Response to reviewer 3

We thank the reviewer for looking closely at all aspects of this paper. In a nutshell, we have reorganized the paper to make the purpose of the study clearer. We have also completely removed the section on Nabro, given that the enhancement appeared to be tropospheric even at mid-latitudes and thus was not large enough in magnitude to persist following transport to northern high latitudes, given the short residence time of UTWV, particularly for mid-latitude summer.

I think the authors address an interesting question but the presentation is confusing and the physical argumentation unclear, such that I could not really follow the line of thoughts.

We agree that the presentation could be more clear. We try to improve the connection between successive thoughts. We have also removed the Nabro section (see below).

We now write:

(p25879L11) To connect the clearly enhanced UTWV at southern high latitudes to the eruption of Puyehue-Cordón Caulle (Puyehue hereafter), UTWV profiles in the 40-60°S band, which contain the latitude of this volcano, were contrasted between July 2011 and July 2012 (a normal July). Figure 5 shows a statistically significant increase in UTWV in the 40-60°S latitude band as well for July 2011 relative to July 2012, and no significant increase above 10 km.

and add:

(p25879L16) "…in July 2011..."

and change:

(p25880L1) "is advected to" -> "resides in"

I recommend a complete rewriting of the paper after the authors have carefully reconsidered how they think that volcanic emission can impact upper tropospheric humidity in remote areas on time scales of several weeks.

For Eyjafjallajökull, while the water vapour enhancement in July 2010 at 11.5 km is largest in terms of rank for that altitude and calendar month, the enhancement is not significant for MAESTRO relative to the standard error of the July 2010 monthly mean at 11.5 km. Thus, for this eruption, the period of significant enhancement is only May 2010 (during which the eruptive phase was ongoing and within the northern high-latitude band) and thus the timescale is not questionable given the lifetime of UTWV of ~21 days (Ehhalt, 1973), discussed below.
The last two sentences of Section 3.3 now become:

ACE does not sample northern high latitudes in June. In July 2010, enhanced UTWV is observed by both instruments only at the local tropopause (11.5 km), but for MAESTRO, this enhancement is not statistically significant.

For Puyehue, in our reply to the comment on Trenberth (1998) below, we show that the UTWV enhancement observed in July-August 2011 is consistent with the residence time of water vapour at the tropopause (which was located at 9.5 km averaging over July 2011 for example), and we now write at p25880L12:

"whereas in September 2011, the UTWV enhancement is statistically insignificant."

For Nabro, we now believe the conclusion that the enhancement at northern high latitudes was mostly due to this eruption is incorrect. In both July and September 2011 at northern mid-latitudes, there is a positive anomaly in water vapour of 2 ppmv at 13.5 km. This anomaly is consistent both in terms of the absolute magnitude and altitude and no other altitude shows a significant enhancement in either month.
The July 2011 mid-latitude enhancement of 2.4±2.2 ppmv appears to be tropospheric after separating profiles from this month and latitude bin into two groups:

A) with a tropopause ≤13.5 km, and
B) with a tropopause >13.5 km

Only group B showed a clear RH enhancement at 13.5 km and it was vertically narrow feature (spanning 12.0-14.0 km). Since it appears to be tropospheric, even if it was entirely due to Nabro, it would be likely reduced to ~0.2 ppmv by mid-September assuming a tropopause residence time of 3 weeks (which may be too long since the tropopause was >13.5 km in 26% of the July 2011 mid-latitude MAESTRO profiles). If this mid-latitude enhancement were to cross the tropopause as it was transported poleward, it would suffer the least depletion if it were transported to the lowermost stratosphere as early as possible.
A stronger argument against Nabro being the main source of humidity at northern high latitudes in September 2011 is that the enhancement at 10.5 km at northern high latitudes is significant for both instruments and the absolute magnitude is 5.7 and 10.8 ppmv for ACE-FTS and MAESTRO, respectively. These numbers greatly exceed the mid-latitude enhancement of ~2 ppm at 13.5 km. This supports the idea that the high-latitude enhancement at 10.5 km was not primarily due to Nabro since those enhancements appear to be too large to be related to a 2 ppm mid-latitude enhancement and a different mechanism (e.g. deep convection) is likely the main one.
Figure 13 of the original manuscript shows that in September 2011 at northern high latitudes, MAESTRO sees a statistically insignificant enhancement of 2.7 ppmv at 12.5 km considering 1 km vertical bins and that ACE-FTS shows an enhancement of 1.4±0.9 ppmv at this altitude. We removed the single September 2011 high-latitude profile which had a tropopause above 12 km (Figure 12), which resulted in a reduction of the ACE-FTS monthly median at 12.5 km by 0.01 ppmv. The mid-latitude enhancement in July 2011 does not appear to be sufficient to explain the September 2011 enhancement at 12.5 km at high latitudes, accounting for a residence time appropriate to the tropopause (3 weeks).
Furthermore, examining MAESTRO water vapour profiles from September 2011 in the 30-60°N band, the enhancement at 13.5 km appeared to be coming from profiles in the 50-60°N latitude range where there was a sharp gradient in both RH and water vapour VMR at 13.5 km while the lower latitudes (30-50°N) showed no sign of a vertically confined enhancement at 13.5 km. The enhancement at 13.5 km in the 50-60°N data appeared to be tropospheric in origin and most

apparent in profiles with the highest tropopauses (e.g. 13.5 km, compared to the normal value of 11.5 km). The enhanced monthly zonal median in September 2011 at northern mid-latitudes is likely due to deep convection, which pushes up the local tropopause, and results in high water vapour VMR and RH near 100% through the entire upper troposphere, as shown in a sample profile below (Fig. 1).

