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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Stratospheric winds: longitudinal distribution and long-term trends" *by* M. Kozubek et al.

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Due to some misunderstanding the reviewer reviewed the original version of the paper, not the version published in APCD.

This paper attempts to analyze the climatology and trends in the Northern Hemisphere mid-latitudinal stratospheric winds, and investigate the impact of QBO, NAO and the solar cycle. The manuscript shows a few interesting results but the paper is not well organized, it lacks consistent analysis of the statistical signiïňĄcance of the results presented and the authors fail to convey the relevance of their analysis. I recommend several major revisions before the manuscript can be considered for publication in ACP.

Major revisions:



-The introduction should be rewritten: instead of focusing on theories of stratospheric dynamics and how reanalysis datasets have issues, I would suggest providing concrete examples of why changes (whether human-induced or not) in stratospheric winds matters. The unprecedented ozone loss in the Arctic in 2011 caused by extreme meteorology comes to mind (see Pommereau et al., 2013). The introduction should also include previous studies of climatology and trends in the stratospheric dynamics based on reanalysis datasets (for example, Monier and Weare, 2011a), the impact of the QBO, NAO... and the analysis of longitudinal distribution of winds (for example, Weare, 2010).

Answer: We have added some of your suggestion to the text of introduction and more information about changes in the stratosphere and importance of ozone as a main stratospheric driver of changes. All four references you recommended are now included in the text and also several others are added. The Introduction is broadened and now it includes also reasons why to study impact of solar activity on total horizontal wind, reasoning why we divided some investigated periods into two (before and after the mid-1990s, time of turnaround of ozone concentration), and reasons why to study longitudinal distribution of meridional wind. The meridional wind is important as the Brewer-Dobson circulation is its component. In the beginning of this study the longitudinal distribution of meridional wind was only by-product of main investigation but now we consider the well-pronounced longitudinal structure of wintertime meridional wind at 10 hPa to be the most important result of our study.

-The analysis of Figure 2, along with Table 2, lacks a proper statistical signiïňĄcance test. For Figure 2, I would highly suggest that the authors plot the continuous time series (not split into three periods), then they can add trends for different periods, and they should provide the correlation/regression (over the whole period) to show in statistical terms how much of the year-to-year and decade-to-decade variability can be attributed to NAO. For Table 2, the authors need a statistical test showing whether the differences between solar min/max and phase of QBO are signiïňĄcant. Since

the sample size of each case is different, I suggest following the method described in Weare (2010).

Answer: Plot in Fig. 2 is now continuous. Table 2 has been replaced by a new Table 2, which shows differences, not absolute values. Statistical significance at the 95% has been computed. Figure 2 reveals slopes of the same sign in wind and NAO for all three periods, which suggests some influence of NAO on winds. Regression analysis over the whole period requires including several regressors to get reasonably reliable results and for some of them we probably do not have reliable input information for the whole period.

-The "winter" analysis is based on the average between October and March. Monier and Weare (2011a) show that the trend in the zonal mean zonal wind north of 50âŮęN over the 1980-2001 period is negative from October to December and positive from January to March. In light of this result, I would strongly suggest redoing the analysis for these two periods (Fall, from Oct-Dec and winter, from Jan-Mar). The results might be different.

Answer: We have done similar analysis for October-December and January-March. Our results do not confirm Monier and Weare (2011a) results; long-term evolution/trends are similar to each other and to winter (Oct-Mar) analysis. Monier and Weare (2011a) used zonal means whereas we are using sectorial means.

-One important result is the change in the trends between the pre-1995 and postÂň1995 periods. It is explained on line 207 as caused by the change in ozone. Ozone is only mentioned once in the entire manuscript. This seems particularly strange, since it follows form the analysis that ozone has a substantial impact on the trends in the stratospheric winds compared to that of the QBO, NAO and solar cycle. The authors should expand the discussion on the impact of ozone on wind and stratospheric dynamics. I would suggest the authors read and reference Monier and Weare (2011b), who discuss the climatology and trends of ozone and its dynamical transport.



Answer: We have added more information about ozone trends to the introduction with discussion of its influence on the climate and stratospheric changes. Some had already been in the ACPD version. Ozone is important for the behaviour of winds as its changes are responsible for changes of temperature field. However, 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. In other words, the effect of ozone might be in fact at least partly the effects of factors like NAO, QBO and others.

- The organization of the manuscript is strange. The authors start presenting a trend analysis and then show some climatology. I would suggest presenting the climatology ïňĄrst (for the 1980-2010 period, see below for an explanation of the choice of this parÂňticular period), and then discuss how this climatology has been changing and what is causing the change.

