with mesospheric and mesopause region winds, where long-term data series are available just for latitudes between 52-55°N. In future we broaden our stratospheric analysis globally but first we are willing to test if it makes sense on a smaller midlatitudinal sample.

L121: Why choosing three latitudes? Why is it not possible to perform the analyses in a latitude band? I cannot follow your argumentation with the land/sea distribution. The NCEP data is available globally.

A: We choose three latitudes, but partly different from selection in previous version of the paper, for studying behaviour of the two-core structure of meridional wind. The sentence with land/sea distribution was deleted. We prefer to make analysis for three separate latitudes instead of one band in order to check possible latitudinal difference (say finer structure) at higher middle latitudes.

L136: Skip text in brackets "(using standard MATLAB routine)"

A: The text could be skipped but one of the reviewers required this sentence before.

L139: How do you divide the data into several groups? Which criteria are used?

A: We divide data of the whole period into several groups according to QBO (east or west QBO phase years) or solar cycle influence (solar maximum years and solar minimum years) and for a trend analysis we divided data into two period (1970-1995 with decreasing ozone and 1995-2012 with increasing ozone). It is now in the text.

L176: Why have these three different time periods been chosen?

A: Only one time periods (1970-2012, divided into two because of ozone behaviour for trend studies) is used for trend analysis in the revised paper.

L179: Why is the trend in wind significant, but not the trend in NAO?

A: NAO is not discussed in the revised paper, as the original Figure with NAO-similar trend in wind in Atlantic sector was wrong (data problem/mistake), for which we apologize.

L184: Why have these time periods been chosen? Why do they differ from the ones which have been chosen before?

A: Only one time periods (1970-2012, divided into two because of ozone behaviour for trend studies) is used for trend analysis.

L269: skip "for"

A: It is not in the text now.

General comment on the Method section: Why has for every part of the study other longitudes and latitudes been chosen? What is your motivation for the specific selection? Why do you need for each sub study another set of latitudes/longitudes and time periods? This causes a lot of confusion.

A: In the revised paper, we choose three latitudes for studying the two-core structure of meridional wind. We study four sectors covering longitudinal circle – two with cores in meridional wind and remaining two without these cores in meridional wind. Only one time

periods (1970-2012, divided into two because of ozone behaviour for trend studies) is used for trend analysis.

General comment on the Result section: I still cannot follow how you derive proof for that QBO, NAO and so on have an influence on the trends in wind speed.

A: NAO is not discussed in the text now (see comment to L179). This section (now 3.2) has been substantially re-worked.

The paper is substantially re-structured with respect to comments of reviewer #2.

Response to comments of reviewer #1:

General comments:

Overall: The authors have made an effort to address my latest comments. However, I still think the paper is not suitable for ACP. I think the paper still suffers from the following shortcomings:

(i) Confusion – there is a lot of information, e.g., in the Introduction, but there is poor organization, the focus is unclear, and the arguments are difficult to follow;

A: The introduction was reformulated and now it should be more to point.

(ii) Discussion over a narrow latitude band – despite the extensions made in the last version, the paper is still mainly about features over a very narrow latitude band, and which is not convincingly justified in the text;

A: The paper changed its structure. The narrow latitude band is slightly broader and we hope justified. It was chosen for studying the two-core structure of meridional wind (section 3.1).

(iii) Trends – fine, 95% is a standard significance level in meteorology, however, one wishes to test that the quantity estimated, in this case trends, is robust. One way of doing this is testing the significance level at 99%. In my view, the results for the trends are generally not robust;

A: The trend analysis is now on 99% significance level. The 95 % threshold remains only for difference between total wind in different groups analysis (Tab. 2). On the other hand the longer time series does not always have to improve the trend analyses.

(iv) The English – much better, but still there is use of vague and qualitative language.

A: We have tried to improve the English. The manuscript has been checked by native speaker (even not physicist).

I would add that there is evidence that the authors, in their response, agree (at least partially) with comments (ii) and (iii) above:

Narrow latitude band:

L. 153: The latitude band is 49N – 56N, a range of 7 degrees. Is this representative of mid latitudes?

A: We now analyse latitudes 50, 55 and 60^{o}N, related to the two-core structure. Paper is substantially re-written.

My answer: I agree with you regarding the representativeness of the relative narrow latitude band.

A: The narrow latitude band is now discussed better. It was chosen for studying of two-core structure of meridional wind (section 3.1).