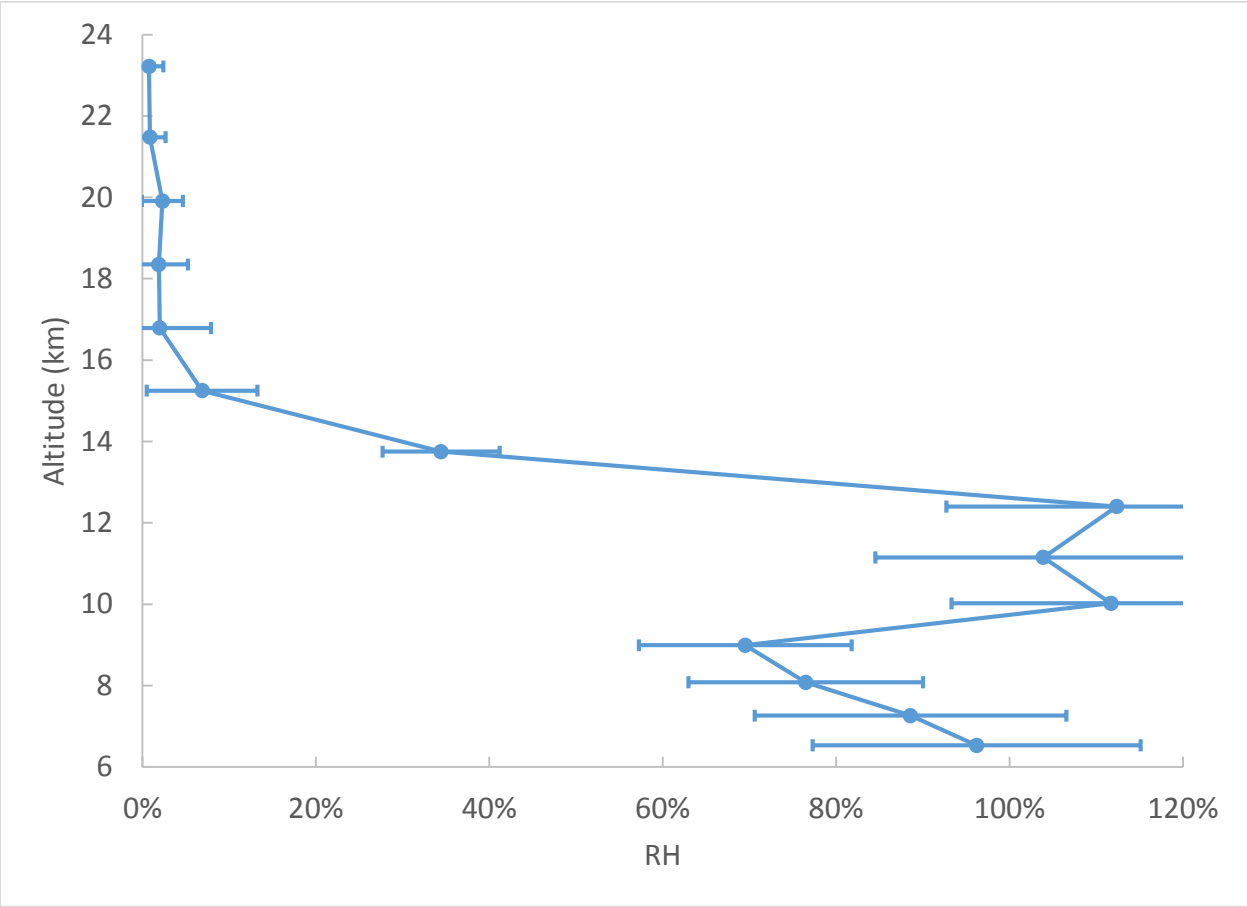

Figure 1. RH profile for sr43466 on Sept. 8th, 2011 at 50.5°N derived using MAESTRO water vapour.

We believe, as suggested by reviewer 1, that unusually deep convection in the summer of 2011 is more likely to explain the positive anomaly in the high-latitude tropopause region observed in September 2011. The northern mid-latitude UTWV enhancements, particularly in September 2011, are also more likely to be due to unusually deep convection (Schwartz et al., 2013) than from a low latitude volcano months earlier. Also, MLS observations of the low-latitude Nabro plume one day after eruption indicate that enhanced water vapour reached the tropopause but essentially did not appear to have gone any higher (see response to reviewer 1), indicating that if the volcanic enhancement reached the stratosphere, it would have been during subsequent poleward transport, meaning the water vapour enhancement resided initially in the low latitude upper troposphere where precipitation could deplete it more quickly.

We have deleted Sect. 3.2 and all of the discussion regarding volcanogenic water vapour in the stratosphere.

1) General: I found nowhere a good explanation of why this study focuses on water vapour in high latitudes.

The motivation for studying high latitude UTWV is provided at the end of Sect. 1:

Currently, trends in UTWV are not known for high latitudes (Hartmann et al., 2013). However, the main focus of this work is on improving our understanding of UTWV variability at high latitudes and the role of volcanic emissions relative to other dynamical and thermodynamic processes in this region (see companion paper: Sioris et al., 2015).

Instead of the second sentence of this excerpt, we now write:

The first step toward accurate trends is to improve our understanding of UTWV variability at high latitudes. The variability of upper tropospheric water vapour (UTWV) at high latitudes is dominated by dynamics (Sioris et al., 2015). In this companion paper, a second phenomenon is identified that contributes secondarily to the variability of UTWV: volcanic emissions. The role of volcanic emissions relative to other dynamical and thermodynamic processes in this region on monthly timescales is an open question which motivates this study.

Nabro is close to the equator and Puyehue at 40degS – would it not be much more intuitive to first look at water vapour profiles close to the eruptions?

We did not first look at water vapour profiles close to the eruptions since we did not know that volcanoes perturbed upper tropospheric water vapour on monthly timescales until we compiled our monthly anomaly time series versus altitude (at high latitudes) and then tried to understand the processes responsible for this variance. Similarly, in the companion paper, we did not set out to prove that the hypothesis that the annular modes are a dominate source of variability. These hypotheses only came after having plots of monthly anomaly time series at hand.
We were not explicit about the timescale of interest, but the duration of the volcanic impacts to UTWV was mentioned in the paper and in the abstract. Also our interest in the climatic impact, assessed through cooling rate simulations, also involves a monthly timescale. With the rewording of the introduction (in response to the previous comment), we are now stating the timescale of interest explicitly. We have also removed the reference to Murcray et al. (1981), which should help avoid giving the initial impression to the reader that we were focussed on shorter timescales (days).
Nevertheless, since it is very likely that Puyehue was responsible for the sudden and top-ranking positive anomaly (approximately +50%) in ACE-observed southern high-latitude UTWV in the 2011 austral winter (July-August), it also made sense to us to look at ACE water vapour profiles close to Puyehue (thus our Fig. 5) and MAESTRO-based RH profiles as close to Puyehue in space and time as possible (e.g. Fig. 7) to understand the phase of the water. Reviewer 1 made

