FINAL RESPONSE, H. VOGELMANN, KARLSRUHE INSTITUTE OF TECHNOLOGY,
IMK-IFU, GARMISCH-PARTENKIRCHEN, GERMANY, MARCH 6, 2015

We thank both anonymous referees for their very sound and constructive
comments which helped to significantly improve our manuscript.
We thereafter present our point to point reply.

Referee #1, General comments:

> 1. The conclusions related to the statistics presented in Figures
> 3, 4, and 5 are de- pending also on the number of sampling cases.
> For example, in figure 3, both for the curves investigating the
> variability of the integrated water vapor (IWV) as a function of
> the horizontal distance x between the center of gravity of FTIRIWV
> and DIAL IWV in summer and winter, the change in the slope of the
> curves showing a change in the IWV variability is also
> corresponding to a decrease in the number of cases available for
> the analysis. Is in this cases the sampling sufficient to justify
> your conclusion? The minimal sampling issue in your analysis
> should discussed and justifies to ultimately support you
> conclusions. This is important not only to justify the reliability
> of your analysis but also to assess the real magnitude of the IWV
> variability with the change of the time and vertical resolution,
> the seasons, and any other relevant parameters to correlate with
> the IWV.

Thank you for this helpful comment. Of course, the standard
deviation of the differences between IWV samples from FTIR and DIAL
can be considered as measurement with an uncertainty. This
uncertainty partly depends on the number of samples respected for
the calculation. Because of the small sample-sizes we decided to
use the bias-corrected estimate S(s) = s / sqrt (2 (n-1)). We added
the uncertainty as error bars in Figs. 3-5 and added information to
the figure captions and also 2 sentences in the text.

> 2. From the text of the manuscript, it seems that the aim is also
> to provide a more general methodology to assess the uncertainty
> due to the non-physical collocation of atmospheric measurements:
> this should be better explained since the generalization of this
> approach to other sites and instruments, as suggested in the
> conclusions, looks extremely depending of the experimental setup
> of the lidar and FTIR and the Zugspitze site. Possible extension
> and limits of the methodology should be clearly identified and
> discussed.

Our analysis is mostly applicable to the free troposphere. However,
it is a consequence of measuring above a complex alpine terrain that
we observe the influence of local convection in our measurement
range during the warm season (up to 4 - 4.5 km). Our results show,
that the amplitudes of the water vapor variability induced by
long-range transport can exceed those induced by local convection by
roughly one order of magnitude. From this, one could conclude, that
the variability inside the boundary layer above less complex
terrain, is assumably reduced to values that we observe under stable
conditions with local convection reaching our measurement range and
dominating the observed variability. This assumption, of course,
implies that the fast advection of heterogeneous air layers does not
impact the boundary layer. We added this thought to the conclusions.

> 3. The analysis reported in sections 4 and 5 related to the water
> vapor variability alone the vertical profiles might be strongly
> enhanced by the use of data from mesoscale models in support of
> the air mass backtrajectory analysis alone. Moreover, more details
> about the backtrajectory analysis should be included like if an
> isentropic or a vertical velocity model has been considered to run
> Hysplit. Indeed, backtrajetories could be reported below each of
> the figure 8-11.

For the two cases of local convection we find that backward trajectories do not yield much helpful input, because the observed variability was initiated locally. The calculated trajectories in principle describe the atmospheric conditions. Downwelling in the stable case and upwelling in the unstable case. Also mesoscale models do not resolve the details of these very local processes better than our direct visual (and lidar) observations of local slope updrifts or the nearby formation of a thunderstorm. Thus, we added only a descriptive sentence.

For the stratospheric intrusion case we decided to show one trajectory plot from ETH Zuerich calculated with the Lagrangian model "LAGRANTO". One reference added. This trajectory model provides potential vorticity and is well suited for studies of stratosphere to troposphere transport. The spaghetti plot shows the particular complex dynamic of this event.

For the second long-range transport case, we added two HYSPLIT trajectory plots which show a switching between Pacific surface-level and Pacific upper troposphere as very remote source regions.

Referee #1, Specific comments:

>1. page 8, lines 9-10: the difference in the typical time
>integration used for the lidar and the FTIR should be justified and
>the authors should explain if and how this may impact the analysis.

The integration times are inherent for both DIAL and FTIR and are needed to achieve reasonable SNR values. But, of course, they tend to blur out very short-term variations on the minute scale. The difference of 13 to 17 minutes mean that the DIAL underestimates very short term variability of IWV a little more than the FTIR. For our variability analysis the integrations times mean, analogously to error propagation that single variations within a time of less than 4 minutes are statistically underestimated by a factor of about 2. Two sentences added.

> 2. page 8, lines 23-25, the statement at the point number 2 about
> the heat driven convective dynamic should be supported by a
> reference or a previous study.

Convection being stronger during the summer season is generally accepted. 2 references to former studies are added.

> 3. page 8, lines 26, why did the authors use measurements pairs
> within different time intervals in winter and summer? Is this
> related to the IWV variability? Please clarify.