Trends:

L. 243: Only 4 trends (out of 192, I understand) are significant at the 99% level. And as

mentioned by the authors, this is likely due to the limited length of the datasets. Which begs the question, why calculate trends with a dataset limited in length?

A: The original Table 1 and related text have been removed from the paper as a consequence of re-structuring the paper and changed sector selection/definition. The trend analysis is now on 99% significance level. The 95 % threshold remains only for difference between total horizontal wind in different groups analysis. On the other hand the longer time series does not always have to improve the trend analyses.

Table 1: Why show this table if significance at 99% only occurs for 4 cases?

A: Original Table 1, which suffered with only 95% significance, was removed as a consequence of re-structuring the paper and changed sector selection/definition.

To summarize, before the paper is suitable for ACP, the authors should address the above issues, as well as the specific issues below.

Specific comments (not exhaustive, but illustrative):

L. 13: Introduce acronym for NCEP/NCAR – in the abstract and in the main text. Do the same for all acronyms.

A: The acronym is now described.

L. 17, 22: "seems to", "appears to" are vague and, in my view, unsuitable for a scientific paper. Please avoid such language.

A: The abstract has been reformulated and seem is not any more in the text.

L. 79-85: No mention of trends.
A: The introduction was changed and the trend is now in the text.

L. 106: "slightly better" is qualitative. Please quantify your statements.
A: This was reformulated in the text.

L. 177: "are similar in tendency" – no, they are not. There are negative and positive tendencies.
A: This is not any more in the text.

L. 202: "support a tendency…trends". Rephrase to give sense.
A: This is not any more in the new text.

L. 234: At what level are the differences significant? I presume it is 95%, but this information should be provided to the reader. There are other parts of the text where this information is not provided.
A: Thank you for your comment. It is corrected in the text.

Table 3: I suggest to the authors that this is the only robust result of the paper (significance at both 95% and 99% levels).
A: Tables were redone and Table 3 was included into Table 1 with 99% significance level.

Fig. 3: Indicate in the caption what the straight lines are.
A: The original Figure 3 is not any more in the text.

The paper is substantially re-structured with respect to your comments. Thank you very much for your efforts.

Response to comments of reviewer #2:

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.

A: The paper has been changed and the two-core structure analysis is treated first as the main part of section Results and after that the trend analysis is shown. In section 4 Discussion we add new Fig. 7, which shows that temperature and ozone also respond to the two-core structure, and we discuss this phenomenon somewhat more. Answer the question of impact on troposphere-stratosphere interaction needs first to look in more detail how the two-core structure and Aleutian high in geopotential heights develop downwards down to the troposphere, which is topic of future research, which we plan to start immediately after this paper is finished (next two months will be period of priority writing of various reports for funding institutions, project, the institute etc., anyway).

-#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 20-60ºN 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.

A: Thank you very much for this idea and for your Figures 1 and 2. We shall do it in (near) future. In this paper we limited trend analysis to latitudes where the two-core structure is well-

developed. The results show clearly much stronger and more statistically significant trends at 99% level, which was one of the main requests of the second reviewer, in cores than outside core area, where the significance occur only at 95% level.

The Fig. 2 that I attached contradicts the statement that there are no trends outside of 5055ºN. It also shows trend outside of the Atlantic quadrant. Also the fact that trends are largest in Jan-Feb-Mar potentially indicates that ozone and troposphere-stratosphere interaction are involved.

A: We apologize our previous results were affected by data problems and therefore they were not correct. You are right.

General answer: The analysis of NAO was withdrawn (the original Fig. 1 was wrong – data problem, we apologize for that) and only trend analysis of latitude where two core structure is presented. For this analysis we have chosen different sectors, which is closely connected with two-core structure. We analyze the different sectors for each cores (50°N-60°N) and that is why we do not show trend for each grid points (the important features or statistical significant trends could disappear or could not be detected). The significance level of this analysis is 99 % now which will improve the robustness of the analysis (one of the main request of the second reviewer). Only 10 hPa level remains because there is no two-core structure at 100 hPa. We have analyzed DJF period because we found the strongest two-core structure in January. The two different periods (pre-1995 and post-1995) shows clear change of trends anyway.

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

-Is ozone driving the trend in the Atlantic sector or is it there NAO?

A: NAO effect is no more studied in the paper as the original Fig. 1 was wrong (data problem). The trend in Atlantic sector does not exist anymore + sectors are now somewhat different from original sectors (taking into account the core structure).

