

Answer to comments from reviewer 2:

Kalakoski et. al.: Validation of Copernicus Sentinel-3/OLCI Level 2 LAND Integrated Water Vapour product

The paper presents the analysis of validation of the Integrated Water Vapour (IWV) from Sentinel-3 Ocean and Land Colour Instrument (OLCI). The statistics is based on the comparison with data from the SUOMINET network and from the IGRA database. In my opinion, the scientific value of this research is rather high since IWV is the atmospheric parameter required for solving a large number of problems of climate change and weather prediction. Besides, the scientific interest to the described results is determined by the fact that the presented analysis is comparative (the data are provided by three independent sources) and covers different geographical locations. The obtained results could be helpful for the improvement of the retrieval algorithm used to process the OLCI measurements.

Despite the fact that the paper contains valuable and interesting results, I can not recommend it for publication in the present form. **The paper requires major revision.** There are several issues of general character which should be addressed in this paper. Besides, there are also specific remarks.

Authors thank the reviewer for careful reading and constructive comments. The suggestions given, especially for the results section, help to make the paper more coherent and focused.

Please find below our answers (in blue) to the comments presented (in black).

General critical issues:

1) The IWV measurements by OLCI are not placed within the context of modern space-based observations of IWV. Such placing is especially important since providing information on atmospheric parameters is a secondary goal of the OLCI mission and not a primary goal as the authors indicate in the Introduction section. My recommendation is to give a comparison of the IWV retrieval accuracy values and ground pixel size values which are declared or estimated for OLCI and other satellite instruments which measure IWV. Such comparison would help to assess the value of the IWV observations by OLCI.

Section on other satellite sources with comparison to OLCI was added to the introduction (Lines 22-42).

2) Section 2 “OLCI Integrated Water Vapour retrieval” is too sketchy. I recommend giving more details of the retrieval algorithm instead of giving only references to technical guidelines. In addition, full resolution and reduced resolution modes should be explained which are now only briefly mentioned (Page 2, line 46).

Section describing the algorithm and retrieval was extended.

3) The description of the data selection procedure in Section 4 is very unclear. In particular, the sentence “Missing satellite extractions...” (Page 4, Line 88) can be misleading. Please explain what you mean when you say that the reference observations should be validated (Page 4, Lines 90-91). I recommend the authors to remove current subsections of Section 4 and to provide the information on data selection and quality control on step-by-step basis

separately for IGRA and SUOMINET. All criteria for match-up should be given clearly. The reasons for choosing 31x31 macropixels should be presented also.

Agreed, the data selection section is unclear. The section 4 was written in subsections to separate selection done at the database creation (4.1) and selection done during the analysis (4.2). As it is, this distinction is not clear.

Line 88: Agreed, clarified in the revised version.

Lines 90-91: Validated here is only taken to mean “fulfill the quality criteria”.

Section 4 was reorganized and clarified in the revised manuscript as suggested.

4) In my opinion, the presentation of the results in Section 5 requires improvement:

- The cloud-screening criteria (Page 4, Line 102) have been already described in Section 4. Mentioning these criteria in Section 5.1 can produce a wrong impression that they were used not in all cases.

Agreed, cloud-screening criteria are repeated unnecessarily. Repeated discussion is removed from the revised version of section 5.

In order to avoid confusion, I recommend not to mention the results with WATER pixels. The authors note that they make validation for land pixels and “WATER pixels are not strictly part of the ESA OLCI product”.

Agreed, discussion of WATER pixels was removed from the revised version of the manuscript.

I can not understand whether figures A1 and A2 belong to any Attachment or not. I would like to see these figures and relevant expanded discussion within the main text. I am not sure if bottom rows in these figures are necessary since all latitudes are taken together and therefore seasonal dependence seems to be hard to detect.