Answer: The first part deals with trends in the total horizontal winds, whereas climatology in terms of longitudinal distribution is studied only for the meridional component.

Minor revisions:

-The title should reïňĆect the fact that the analysis is only for the Northern Hemisphere and the midlatitude. I would suggest something like: "Northern Hemisphere midlatitude stratospheric winds: longitudinal distribution and long-term trends"

Answer: Thank you for your suggestion. We changed the title "Northern Hemisphere midlatitude stratospheric winds: long-term trends and longitudinal distribution."

-Abstract: Line 12, the authors state they use the MERRA reanalysis but later in the main text it is revealed they use NCEP/NCAR-1.

Answer: This problem has been already solved in the ACPD version of the paper. MERRA reanalysis was deleted.

-Section 2: The choice of the NCEP/NCAR-1 reanalysis is not explained and it would

be beneïňĄcial to discuss how the analysis of one single reanalysis might be indicative of any robust behaviour. At least provide a few sentences on why the authors chose this particular reanalysis compared to ERA-40 or MERRA (which the authors state they used in the abstract, by the way)... but I would suggest citing Monier and Weare (2011a) would analyze the climatology and trends of stratospheric zonal mean ïňĆow in both the ERA-40 and NCEP/NCAR-1 reanalyses.

Answer: This problem has been already explained in the ACPD version of the paper. According to Kozubek et al. (2014) winds from the NCEP/NCAR reanalysis reveal the best results when compared with ERA-40 and ERA-Interim reanalysis or observations at Prague-Libus. ERA-40 has a problem with wind speed and direction distributions at 10 hPa in the last four years (1998-2002) and ERA-Interim agreement with observations is slightly worse than that of NCEP/NCEP reanalysis. Moreover, neither ERA-40, nor ERA-Interim separately covers the whole period 1970-2012.

-I would suggest the authors not use the reanalysis data prior to 1980 because of the lack of satellite data. The manuscript should not change substantially if only using data from 1980 to 2010. Answer: General pattern and long-term changes of stratospheric winds in all three reanalyses (except for the last four years of ERA-40) are very close each other since about 1970 (Kozubek et al., 2014), therefore it is sufficient to use only one reanalysis. We use latitudinal band where land prevails over oceans, so problem of absence of satellite data is not as hot as in most latitudinal bands. The longer data series, the better.

-Figure 1, Figure 2, Table 1 and Table 2: it is unclear why the authors choose these particular latitude bands. I would suggest averaging over different latitude bands (30Âň40, 40-50, 50-60, or even 30-60N) and then compute the different time series, trends and statistical tests for signiïňĄcance. The results should be more robust and more representative of the mid latitudinal behaviour. Answer: We agree with you that one latitude would not be enough. We use three latitudes, 50o, 52.5o and 55oN (e.g., Table 1 or lines 112-116). The three selected latitudes represent middle latitude band from 49°N



to 56°N. We analyzed them separately to show the detail changes at each latitude but usually the results are quite similar (see Table 1), therefore we present them predominantly for one latitude only. Moreover, the advantage is that this latitudinal band consists mainly of land, not ocean, i.e. data coverage even before satellite era seems to be relatively good, better than in majority of other latitudinal bands.

- Line 179: "repeated the analysis of v wind component for each grid point from 60N to 20N" I don't understand how the authors have "repeated" any previous analysis (they are simply plotting the mean v over 1958-2012) and "for each grid point" since you are averaging over the latitudinal band.

Answer: This problem does not appear in the ACPD version of the paper any more.

-The impact of ENSO is mentioned in the abstract and introduction but never analyzed. Why?

Answer: Sorry for mistake, ENSO was deleted from abstract.

-Figure 5 and Figure 6: I am not sure these graphs are needed. I would suggest just stating in the main text that there is no difference between the climatology at 00 UTC, 06 UTC and 12 UTC, thus indicating that diurnal or semidiurnal tides are not responsible for the dipole structure.

Answer: We consider the existence of two-core longitudinal distribution of meridional wind in winter to be the most important result of the paper. Therefore we are willing the statement that neither diurnal, nor semidiurnal tide is responsible for such a distribution supported by Fig. 5; if it is strongly requested, we could delete this Figure but at present we keep it. As for Figure 6, the longitudinal distribution of geopotential heights is key to explanation of the two-core longitudinal structure, therefore we consider important to keep Fig. 6.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16387, 2014.