the same suggestion as Reviewer 3 and so we analyzed MLS SO_2 and H_2O data of the Nabro plume one day after eruption (see response to Reviewer 1). In writing the original manuscript, we looked at ACE mid-latitude (30-60°N) water vapour profiles close to Nabro in July (including early July) but there are no ACE occultation events that are spatially very close to the Nabro plume in June. We did not look at low latitudes in July 2011, and fortunately there is one ACE occultation event (ss42439) that, by fluke, fell in the Asian monsoon region (26°N, 45°W). There is clearly reduced signal-to-noise in the two ACE-FTS spectra from this occultation event at 13.5 and 15.6 km which led to rejection of these spectra from the operational v3.5 processing. These spectra were included in the ACE-FTS water vapour retrieval by Chris Boone in response to this comment (shown below). Similarly, at 14.1 and 15.1 km, MAESTRO measures water vapour with >100% uncertainty due to a overlying, unusually thick cloud and/or aerosol layer with 560 nm aerosol extinction peaking at 16.2 km (see Fig. 2 below). Water vapour above 15.1 km is below MAESTRO's lower detection limit for this occultation and is likely due to the reduced signal as a result of this overlying "cloud". At 13.1 km, MAESTRO measures 92±88 ppmv of water vapour, which translates to a relative humidity of 48±46%. This implies, in spite of the huge measurement uncertainty, that the conditions are not favourable for homogeneous nucleation of ice. In the spectrum immediately below 13.1 km (at 12.2 km), MAESTRO observes a much stronger water vapour absorption signature that is likely due to an spatial inhomogeneity between spectra measured below and above 13 km. In any case, the MAESTRO water vapour retrieval does not converge below 13 km. The ACE-FTS water vapour profile (Fig. 3 below) is consistent with the MAESTRO observation at 13.1 km: at 12.9 km, ACE-FTS measures 33±5 ppmv of water vapour, again implying unsaturated air. The thermal tropopause is at 17.5 km or 93.9 mb, where ACE-FTS water vapour is 9.9±0.2 ppmv, clearly an anomalously high value (99^{th} percentile) in the context of Fig. 2 of Schwartz et al. (2013). Saturated air exists at the tropopause according to FTS, whereas at 16.0 km, the RH inferred from ACE-FTS water vapour is 39±13%. The cloud+aerosol extinction peak is nearer to 16.0 km however, according to ACE NIR-Imager (15.5 km) and MAESTRO (16.2 km), implying a likely contribution from Nabro aerosols to the observed cloud/aerosols at 16 km.

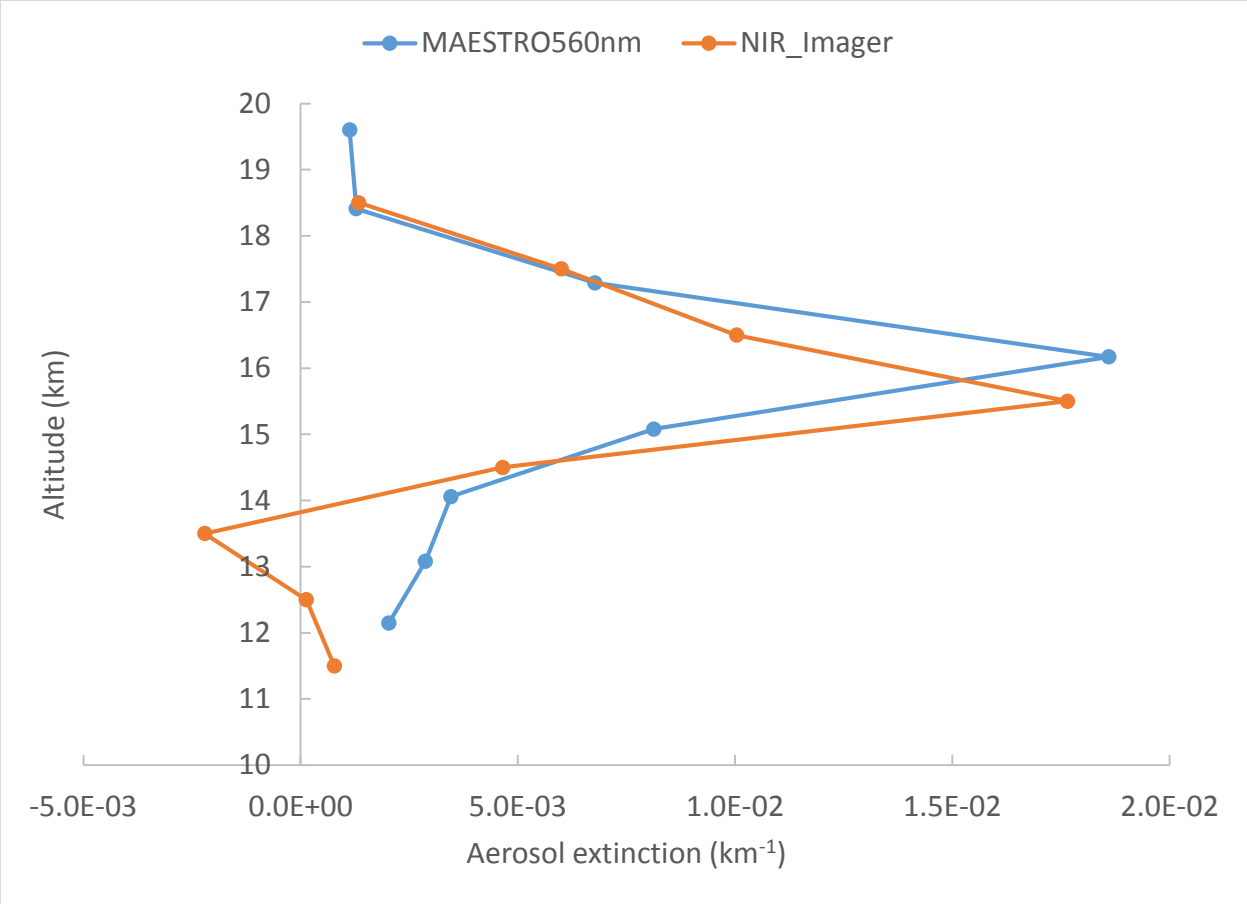

Figure 2. Aerosol extinction profiles observed by ACE instruments during ss42439 at 26°N, 45°W on 1 July 2011. Both instruments are shown to illustrate the agreement on profile shape and peak height. Conclusions regarding the Ångström exponent from single profiles should be avoided, partly due to difference in the size and shape of the field-of-view given possible aerosol/cloud heterogeneity over the width of the sun at the tangent point (~20 km).