We agree with the reviewer that the status of figure A1 and A2 is unclear. Figures are moved to Section 5 in the revised version (figures 4 and 5) with revised discussion (lines 175-179). Likewise, we agree that the seasonal dependency (or lack of it) is difficult to see in the bottom panels. Accordingly, the bottom panels are removed from the revised versions of the figures.

Besides, I have the feeling that Fig. 3 duplicates information which is already contained in Fig. 2.

Information on Figure 3 can indeed also be seen in Figure 2. However, we feel that the presentation in Figure 3 is clearer to some readers.

- The title of Subsection 5.2 “Classification of biases” is misleading because mainly the problem with camera 3 is discussed. I am not sure if Fig. 5 is necessary. It is sufficient to mention in the main text about negligible influence of neighbouring cloud-flagged pixels.

Agreed, we changed the title of the subsection and removed the figure 5. Mention of the cloud proximity was added to section 5.1 (lines 172-174) with the figure 5 provided as supplementary material.

- In Subsection 5.3 please explain how the distribution of the error estimates from the retrieval algorithm was obtained. I suppose that every single OLCI measurement has its own error estimation and you just collected this information from all OLCI measurements. Is it so?

Yes, each OLCI measurement has an associated uncertainty estimate. The bottom-left panels of Figure 6 show the mean value of the error estimate for pixels within the macro-pixel.

- I would like to recommend removing Subsection 5.4 and Fig. 7 in order to avoid confusion. As I have already mentioned above, the reason is that water pixels are not included in the OLCI products. If the authors decide to keep this subsection, all terms should be explained (WATER, TIDAL, INLAND), the number of observations in this critical pixels should be given, the observational conditions should be described (water, ice, ice covered with snow) etc.

Agreed, the emphasis of the paper is on the LAND observations. The discussion of WATER pixels and Figure 7 was removed from the revised manuscript.

Specific remarks:

Page 1, Line 2

What is the meaning of the term “geophysical” in the context of validation?

The word “geophysical” is removed to avoid confusion.

Page 1, Line 15

Remove the abbreviation ECV which is not used in the text below.

Removed accordingly

Section 3.1

Is it possible to roughly estimate the typical horizontal drift of a radiosonde during ascent to the pressure level of 500 hPa in order to assess the “effective” horizontal resolution of radiosonde measurements of IWV?

Median horizontal drift of the soundings is about 10 km, depending on the latitude (Seidel et al., 2011). We added this information in the revised version (lines 104-108).

Section 3.2

If possible, please provide the information about pressure level which is assumed as an upper limit for IWV in the SUOMINET GNSS network? What is the area of horizontal averaging for the derivation of IWV by the GNSS method?

GNSS IWV is assumed here to represent full atmospheric column. If any upper limit exists, we assume it to be high enough to have little or no systematic effect on the GNSS derived IWV.

The horizontal resolution of the GNSS method depends on the azimuth angle of the GNSS satellites visible at the time of the observation. In general, Van Malderen et. al., (2014) consider the GNSS observation to represent a cone with area of roughly 100 km². Value and the reference were added to Section 3.2 (lines 116-117).

Page 4, Line 82

Please explain the acronym ACRI-ST.

To our knowledge, ACRI-ST is not an acronym.

Page 5, Line 112

Please compare typical time of the sonde ascents to the level of 500 hPa with the allowed time mismatch between OLCI and sonde observations.

Median descent time to 500 hPa is 30 minutes (Seidel et al. 2011). Discussion is added to the revised version of Section 3.2 (lines 107-108).

REFERENCES

Seidel, D. J., Sun, B., Pettey, M., & Reale, A. (2011). Global radiosonde balloon drift statistics. *Journal of Geophysical Research: Atmospheres*, 116(D7).

Van Malderen, R., Brenot, H., Pottiaux, E., Beirle, S., Hermans, C., De Mazière, M., ... & Bruyninx, C. (2014). A multi-site intercomparison of integrated water vapour observations for climate change analysis. *Atmospheric Measurement Techniques*, 7(8), 2487-2512.