Response to referee 1

We appreciate the comments of this reviewer. We have made the presentation more concise, and eliminated the less relevant details. We believe the discussion in Sect 4.2 is now simpler and more well-founded after almost entirely rewriting it and considering the uncertainty of the response of saturated VMR to the annular modes (Figs. 8-9).

The paper offers too many details on the satellite retrieval methodology (section 2.1 and 2.2).

The reason for the details on the other satellites is so that the reader appreciates the virtues and uniqueness of the ACE measurements. We have deleted some details on other satellites and regarding MAESTRO as follows:

(p22296L7) "because the atmosphere is much more transparent in the polar regions and the ice sheets of Greenland and Antarctica are elevated, leading to a surface contribution that complicates the application of the radiance-to-humidity relationship at high latitudes"

and

(p22296L12) "and the number of degrees of freedom of signal is < 1"

and

(p22296L24) "MAESTRO measures absorption of solar radiation by water vapour in the ~940 nm overtone-combination band (Sioris et al., 2010)."

and

(p22296L29) "where the apparent water vapour optical depth (i.e. at MAESTRO spectral resolution) can be 4 to 5 at ~935 nm, the wavelength of maximum absorption."

and

(p22297L3) "and relies on the HITRAN 2008 spectroscopic database (Rothman et al., 2009)."

and

(p22297L12) "The water vapour profiles are retrieved on an altitude grid that matches the vertical sampling, typically 0.4–0.6 km in the upper troposphere."

We also condensed the two sentences on IASI (p22297L13) into one:

"IASI (Infrared Atmospheric Sounding Interferometer) water vapour retrievals have coarse poor vertical resolution in the polar upper troposphere and the upper altitude limit of the retrieval only approaches the tropopause (Herbin et al., 2009; Wiegele et al., 2014)."

We disagree that section 2.2 has too many details of any sort. It is important for the reader to understand the extent to which the datasets have been previously validated and the reported retrieval uncertainty profiles.

The description on the seasonal cycle of water vapor is also too long winded. The reader could easily lose track of what points matter and what don't. It is unclear to me what new insights are gained from these analyses.

The new insights we need to communicate are:

1) MAESTRO data are of sufficient quality in the high southern latitude upper troposphere in September in spite of the dehydration (which might be expected to make reliable measurements challenging).
2) There is a strong seasonal cycle in the high latitude upper troposphere in both hemispheres which is consistent with the seasonal cycle of the local temperature.
3) The seasonal cycle observed by the ACE instruments differs between hemispheres due to interhemispheric differences in ACE's spatiotemporal sampling.

We have deleted:

(p22302L21) "In the upper troposphere, the September dehydration is a cumulative effect of local condensation (see also Randel et al., 2012) with the temperatures at 9.5 km reaching so low that the corresponding saturation mixing ratio can be as low as 4.4 ppm, much lower than minimum mixing ratios observed in the troposphere outside the Antarctic. In the mid-troposphere, the driest month shifts closer to mid-winter (e.g. August). This is observed by both ACE instruments and by POAM III.
The vertical distribution of the lower stratospheric dehydration resembles that measured from other solar occultation instruments: HALOE (Halogen Occultation Experiment) and POAM III in that the lowest water vapour mixing ratios occur at pressures higher than 100 hPa (below 16 km) (Hegglin et al., 2013). The MAESTRO climatological mean mixing ratio for September exhibits a minimum at 12.5 km altitude with a value of 2.9 ppm (Fig. 4), which compares well with the September minimum values observed by other instruments (Hegglin et al., 2013). Also, the stratospheric monthly medians are reasonable with mixing ratios <7.5 ppm up to 22.5 km, the upper altitude limit of the climatology."

and

(p22304L2) "Sioris et al. (2010) studied the seasonal cycle in the 60-70°N band using an earlier version of the MAESTRO dataset. They incorrectly concluded that saturation vapour pressure changes could not explain the seasonal cycle assuming the seasonal cycle amplitude in temperature at 8.5 km was only 8 K (based on climatological subarctic winter and summer temperature profiles)."