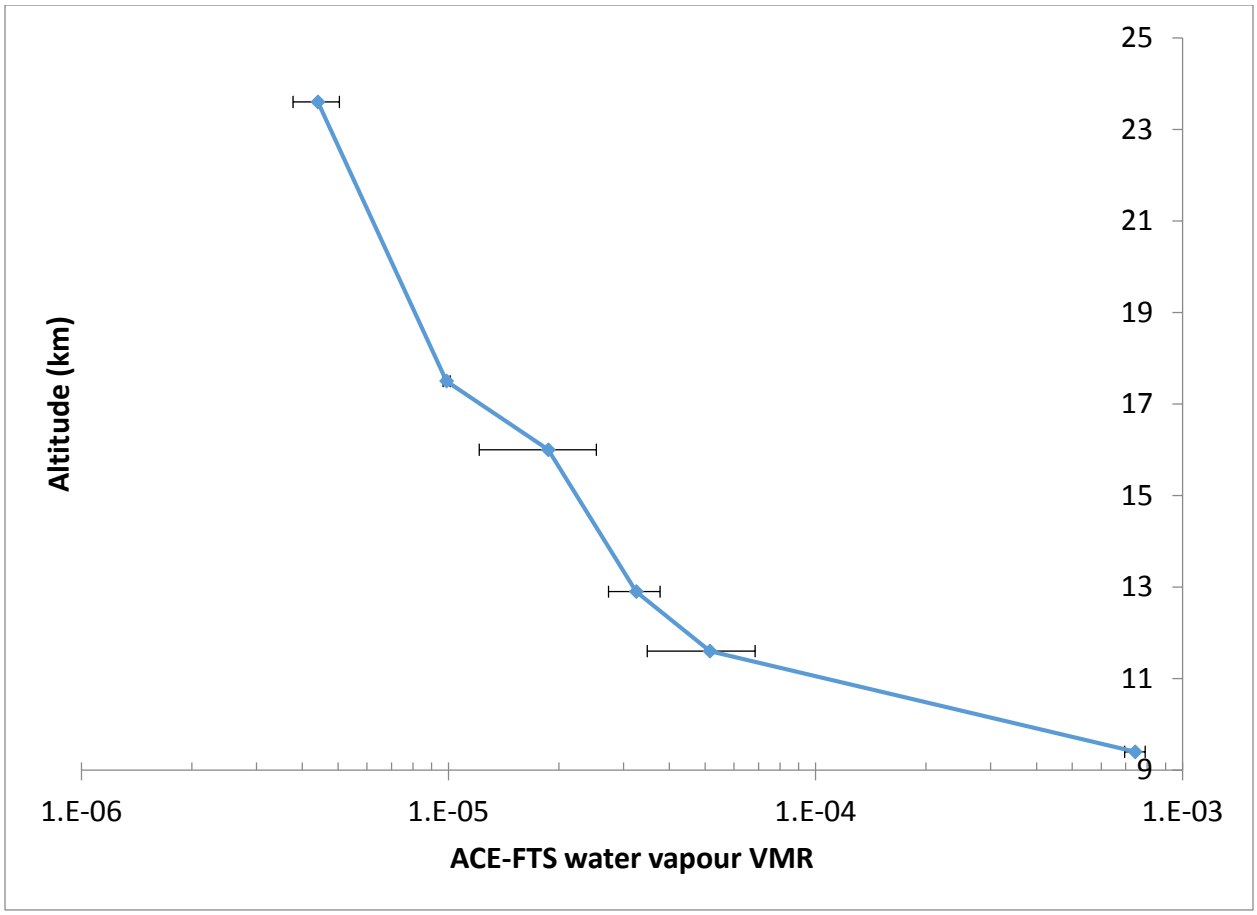

Figure 3. ACE-FTS water vapour profile in the UTLS during ss42439.

However, our main focus, as stated in the title, is on high latitudes, particularly understanding UTWV variability, so observations close to the volcanoes (in space and time) is examined to make a stronger case that the high latitude variability could be due to the eruptions.

What is your argumentation that water vapour emitted near the equator should reach the polar regions (see also comment 2)?

The reviewer's question pertains to Nabro. As discussed above, we removed these arguments from the paper.

The authors should explain how many profiles would be available to look at the surroundings of the volcanoes and why they decided to not look at them (except for one profile in Fig. 7).

For Nabro, as mentioned above, there is only one profile in the surroundings of the volcano in the first 18 days after eruption. There are no processed ACE occultation events in June 2011 in the southern hemisphere, following the Puyehue eruption. By July 2011, it is known that the Puyehue volcanic plume had already circumnavigated the globe twice (Vernier et al., 2013), so zonal (40-60°S) July 2011 median data are preferable as shown in Fig. 5 of the manuscript. For Eyjafjallajökull, there are many profiles in the surroundings of the volcano, however a profile on

May 16th, 2010 60.1°, 6.7°W shows no enhancement relative to the zonal monthly median at all heights below the hygropause (~10.5 km) but it is 11 days after the second major eruption (Gudmundsson et al., 2012). In April 2010, after the initial major Eyjafjallajökull eruption, ACE observations are in the southern tropics and high latitudes. So there are no observations in the surroundings of Eyjafjallajökull within 10 days of one of its two major eruptions in spring 2011.

2.) General: It is not trivial for water vapour emitted in the tropical/midlatitude troposphere to reach the polar upper troposphere. As long as the air is not saturated transport is along isentropes which slope upward towards the pole. Therefore air parcels moving poleward from Nabro or Puyehue are expected to experience adiabatic cooling, leading to cloud formation and rainout. Since I assume that the emitted air from the volcanoes is humid, it requires only a minor lifting to reach saturation and cloud/rain formation.

There are two steps required if the reviewer's comment is valid. The first one is that adiabatic cooling is sufficient for cloud formation. This is probably correct based on calculations for Puyehue humidity at 7.5 km assuming a 1 km rise at the dry adiabatic lapse rate. The second is that cloud formation leads to significant precipitation. Cloud droplets need to grow sufficiently before they begin to fall. The reviewer's comment applies only in some places at some times. In fact, saturation, which is a condition for cloud formation, is rather rare in the southern high latitude upper troposphere in austral winter 2011 (see discussion at bottom of p25879 continuing to top of p25880). Furthermore, the precipitate would like be in the form of tiny ice crystals (not rain) which could vaporize before falling too far down given the warmer temperatures below. We infer that saturation/condensation did occur in some Puyehue observations (p25880L12).

For the Nabro case, there is another assumption that the reviewer is making which appears to be completely false: according to Bourassa et al. (2012), the isentropes slope downward toward the North Pole in summer of 2011. Therefore, for Nabro, air parcels moving poleward are expected to experience adiabatic heating, potentially leading to melting of ice coatings on volcanic aerosols and a local increase in water vapour.

We deleted the Nabro section and added to the Puyehue-related discussion in Sect. 4 (provided in a reply below).

In other words, water vapour cannot be transported easily from the tropics
to the polar upper troposphere without being deposited at the ground via precipitation.

We agree. By deleting the Nabro section, this comment is addressed.

Therefore studies on the typical tropospheric residence of water estimate values of a few days (e.g., Trenberth, K. E. (1998). Atmospheric moisture residence times and cycling: Implications for rainfall rates and climate change. Climatic Change, 39(4), 667-694, and several other/more recent studies on this topic).