-Why investigate how the QBO modulates the trend (which is not even properly explained), but not ENSO?

A: The QBO affect mainly equatorial and northern hemisphere stratosphere and is closely connected with stratospheric wind (Salby and Callahan, 2000, Labitzke and Kunze, 2009, Limpasuvan et al. 2004, Naito and Hirota, 1997, Labitzke and Loon , 1988). ENSO affects mainly southern hemisphere circulation. Lastovicka et al. (2014) found no influence of ENSO on partly dynamically driven ozone laminae.

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:

a. NAO is unlikely to explain the trends outside of the Atlantic quadrant

A: NAO is not treated in the revised paper. We have not been intended to claim that NAO could explain trends in other sectors – we agree with you.

b. trends in Jan-Feb-Mar are larger than in Oct-Nov-Dec potentially indicating that ozone troposphere-stratosphere interaction are involved

A: The paper was simplified and we are focused mainly on the two-core structure at 10 hPa and trend in this area. We have analyzed DJF period because we found the strongest response (two-core structure) in January. The two different periods (pre-1995 and post-1995) shows

clear change of trends anyway. The difference between early and late winter (OND versus JFM) will be studied in a separate paper.

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".

A: Introduction was redone and it should be more to point. The results of meridional distribution could have an impact on Brewer-Dobson (B-D) circulation changes, which is very important for the ozone transportation. The distribution of meridional wind is very important for wave propagation in the stratosphere (Matsuno, 1970, Kodera et al., 1990). Therefore here we investigate longitudinal distribution of meridional and zonal component of stratospheric winds at northern middle latitudes and his impact to QBO or Sudden stratospheric warmings (SSWs) which is mainly wave driven.

-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.

A: The analysis of longitudinal distribution has been done for three reanalyses (MERRA, ERA-Interim, NCEP/NCAR) and the results are very similar. Kozubek et al. (2014) found very close evolution of wind from to year between ERA-Interim and NCEP/NCAR over the whole period of ERA-Interim data. That is why we used only NCEP/NCAR reanalysis for trend analyses. This reanalysis has the longest time series and this is one of the main requests of the other reviewer (length of time series).

-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.

A: The analysis of longitudinal distribution has been done for three reanalyses (MERRA, ERA-Interim, NCEP/NCAR) and the results are very similar. Kozubek et al. (2014) found

very close evolution of wind from to year between ERA-Interim and NCEP/NCAR over the whole period of ERA-Interim data. That is why we used only NCEP/NCAR reanalysis for trend analyses. This reanalysis has the longest time series and this is one of the main requests of the other reviewer (length of time series). The paper is re-arranged, now we start section 3 Results with climatology in 3.1.

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."

A: It has been included into the text.

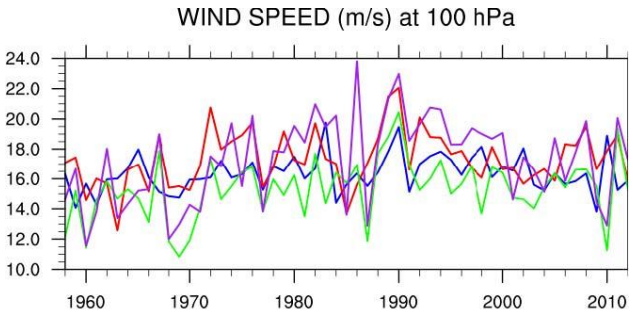

Fig. 1 Same as Fig. 1 in manuscript (52.5ºN), except using daily mean instead of 00UTC.

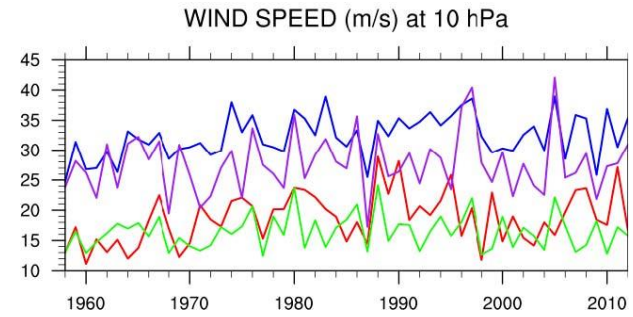

Fig. 2 Trends in horizontal wind speed (m/s per decade) pre-1990 and post-1990 at 100-hPa for Oct-Nov-Dec and Jan-Feb-Mar for the NCEP/NCAR reanalysis.