The authors tried to use the existing two mechanisms to explain the anti-correlation between water vapor at UTLS high latitudes and the AO/AAO. But I found it hard time to follow the authors' argument based on Figs. 11 and 12.

Figs. 11-12 have been deleted. The arguments we are making in Sect 4.2 have been simplified and by plotting the response of saturation VMR to the annular modes and the associated standard errors in Figs 8-9, the interpretation is simpler for us and hopefully for the readers. We now write in Sect. 4.2:

“The response profile of saturation VMR relative anomalies (from analyses of the GEM assimilation system) to the AAO (Fig. 8) is studied in order to gain insight into the relative contribution of the two proposed mechanisms. The ability to distinguish between the two mechanisms using saturation VMR anomalies requires that the mechanisms are not correlated spatially with each other to a high degree. This has been verified using the latitude and altitude dependence of their responses to the annular modes (Thompson and Wallace, 2000). The two mechanisms are complementary in that they both increase UTWV at high latitudes during the negative phase of the local annular mode.

Below 9 km, this response tends to be weaker than the response by deseasonalized water vapour observed by the ACE instruments, implying that the temperature mechanism cannot fully explain the strong observed response of water vapour at southern high latitudes. Near the tropopause (9.5-10.5 km), the response of saturation VMR to the AAO becomes effectively zero (within 1σ), but the response of observed water vapour to the AAO is also decreasing considerably relative to lower altitudes. The response of water vapour to the AAO differs significantly between MAESTRO and ACE-FTS except at 5.5 and 6.5 km, making it generally difficult to unequivocally determine the relative contribution of the two proposed mechanisms. Nevertheless, there is an obvious need for a mechanism in addition to the temperature-related one to explain the observed response of water vapour in the southern high latitudes upper troposphere. The effectiveness of the meridional flux mechanism during negative AAO periods is amplified by the large latitudinal gradients in water vapour between this isolated region and southern mid-latitudes.

At northern high latitudes, saturation VMR responds to the AO in a similar fashion to its response to the AAO at southern high latitudes (Figs. 8-9). The response of saturation VMR to the AO at northern high latitudes tends to be smaller in magnitude than the response by water vapour inferred from ACE observations, but the difference is not statistically significant at all altitudes compared to the ACE-FTS water vapour response. The water vapour anomalies from the two ACE instruments show a decreasing response to the AO with increasing altitude at northern high latitudes, but generally differ in the magnitude of the response, as is the case as well at southern high-latitudes. Thus, no general conclusion can be unequivocally drawn about the relative contribution of the two proposed mechanisms in the northern high latitude upper troposphere."

Essentially, we believe that the need for a second mechanism in addition to the temperature-related one is fairly obvious in the southern hemisphere while not obvious in the northern hemisphere given the combined experimental uncertainties implied by the differences between the ACE instruments in the revised Figs. 8-9. In the southern hemisphere, latitudinal gradients of upper tropospheric water vapour VMR (between high and mid-latitudes) amplify the effectiveness of the meridional flow mechanism.

In order to simplify Sect. 4.2, we have also removed any discussion of ozone since it is not central to understanding the arguments for both mechanisms. Sect 4.2 has been extensively rewritten with more justifiable conclusions about the role of the two mechanisms in each hemisphere.

Also, no attempt is made to discuss the implication of the findings. The manuscript ends abruptly by pointing out that "longer datasets and further analysis would be helpful to understand the contribution by each proposed mechanism."

The finding that the annular modes largely control high-latitude deseasonalized upper tropospheric water vapour has minor implications for climate based on our November simulation for the AAO (Sect. 3.2, with details of the method in the Appendix) and more extensive calculations for the AO by Li et al. (2014) which indicate larger radiative forcing variations due to AO-related cloudiness changes (cited in Sect. 1). However, radiative forcing anomalies due to AO-related cloudiness variations may be, to some extent, driven by AO-related water vapour variations, although such implications are outside the scope of our study. Regarding water vapour trends, the implication of the finding is that trends in high-latitude upper tropospheric water vapour should include the annular mode as a basis function to reduce trend uncertainty (Sect 4.1) and possibly reduce bias, depending on the studied time period. The two implications of our study are already discussed in the ACPD version.