The analysis by Trenberth (1998) is not vertically resolved. Since almost all of the water vapour is below 5 km, even at polar latitudes, Trenberth (1998) effectively provided the residence time in the lower troposphere. Thus, this reference is not very relevant. Nevertheless, Trenberth (1998) finds atmospheric moisture residence time of 30 days in the sub-tropics based on annual

means. 30 days is also a more reasonable residence time for the high-latitude upper troposphere where, similarly to the sub-tropics, precipitation is not effective at shortening residence time, particularly in winter where convection is very weak at high latitudes. The high-latitude upper troposphere should have longer residence times than the sub-tropics because of greater vertical stability in the former region. Brasseur et al. (1998) state that the water vapour residence time at the tropopause is "weeks". Freeman and Liou (1981) estimate the residence time of water vapour in the upper troposphere to be ~30 days. Support for these quoted residence times could not be found. Fortunately, Ehhalt (1973) determined the residence time versus altitude in the troposphere using tritiated water measurements. At the tropopause, his estimate was three weeks based on winter and spring measurements at a mid-latitude site (Nebraska, 42°N). An additional minor point is that dry removal of water vapour is expected to be less efficient at high latitudes due to the greater atmospheric stability and reduced surface area of snow versus forest (Prospero et al., 1983). Vertically-resolved moisture residence times inferred from Fig. 3 of Kennett and Toumi (2005) in the sub-tropics appear too short compared to Trenberth's 30-day estimate there. We disregard the residence times of Kennett and Toumi (2005). The data in their Fig. 3 was not available from the authors (Toumi, priv. communication). From their Fig. 3, large portions of the polar upper troposphere have a residence time of >5 days, but how much greater than 5 days cannot be said.
Assuming a 3 week residence time for UTWV (Ehhalt, 1973), a simple comparison of the MAESTRO-observed and simulated exponential decay shows good agreement (Fig. 4 below) at 8.5 km at southern high-latitudes, and there is good consistency between MAESTRO and ACE-FTS. Some differences between observed and simulated decays could be due to neglected monthly changes in wind velocity (and thus to the advection of water vapour to the southern high-latitude region). Also, in September, the temperature may be cold enough for condensation at 8.5 km which would shorten the residence time in the upper troposphere if sedimentation occurs or if the resulting ice crystals tend to remain the condensed phase during that month.

We now write at the end of the introduction:

Water vapour at the tropopause has a typical atmospheric residence time on the order of three weeks (Ehhalt, 1973; Brasseur et al., 1998) and is mostly removed by precipitation (Junge, 1963). The residence time decreases to ~2 weeks at an altitude of 5 km (Ehhalt, 1973) which limits the distance over which UTWV enhancements can be advected.

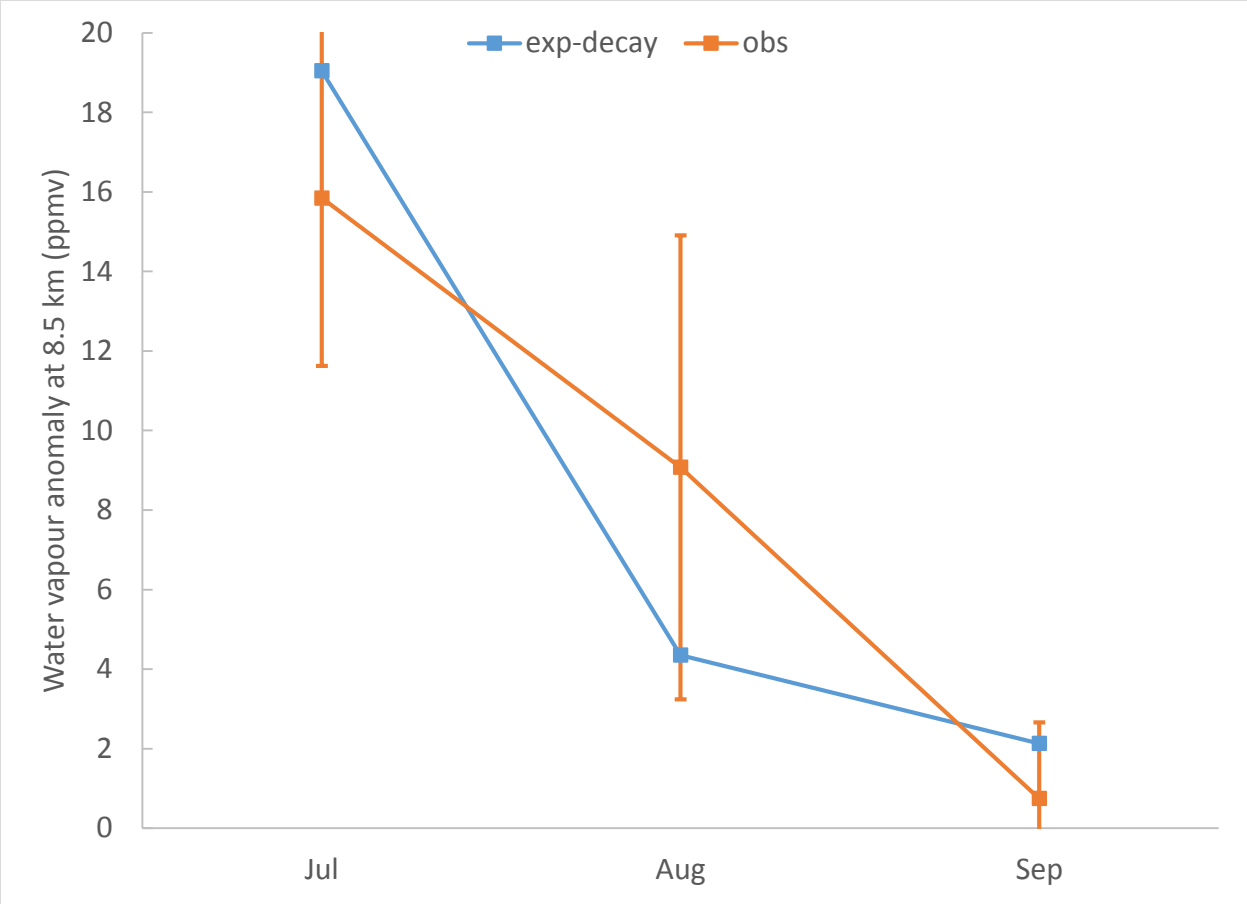

Figure 4. Water vapour median anomalies in 2011 austral winter at southern high latitudes, following the Puyehue eruption (Chile). The vertical error bar is the standard error of the 2011 monthly mean.

