

Interactive comment on “Spatio-temporal variability of water vapor investigated by lidar and FTIR vertical soundings above Mt. Zugspitze” by H. Vogelmann et al.

Anonymous Referee #1

Received and published: 10 December 2014

The manuscript “Spatio-temporal variability of water vapor investigated by lidar and FTIR vertical soundings above Mt. Zugspitze” by H. Vogelmann et al. aims to provide a methodology to assess the impact on non-physical collocation of lidar and FTIR measurements performed on the mountain site of Zugspitze. The methodology takes advantage of the special location of the two instruments and of the scanning capabilities of the FTIR. A study of the time and spatial high-resolution variability of water vapor at the site is also provided also taking advantage of the air mass backtrajectory analysis. The paper is well written and it fits in with the scope of the journal. Nevertheless, I ask to address some major issues to allow the statistical analysis to become more

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robust also appealing for a wider community.

General comments

1. The conclusions related to the statistics presented in Figures 3, 4, and 5 are depending 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 FTIR IWV 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 be discussed and justified to ultimately support your 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.
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.
3. The analysis reported in sections 4 and 5 related to the water vapor variability along 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, backtrajectories could be reported below each of the figure 8-11.

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Specific comments:

1. page 8, lines 9-10: the difference in the typical time integration used for the lidar and the TIR should be justified and the authors should explain if and how this may impact the analysis.
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.
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.
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.
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.
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.
7. Conclusion should be reconsidered according to the general comment #2.

Technical corrections:

1. y-axis label in figures 8-11 should report altitude above ground or sea level.
2. page 4, line 1: "at our site" please change it in "at Zugspitze site".
3. page 5, line 19: please replace examining for example with investigating.
4. page 7, lines 15-19: please rephrase, I got the meaning but the sentence is somehow cryptic.

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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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 28231, 2014.

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