Many of the formulation of analysis method are too subjective and thus need to be further justification.

1) P22300 Line10-15 I am not convinced of removing single particular month from calculating the climatology just because of the results of regression is improved by doing so.

The reviewer claims that the analysis method is subjective in many instances, but from their comments, it seems that only this first one involves any apparent subjectivity on our part. To justify the discarding of July and August 2011 from the climatology at 6.5-9.5 km, we instead refer to the clearly anomalous water vapour VMRs at 7.5-8.5 km illustrated in the companion paper that has now appeared in Atmos. Chem. Phys. Discuss. Furthermore, because the anomaly is caused by a phenomenon (volcanic emission) that is completely disconnected from the AAO, it seems reasonable to omit this two month period in compiling a 10-year climatology given that this was clearly the most explosive volcanic eruption on Earth in the past two decades (1992-2014).

2) P22301 line 10-15 I don't understand why an index plus a constant is needed for the regression analysis.

This is a moot point: if the constant is not needed, it will take a value of 0. As implemented, the linear basis function is simply an array of decimal times (in years) for each available month, so this basis function does not average to 0 over the time period. Similarly, the respective means of the annular mode indices over the ACE time frame are not subtracted off; each annular mode index is used 'as is', thus a constant is required to handle the offset. Over ACE's ~10 year period, neither annular mode index averages to 0. The same number of degrees of freedom are ultimately used when a constant is fitted during the regression or if the mean is removed from each of several basis functions (i.e. $c= d*c_1 + e*c_2 + f*c_3$, where c is the constant currently used in the regression, c_1, c_2, c_3 are the constants fitted if taking the approach of removing the mean from each basis function and d, e, and f are the fitting coefficients for three sample basis functions).

3) P22301 line 20-25 What's the meaning of the correlation between averaged sample latitudes in the high latitudes and corresponding annular index?

This statistic is used to test whether there is a temporal correlation between the latitudinal sampling of ACE and the corresponding annular index. If a strong correlation existed, the latitudinal sampling could interfere with our determination of the response to the annular mode (as indicated on p22301L26). We made the following changes:

(P22301L26) "this is tested" -> "this is tested below".

(P22301L21) "2.1" -> "2.4"

4) P22308 section 4.1 I think _10 years of data is too short to discuss the decadal trend.

It is clear from P22308L21 that by 'decadal', we mean a period of 10 years (2004-2013). Also, we do not discuss results for the decadal trend, we focus on the improved trend uncertainty when using the annular mode as an additional basis function. The trend uncertainty reduction resulting from the inclusion of an annular mode index as an additional basis function applies to longer datasets, but the improvement will tend to be smaller.

5) The authors repeatedly emphasize the linear correlation is somewhat larger on seasonal time scale than that on monthly time scale. Since the degree of freedom is reduced based on the seasonal-mean data, how important or meaningful by comparing these two correlation coefficients?

The reviewer is correct: while the correlation coefficients can be compared, the statistical significance of each correlation depends on the sample size. Instead, we compared the relative standard error of the fitting coefficient for the annular mode using monthly and seasonal timesteps. For MAESTRO, we find a slight improvement in the relative standard error as well, so the seasonal timestep is preferred for Fig. 7.

We have deleted the following sentence (P22304L17):

“Stronger anti-correlation (R = -0.68) at the seasonal timescale is also found for ACE-FTS water vapour at 7.5±0.5 km, the altitude of its strongest anti-correlation with the AAO index.”

as well as (P22306L24):

“This likely partly explains why a larger anti-correlation between southern high-latitude UTWV and the AAO index is found when a seasonal timestep is used.”

We now write (P22304L15):

“…, it is observed that the relative standard error on the AAO fitting coefficient is reduced when the regression is performed using a seasonal timestep rather than a monthly timestep.”