Your statement on p. 25879/80 "most of the water emitted ... will tend to remain in the vapour phase as it is advected to the southern high-latitude upper troposphere" is most likely wrong. I think with a simple parcel model, lifting a moist air mass to the upper troposphere, you could show that saturation would occur rather quickly.

As stated above, saturation can occur, but not necessarily precipitation and the precipitation will not completely remove the volcanic water vapour. Precipitation may vaporize before reaching the ground given the low ambient humidity. Ice coatings may form on aerosols as a result of saturation/condensation. This ice could vaporize later while the particles are still in the upper troposphere. As the plume disperses, the very high humidities rarefy, and saturation will tend to occur less: the saturation will tend to be mostly in the initial eruptive phase closer to the volcano. The extratropical upper troposphere has fast winds that help to disperse the humidity and keep RH<100%. The fast winds can be inferred from the fact that Puyehue circumnavigated the globe twice before the start of July 2011 (Vernier et al., 2013).

Of course an alternative pathway of water vapour transport is via the stratosphere. If the volcano injects water (most likely in form of ice particles) into the lower stratosphere, then this vapour can "survive" much longer without being trapped by clouds and could maybe make it to the polar regions.

We agree with the comment. However, very little material, if any, from Eyjafjallajökull reached the stratosphere. For Puyehue, according to the ACE observations, the water vapour in July 2011 at middle and high latitudes was in the upper troposphere. No observations in June are available in this region. Water could have fallen from the stratosphere as ice coatings on ash during the latter part of June, but the observed UTWV enhancement at mid-latitudes could be transported to southern high latitudes without necessitating entry into the stratosphere (see replies above and below). For Nabro, as mentioned, the section has been deleted.

No change is made to the Eyjafjallajökull or Puyehue sections of the paper.

But the paper remains very fuzzy about which transport pathway occurred, and I find it irritating that the aspect of saturation and cloud formation associated with poleward transport in the troposphere is never mentioned.

We thank the reviewer for the suggestion to be explicit about the transport pathway for Puyehue. We have now reworded (p25879L16) as follows:

The consistency of these ratio profiles between middle and high southern latitudes provides evidence of the poleward transport in the upper troposphere of water vapour emitted by the Puyehue eruption.

As we replied above, the saturation does not necessarily imply complete removal from the atmosphere or even from the upper troposphere. With the rise in altitude, the resulting ice crystals may fall, but the lapse rate that led to their condensation also means that warmer temperatures exist below which can result in the rapid vaporization of these ice crystals given their small size, as expected for ice crystals formed at the low specific humidity of the upper troposphere. The net effect is that air is transported poleward with little change in the water vapour profile due to upward sloping isentropes.

We now write in Sect. 4 where Puyehue is discussed:

During poleward transport, air parcels follow isentropes typically to higher altitudes. Such transport involves adiabatic cooling which can lead to saturation. However, the saturation does not necessarily imply complete removal from the atmosphere or even the upper troposphere. With the rise in altitude, the resulting ice crystals may fall, but they may be vaporized very quickly given their small size and the warmer temperatures below with the net effect being that air parcel transported poleward on an upward sloping isentrope may experience little change in the vertical profile of the water vapour enhancement.

For Nabro, we reiterate that the section has been deleted.

3) General and in line with comment 2: the authors sometimes compare aerosol signals with water vapour signals, and they seem to conclude that when the volcanic aerosol plume reaches the high latitudes, that then an observed water vapour enhancement is also due to the volcanic plume. Again, water vapour is rather short-lived in the troposphere and responds differently to cloud formation and rainout than aerosols. Therefore I would be much more careful with linking volcanic aerosol plumes to water vapour signals.

The reviewer is correct that there is a danger in concluding that when the volcanic aerosol plume reaches high latitudes, then the water vapour enhancement is also due to the volcanic plume. But for both Puyehue and Nabro, we did not conclude solely on this fact so we were "more careful". For both cases, we showed an enhancement in water vapour at mid latitude at a consistent altitude with the enhancement at high latitudes. However, in the case of Nabro, our original conclusion based on both of these facts is incorrect and the section has been deleted. Thus the comment will be addressed as it pertains to Puyehue. Water vapour and aerosols do not have identical lifetimes in the upper troposphere but they are very similar: aerosols have a residence time there of 30 days (Prospero et al., 1983; Pruppacher and Klett, 2010) and this is not surprising given that the main mechanism for their removal, namely precipitation, is the same for both constituents. With each volcano, we are using aerosols as a volcanic proxy. In other words, if we observed enhanced water vapour in the upper troposphere without the presence of a volcanic aerosol layer, we would reject the notion that the water vapour enhancement was volcanogenic. Conversely, we understand conversely that volcanic aerosols may exist, particularly in the stratosphere, without an accompanying water vapour enhancement. Because aerosols appear to have a slightly longer residence time in the upper troposphere, they could be also remain there while the water vapour enhancement could have been more quickly depleted. Fig. 6 of the paper illustrates the monthly zonal mean aerosol extinction in July 2011 to demonstrate that a volcanic aerosol layer was "initially" present in the upper troposphere at southern high latitudes. The temporal evolution of the Puyehue UTWV enhancement at southern high latitudes is consistent with a residence time of three weeks as illustrated above (Fig. 4).

We now write in Sect. 3.1 (p25880L9):

The decrease over these winter months is consistent with the lifetime of water vapour in the upper troposphere (Ehhalt, 1973).

4) p. 25874 line 15: for most readers of ACP the volcanic explosivity index is not known. Therefore mentioning the index value for one eruption (but not for the others) and without a more general context is not useful in the abstract.

A similar comment was made by Mike Fromm so we cite Newhall and Self (1982) in the revised manuscript. We no longer mention VEI in the abstract.

5) p. 25875 line 4: what do you mean by "in theory"? I don't think that there is a theory about this topic.

The reviewer seems to have missed the references provided at p25875L4. While we chose the phrase "in theory", it appears the authors of one of the two cited papers also uses the same language. Consider the first and third sentences of the abstract of Glaze et al. (1997):

Contrary to assumptions often made in the literature, explosive volcanic eruptions are capable of transporting significant amounts of water into the stratosphere. (…) A theoretical model for the conservation of mass, momentum, and thermal energy of four separate components (dry air, water vapor, liquid condensates and solid particles) is used to determine the extent of atmospheric water redistribution.

Also, from their conclusion section:

The theoretical results address two important issues concerning water vapor transport: (1) the extent to which volcanic eruption columns are capable of entraining water vapor at lower levels and (2) whether or not volcanic columns are capable of injecting significant amounts of water into the stratosphere.

We have deleted the paragraph containing “in theory”.

