

Interactive comment on “Total columns of H₂O measured from the ground and from space at Observatoire de Haute-Provence in France (44 N)” by S. Alkasm et al.

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The establishment of truly independent water vapour climatologies from both ground based data as well as satellite observations is an important prerequisite in order to gain understanding of the global and local evolution of the 4D water vapour field. This data then in turn can be used for the evaluation of climate models and the direct studying of climate feedback related to the hydrological cycle. The work presented here provides some insights in the usage of two existing ground-based sensor measurement series located at the Observatoire de Haute-Provence (OHP), which so far have not been exploited for long-term measurements. Both spectrometers provide some similarities

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in principle detector design, covering the same or similar spectral regions in the visible, which are also exploited by the GOME, SCIAMACHY and GOME-2 instruments on ERS-2, Envisat and Metop for the retrieval of total water vapour columns (WVC). While the authors are principally aware of the main difficulties involved when comparing satellite based (visible) retrievals of WVC to ground based retrievals, those being predominantly related to measurement geometry and instrument characteristics as well as the strong horizontal and vertical gradients involved when dealing with the 4D WV field, in its present form I find the paper not convincing in dealing with these difficulties in an adequate manner. Apart from more specific comments (as listed below) my main concerns are the following:

Main comments

I) I am missing a clear motivation for exploiting the specific ground-based instrumentation as addressed here. While radiosondes may exhibit clear disadvantages in accuracy under some conditions, there is quite a range of instrumentation available, like microwave radiometer, as well as lidars, which provide information of the water vapour profile at very accurate levels, and, depending on the operations and location, partially already at significant time scales. While one can guess about the specific interest in using the two spectrometers SAOZ and Elodie (as I have done in my introduction), and while SAOZ and Elodie have the distinct disadvantage to measure only during the twilight or night conditions (while the exploited satellite instrumentation measures only during the morning), nothing is stated along these lines in the introduction or the summary of the paper. Is the purpose of the work to demonstrate that these instruments can be operated more accurately than other instruments and under which conditions?

II) My main concern is that the results of the paper are quite likely strongly related and dependent to the selection of the co-location areas (Fig. 1) selected for the various instruments. If a different selection would have been chosen (like the obvious one to use the same quadrangle for all the instruments, optimizing for all statistics), the results would have been significantly different especially with respect to the impact of the sea-

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land persistent WVC differences (which impact is obviously based on this selection). So considering the importance of this choice the section on the selection of the quadrangles is very weak, with respect to the criteria used. Why have larger areas been investigated and reported for SCIAMACHY than for GOME-2 (see table 1) and what is the interpretation of the presented results. Are larger correlation coefficients reported in table 1 for some instruments than others are not just a result of better statistics for larger areas? In contrast, the same quadrangle settings should be applied to all instruments and the differences in r-square should be discussed in light of instrument viewing geometry, instrument foot-print etc. Additionally, it should be reported how the collocations are distributed within each selected area. Again these are important questions to be answered since one of the main conclusions and findings of the work (the land/sea =WVC gradient) and its impact will strongly depend on the choices made in this section.

III) The other main finding as reported and discussed by the paper in section 5.2. is that the diurnal cycle of water vapour does not play a significant role in the evaluation of the comparison between the ground based measurements (partially taken at night or at twilight) and the satellite measurements (all local time morning overpasses). Apart from the fact that the statement that the diurnal cycle "can be caused by the influence of large-scale synoptic flows" is probably just a problem in wording, diurnal cycles in WVC at mid-latitudes - where the mentioned synoptic scales have a large and dominating influence - can consequently only be found when averaging over much longer time-scales than is done here using ECMWF forecast data. For the satellite/ground-based instruments monthly averages are used. Therefore the diurnal cycle and the quantification of its contribution must be evaluated at least based on monthly averages of analysis (not forecast) data and for each month of the year before it can be ruled out as being in-significant exactly because of the expected dominating effect of large-scale weather systems. 6 days, as provided here, are not sufficient. The diurnal cycle effect should therefore be best evaluated in the same fashion as the land sea effect, i.e. in all of the 35 sub-zones and based on the full time series of data. For this evaluation

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analysis and not forecast data should be used, since the model will have a tendency to over-estimate the diurnal effect with respect to the large-scale circulation.

Specific comments

Abstract: The first sentence seems to be incomplete

Page 4251, l. 9: "Note that these instruments were not initially dedicated" -> to which instruments does the author refer here?

Page 4251, l. 20: "The objective of this comparison was to build up continuous decadal series of H₂O data." -> From which instrument? GOME? Please (re-)formulate the objective of the paper clearer. Is the objective to demonstrate that long-term data-series can be used under certain conditions from the SAOZ and Elodie instrument and for what purposes. Are they expected to be more accurate like the available data from the operational radio-sonde network, or even from MWR or lidars? Or is the latter what will be evaluated by this work? (see general comment I)

Page 4252, l. 16ff: "One of the objectives of this publication is therefore to bring quality..." -> This objective should be stated in the introduction and in the light of other available and very accurate ground based techniques like the mentioned lidar and micro-wave instruments. What are the advantages of using SAOZ and Elodie above other instrument techniques. How well do both techniques compare to radio-sondes or other ground based instrumentation.

Page 4254, l. 6ff: "ESA has been delivering users three-day.." -> I think this information is not relevant here. The delivery of level-2 and 1 data by ESA should be in the acknowledgment, as well as EUMETSAT should be acknowledged to deliver GOME-2 level-1 data, both in near-real time as offline.

Page 4254, l.17: What does "am" mean in this context?

Page 4255, Section 4: I suggest calling section 4 "results" only and Section 5 "Discussions".

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Page 4255, l. 9: Remove “together”.

Page 4255, l.21: “trapped” -> ?

Page 4255, l.24ff: “We can see also note that for...” -> please rephrase sentence.

Page 4256, l.19ff: “An air-mass correction factor in the algorithm...” -> Sentence is not clear. It reads as if AMC-DOAS first deals with clouds and then all cloudy-cases are removed.

Page 4257, l. 1ff: Why are GOME-2 data not used? In this list the comparisons between SAOZ/Elodie and GOME-2 are not mentioned, while in the next paragraph they are mentioned again. Overall it is not clear why GOME-2 data is mentioned in first place since the data series seem to stop at 2004 with the ground based comparisons, and only inter-satellite comparisons are shown for later periods with GOME-2. Please clarify.

Page 4257, l. 13: “According to the figures previously shown, which compare the monthly averages of H2O VCD provided by our instruments,…” -> which instruments?

Page 4257, l. 20: “The quantification in Table 3 shows that the differences between…” -> What kind of differences are we talking about here? About the absolute differences between individual co-locations or about the before mentioned “amplitude differences”.

Page 4259, l.12ff: “Finally we can say, in the case of instruments…” -> This conclusion is drawn from what?

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