See answer to comment 3 by Referee #2.

> 4. page 10, lines 2-6: indeed, the minimum average distance is
> something like 50 days far from the maximum variability of the
> water vapor. This means that this conclusion is a bit forced and
> should be reconsidered.

Thanks for this comment. Statement is modified.

> 5. page 11, lines 5-10: also in this case the conclusion is a bit
> forced and should be reconsidered. The blue and red curves start
> being divergent above 30 minutes, though in a less pronounced way
> than below 30 minutes.

By mistake, the submitted Fig. 5 was a very old and preliminary version based on a deficient data-filtering. It is replaced and now supports our conclusions much better.

> 6. Since the manuscript aims at assessing collocation uncertainty

> too, the errors bars dealing with the random and bias component of
> the uncertainty should be reported everywhere in the plots to
> support the discussion.

Done so. See comment 4, Referee #2.

> 7. Conclusion should be reconsidered according to the general
> comment #2.

Done so, see comment #2.

Referee #1, Technical corrections:

> 1. y-axis label in figures 8-11 should report altitude above
> ground or sea level.

Done so.

> 2. page 4, line 1: "at our site" please change it in "at Zugspitze
> site".

Done so, and also at some other places.

> 3. page 5, line 19: please replace examining for example with
> investigating.

Done so.

> 4. page 7, lines 15-19: please rephrase, I got the meaning but the
> sentence is some- how cryptic.

Sentence added.

> 5. Figures 3-5, not sure the number on the plots are the best way
> to consider the data sampling maybe the authors could couple
> number and colors, though this is only an advise not mandatory.

After many attempts to make these figures as readable as possible we came to the result, that writing numbers beside the curves is best. For assigning the numbers to their related curve we plotted them in the same color for Figs. 3-5.

Referee #2 General comments:

> Specific comments: 1. Provide defining equations for the
> statistical quantities used, and describe in more (mathematical)
> detail how they are determined. It is quite an effort to come up
> with a consistent set of equations, but it would take the
> guesswork out of the paper.

We agree with this point and explain the mathematical retrievals for statistical quantities in more detail. In fact, the retrieval is a bit more sophisticated, than just calculating the standard deviation of differences between the IWV values from both instruments: simga_IWV is the standard-deviation of IWV residuals from a linear model which is given by a regression line calculated for each sample. However, for large sample sizes (n > 20) this yields almost equal results as just calculating SDD between the IWV-values from DIAL and FTIR themselves. Therefore, we added information about this to Sect. 3.

> 2. The authors use the center of gravity height of the vertical
> water-vapor distribution to define the reference plane in which
> the distance between FTIR measurement and DIAL measurement is
> computed. Further, they state that the FTIR measures (IWV of) the
> vertical water-vapor distribution, at least that¿s how the
> reviewer understands the caption of Fig. 1. That¿s confusing. The
> FTIR certainly does not measure vertical IWV but IWV along a

> slanted path. So is an air mass factor taken into account in the
> FTIR retrieval to provide IWV? Please clarify.

Of course, vertical profiles and IWV values from the FTIR instrument are calculated with an angle correction to be consistent with vertical pointing. To clarify this, a sentence is added to Sect. 2.1 and modified the caption of Fig. 1.

> 3. Running text (pp. 8,9) and caption of Fig. 3 are contradictory.
> Is the coincidence time interval 60 min for both winter and summer
> data sets (see caption), or only for the winter data set (and 30
> min for summer, see text)? Please, check! If data shown in Fig. 3
> are indeed for 60 min in both cases, why not show 60-min points in
> Fig. 2?

Thank you for this comment. In a first attempt we used a coincidence interval of 30 minutes for both summer and winter. Because of low sample sizes for larger distances, we switched to 60 minutes, but only for the winter season. We corrected this in the caption of Fig. 3. If using 60 minutes also for summer, the clear onset of the spatial fraction of the variability slightly blurs out.

> 4. Measurements have errors. Some more comments on the robustness
> of the retrieval would be appreciated. At the very least include
> error bars in Figs. 8-11.

We added error bars to Figures 8-11. And added some information about the robustness of the retrieval and error considerations to the text.

> 5. Are the backtrajectory computations reliable enough to trace
> back the origin of the air masses to the North-West Pacific Ocean?
> How many days backwards? The results are plausible, but. . .

In principle, back trajectories are not considered as proof, but for plausibility. Backtracking more than 10 days, of course, is not always reliable. For the North-West-Pacific case, we took this just as basic information of possible origins and than looked at weather charts and satellite images of this region. We added trajectory plots with some comments. See also comment 3 by Referee #1.

Referee #2, Technical corrections:

>Some typos: 1. P. 5, l. 9: ¿information¿

Done.

> 2. P. 15, l. 12: explain "Alpine pumping"

Done.

> 3. P. 16, l. 16: "these conditions"

Done.

> 4. P. 19, l. 20: "relative short-term" or "relatively short-term"?

Changed to relative 'short-term'.

> 5. P. 36, l. 2: unit "s" not in italic

Done.