6) p. 25875 line 15: here you mention an indirect effect: volcanic eruption –> temperature change –> humidity change. What I am missing here, is a systematic summary of different processes of how volcanic eruptions may influence tropospheric humidity and on what time scales (direct emission, transport, indirect effects via temperature, pathway via the stratosphere, ...).

This line has been deleted. Discussion of a stratospheric pathway is not very relevant to the revised manuscript. One indirect effect via temperature and the related reference to Soden et al. (2002) has been moved to the introduction. We add the timescale to this sentence as follows:

UTWV was observed to decrease following the Pinatubo eruption due to global cooling below the tropopause and did not return to normal levels for two years (Soden et al., 2002).

We also add the following sentences on a temperature-related mechanism which could enhance UTWV following a volcanic eruption:

For volcanoes with an eruption height at or below tropopause, local warming by radiation-absorbing volcanic aerosols such as ash can lead to local increases in water vapour. The timescale of UTWV enhancement due to such a thermal mechanism would be controlled by rainout and fallout of the aerosol, which is on the order of ~1 month (Prospero, 1983; Pruppacher and Klett, 2010) for particles of intermediate size (~0.3 μm).

The residence time of UTWV and how it limits the contribution by volcanic eruptions at lower latitudes is now provided in the introduction as well (see above).

7) p. 25875 line 17: what do you mean by "remain in the ... data": is it persistent feature over many years?

What is meant is that the data have been reprocessed (Hurst et al., 2011) but the feature at 24-26 km remains.

This sentence has been deleted.

8) p. 25875 line 23: this sentence is very long, contains different things and is confusing. Please try to write in a clearer way.

This sentence and the entire stratospheric discussion has been deleted.

9) General: I find it strange that the coordinates of the volcanoes are never given. This is important information.

We agree with the reviewer and have added the coordinates of the two volcanoes in the first sentence of Sect. 3.1 and what is now Sect 3.2:

The Puyehue-Cordón Caulle volcano (40.59°S, 72.12°W) erupted explosively in early June of 2011.

and

Eyjafjallajökull (63.63°N, 19.62°E) began erupting on (…)

10) p. 25876 line 14: cf. comment 1): Why do you mention here only high latitudes?

See reply to General comment 1) (above). A second reason for the high-latitude focus is that the ACE orbit is more suited for studying processes there as compared to low latitudes.

11) p. 25876 line 19: Bernath et al. is not in the list of references.

Thanks to the reviewer for spotting this. The reference has been added.

12) Figures 1 and 2: the caption of Fig. 1 mentions VMR (of what?).

The Fig. 1 caption now reads:

Comparison of global median water vapour VMRs from MAESTRO (blue) and ACE-FTS (black) (…)

What should the reader learn from Fig. 2? I was confused by the many lines, instruments, errors ...please help the reader to understand what is relevant for this study.

What the reader should learn from Fig. 2 was already provided in eight lines beginning at p25877L5. This is a conventional validation figure. The many lines correspond to the many instruments measuring UTLS water vapour profiles. The reader can see that MAESTRO and ACE-FTS agree fairly well and ACE-FTS and MIPAS-IMK agree fairly well in their respective coincidences. The middle panel shows that the rest of the instruments have large biases that appear at 12 km, which is typically the tropopause. The right panel ultimately shows that MAESTRO has lacks precision in the stratosphere (but is not biased according to the middle panel). Only the differences between ACE-FTS and SMR exhibit more scatter. In summary, the middle panel tells the reader about biases and the right panel ultimately relates more to precision of the correlative instrument (given the very high precision of ACE-FTS).

It is also irritating that only the caption of Fig. 2 mentions the vertical resolution of the data. I never found this discussed in the text!

We have inserted the following information on the vertical resolution of the MAESTRO water vapour profiles at P25877L3:

The water vapour profiles have ~1 km vertical resolution (Sioris et al., 2010).

and for ACE-FTS at P25877L22:

ACE-FTS gridded version 3.5 water vapour profiles are used in the study (Boone et al., 2013) and are assumed to have 3 km vertical resolution.

The fourth sentence of the Fig. 2 caption now reads:

The profiles from the instrument with the coarser vertical resolution are smoothed to account for the difference in resolution between ACE-FTS and the correlative instrument.

14) p. 25878 line 8: I am not sure that your course analysis of the tropopause height is relevant.

We are not sure what is meant by "course analysis", but the tropopause definition must be provided to the readers. Presumably, the reviewer would like finer vertical resolution of the tropopause height ("course" -> coarse). Tropopause information is used particularly for the Eyjafjallajökull case study where the water vapour enhancement extended up to the local tropopause and is relevant in light of longer residence times for water vapour in the stratosphere. The tropopause height information comes from the GEM model which has comparable vertical resolution to MAESTRO. It is a virtue that MAESTRO and GEM vertical resolution is very similar. MAESTRO and ACE-FTS are both capable of measuring temperature profiles but there is not an operational temperature profile product for either instrument at 10 km.

No change is made to the manuscript.

Also Fig. 12 does not contain very interesting information. I think it would be sufficient to mention that the tropopause height varies between X and Y km.

Fig. 12 has been deleted as has the entire section on Nabro.

15) p. 25878 lines 13: I don't understand this paragraph. "20 observations per altitude bin per month": is this at a particular point or somewhere in the 60-90deg latitude band? In case of the latter, then I doubt that 20 observations are enough to obtain representative monthly mean, high-latitude averaged profiles.

The reviewer is correct. A circle around the Earth at a constant 60° latitude has a circumference of 20000 km. In order to cover all longitudes, given the spatial correlation length of water vapour at the tropopause of 400 km (Offermann et al., 2002) would require a minimum of 50 observations.
Of relevance, in 2011, there are 111, 65, 70 successfully retrieved MAESTRO water vapour profiles for July, August, and September, respectively, at southern high latitudes to study the impact of the Puyehue eruption. For Eyjafjallajökull, there are 132 profiles at northern high latitudes in May 2010. The number of ACE-FTS profiles in any given month always exceeds the number of MAESTRO profiles. The climatologies from each instrument are based on ~1000 profiles since there are typically 9 populated years for each calendar month. The number of profiles used has been added to each caption (see below).

To be clearer, we now write:

The monthly climatology, used to deseasonalize the time series, is generated by averaging the monthly medians over the populated years, with a minimum sample size of 20 observations per altitude bin in each individual month.

This section now ends with the following statement on sample sizes:

For the case studies presented next, there are at least 65 profiles measured by MAESTRO and by ACE-FTS for each month in the July-September 2011 period at southern high latitudes (Puyehue-Cordón Caulle) and for May 2010 at northern high latitudes (Eyjafjallajökull).

16) Figure 4 is an important figure, but I am not sure that it is consistent with Fig. 3. Figure 3 shows an enormous peak in spring 2007 at 7.5 and 8.5 km, but this is not seen in Fig. 4, which I find very irritating. Since the scale in Fig. 3 is a log-scale, this peak should lead to a very prominent anomaly in Fig. 4(?).

The two figures are entirely consistent. Figure 3 of the manuscript shows absolute quantities (monthly mean water vapour mixing ratios). Figure 4 shows relative anomalies so the large VMRs in January (austral summer) that occur annually have been deseasonalized. Thus Figure 4 shows interannual variability only (which is true for any such figure showing relative anomalies). We agree that Fig. 4 is important because it shows that at southern high latitudes, the upper troposphere has low interannual variability even sampled at a monthly timescale (standard deviation of 20%). This low interannual variability allows for a ~50% change in UTWV due to a volcanic eruption such as Puyehue to stand out very clearly.

17) Section 3.1: I found it very difficult to understand the presentation and discussion of the results in this section (which is the core part of the paper). The discussion jumps from high latitudes (60-90S) to the band from 40-60S, from aerosols to water vapour, from a single profile (Fig. 7) to monthly means, from VMR to relative humidity ... this really did not help to understand the story and to find the story convincing. Please help the reader much better to follow your line of thoughts.

We have added the following sentence to help guide the reader at P25879L11:

To connect the clearly enhanced UTWV at southern high latitudes to the eruption of Puyehue-Cordón Caulle (Puyehue hereafter), UTWV profiles in the 40-60°S band, which contains the latitude of this volcano, were contrasted between July 2011 and July 2012 (a normal July).

The reason to discuss the aerosol extinction profiles at mid and high latitudes is that aerosols serve as a volcanic proxy. In July at southern high-latitudes, it is difficult to imagine anything other than a volcano producing a widespread layer near the tropopause (as evidenced by the nearly equal median and mean extinctions in Fig. 6 of the manuscript). Because of the generally low relative humidity in the upper troposphere in July (austral winter), cirrus would not be omnipresent and would have much larger differences between monthly median and mean extinction as is seen for the polar stratospheric clouds at ~20 km. Furthermore, the aerosol extinction peak height at high latitudes is similar to the mid-latitude peak height (~9 km). The reader is already provided with the purpose of Fig. 6 (p25879L24):

"…corroborates the volcanic origin of the water vapour enhancement."

We have removed the single mid-latitude RH profile from 01 July 2011 and replaced it with a median RH profile for the 40-60°S latitude band in early July 2011. We appreciate the reviewer's suggestion to do this.

RH is needed to determine saturation. We do not feel that the RH should be used throughout the paper however since it is not the retrieved quantity from the ACE instruments (i.e. depends on GEM temperature and pressure) and can reflect temperature changes as well as water vapour changes. Water vapour relative anomalies are equally useful for manifesting the sudden changes in UTWV arising from volcanic eruptions.

To justify the use of RH, we now write at p25880L2:

RH profiles (Fig. 7) are used to emphasize that most of the water emitted from the volcanic eruption will tend to remain in the vapour phase as it resides in the southern high-latitude upper troposphere.

18) p. 25880 line 18: I don't understand why there is this sentence about cooling rates at the surface in the paper - also the appendix does not help to understand what has been done and why.

One of the reasons for studying UTWV is that it is effective at trapping longwave radiation, which can lead to warmer temperatures at the surface. In the second sentence of the introduction in the original manuscript, we state that water vapour is effective at trapping infrared radiation, particularly when it is located near the tropopause.

We have now made it the first sentence of the paper.

The paper shows that volcanic emissions can increase UTWV significantly for a period of a month or two. But, we wanted to take this one step further and address the obvious climatic question of whether the surface temperature would be affected by volcanic emissions of water vapour on such a timescale.

Also, we have added a sentence to the introduction to help clarify why the Antarctic oscillation would be included as a basis function in the multiple linear regression discussed in the appendix:

The variability of upper tropospheric water vapour (UTWV) at high latitudes is dominated by dynamics (Sioris et al., 2015).

In order to understand why the cooling rate differences were simulated, we now start the appendix with:

In order to investigate the impact on volcanic UTWV enhancements on surface temperature, (..)

We also added a final sentence to the appendix to clarify the approach:

The use of a multiple linear regression adjusts for a minor contribution by the Antarctic oscillation to the July 2011 UTWV enhancement.

19) General: I find the quality of the figures rather low. For instance, there are often no axis ticks and therefore it is not clear, e.g., in Fig. 3 where 20, 30, ... ppm are. Also in Fig. 3 some vertical lines would help a lot to attribute the values to a particular month. Some figure captions are specific about the region, others are not. I think every figure caption showing a profile should indicate how many profiles have been averaged to produce the profile shown.

Ticks have been added to both axes of all figures, except for the x-axis of Fig. 3 for which vertical gridlines separate adjacent months. Every second available month is labelled (January, April, July, September) so labels are not present for March, May, August, and November. In addition to the vertical gridlines, markers have been added to the four curves (i.e. altitudes) to make the months easier to distinguish.

For the Fig. 2 caption, we now write:

(…) Number of coincidences globally (…)

Every other figure caption was specific about the region.

For each figure containing a vertical profile, we have added the number of profiles as follows:

Figure 1. Comparison of global median water vapour VMRs from MAESTRO (blue) and ACE-FTS (black) (N=15000).

Figure 5. Enhancement factor for water vapour mixing ratio in July 2011 in the 40-60°S band (July 1-July 12, N=78) and the 60-66°S band (July 13-July 31, N=181) (…)

Figure 6. ACE-Imager median and average near-infrared (NIR, 1.02 μm) aerosol extinction profiles for July 2011 at southern high latitudes (N=163).

Figure 7. Relative humidity for July 2011 (40-60°S, N=52) and (60-66°S, N=111) and climatology (60-66°S, July for every year, except 2011 between 6.5 and 9.5 km, N=865) (…)

Figure 8. Southern high-latitude (60-90°S) monthly median water vapour profiles in July for different years, MAESTRO: 2004-2012, ACE-FTS: 2010 (N=169) and 2011 (N=176). A logarithmic scale is used for the x-axis. The number of July profiles (60-90°S) for MAESTRO is 96 per year on average.

Figure 9. (…) The uncertainty accounts for the interannual standard deviation for May (2005-2012) and the relative standard error of individual profiles from the month of May 2010, combined in quadrature (N = 132, 178 for MAESTRO and ACE-FTS, respectively).

Figure 10. Median and average aerosol extinction observed by MAESTRO at 560 nm in May 2010 at northern high latitudes (N=167).

20) P. 25882: here I am completely lost; why do you discuss here data quality issues?

This discussion has been deleted.

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