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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Complete Data Fusion for a Full Exploitation of Copernicus Atmospheric Sentinel Level 2 Products" *by* Nicola Zoppetti et al.

Anonymous Referee #2

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The paper by Zoppetti et al. presents the application of a so-called "Complete Data Fusion" approach to derive a single ozone vertical profile out of hundreds of satellite-derived ozone vertical profiles at pixel resolution to synthetic measurements of the Sentinel 4 and Sentinel 5 future missions. This is presented as a better alternative to a simple arithmetic average of the pixel-resolution derived ozone profiles, since vertical resolution or sensitivity of the retrieval is increased as compared to individual retrievals and also a reduction of the retrieval error is obtained. This concept is promising and the application of such an approach for future ozone retrievals is very interesting. However, the current manuscript dramatically lacks explanations of the results presented in the paper and also comparison to other approaches. Moreover, some important incon-

sistencies in the dataset used for the analysis is found, such the performance of one of the measurements (the infrared sensor of the Meteosat Third General or as mentioned in the paper S4 TIR sensor). The title of the paper is not appropriate either.

Major remarks that I strongly recommend to address:

1) Title of the paper: the current title is far too general, and it should be focused on the actual work presented on the paper. I strongly recommend clearly indicating the only species analyzed "atmospheric ozone profile", the only measurements used "Meteosat Third General and EUMETSAT Polar System Second Generation" and that is a sensitivity study based on simulations. I disagree indicating "Full Exploitation" since it is not the only way to exploit these measurements and "Copernicus Atmospheric Sentinel Level 2 Products" since only one species and 2 missions are concerned. By the way, Sentinel 4 and Sentinel 5 are the denomination of the UVNS sensors and not the ones operating in the infrared. The correct denotation is IRS onboard MTG-S and IASI-NG onboard EPS-SG. The whole manuscript should be corrected in that sense.

2) Introduction and scientific context of this work: this is one the major missing aspects in the paper. The instruction and the other section very rarely cite nor mention other related works on the subject different from previous works of the authors themselves. Although they do exist relatively abundantly, the authors do not mention any other approaches (except for the plain average) to use synergistically different measurements of the same or different sensors to derive ozone profiles. The authors should thoroughly provide a full exhaustive list of approaches of synergism of several measurements to derive ozone profiles and compare them (at least conceptually) to the proposed fusion approach.

3) Explanation of the results of the approach: only the equations of the fusion approach are transcribed in the paper, without any physical explanations, and the obtained results are very superficially described, with very little explanation for understanding them. Although this is the heart of the paper, the reader cannot understand

why the fusion approach allows an enhancement of vertical resolution or sensitivity to ozone from the synergism of several hundreds of profiles. This aspect should be thoroughly explained in physical terms and illustrations should be given. Concerning the retrieval errors, many explanations are missing, and this should be at least compared to the error of the average within the main text, in each occasion. Another missing aspect is how the authors represent systematic errors and co-locations errors. This is important to know and be explained in this current paper, since the persistance of such errors partly differentiates the current approach from the arithmetic average.

4) Datasets used in the paper: the explanation of the missions and simulated datasets is extremely brief. The reader does not understand what the differences between sensors are and why they provide certain DOFs or spatial coverage. The performance of the "S4:TIR" sensor does not seem to correspond to its instrumental characteristics. This sensor, which in reality is called IRS onboard MTG-S and not S4:TIR, has similar instrumental characteristics as IASI, with even a coarser spectral resolution and similar radiometric noise in the ozone band around 10 microns. The total column DOF for an ozone retrieval from IASI is typically 3 and at most 3.5. The current paper shows DOFs for "S4:TIR" greater than 5, which is not possible in my understanding. Unless thoroughly explained, justified and compared to IASI, all results considering "S4:TIR" simulated L2 products should be done again with proper instrumental characteristics. Moreover, the description of the atmospheric scenario should provide within this paper in much more detail (e.g. the resolution and particularities of pseudo-reality, sources, variability, etc).

More particular aspects:

5) Abstract: only tens or hundreds of measurements fall within tens of kilometers if satellite observations are finely resolved. This was not the case before Sentinel 5P for ozone retrievals and it is not the case for IASI-NG either.

6) L23: One can also average or do the median of the datasets.

7) L45-51: This is not true. By reducing the horizontal resolution, we lose natural variability within the grid cell.

8) Sections should be number in order to cite them.

9) L105-108: this sentence is not very clear. Please reformulate it clearly defining S and Stotal

10) L114: "The above formulation was used to simulate ozone profiles in the two spectral bands (UV and TIR) for both S4 and S5" please reformulate. Ozone profile are retrieved using measurements from a spectral band.

11) L120: However, these are not true retrieval from an iterative numerical procedure. How this formulation compares to true retrievals as those from true measurements? This aspect should be clarified and illustrated. Are S and Stotal consistent with those from true full retrievals?

12) L132: why 5%? This should be justified.

13) L139: the notion of "good" or bad is subjective. This cannot be expressed in such a why, but in objective terms (reduction of errors, bias, sensitivity, etc).. Please, reformulate.

14) L173: when embedded in the text, please use the word Figure and not Fig.

15) Error of fuse profile: it is not as low as 1 over the square root of the number of measurements as it would be for random errors and the arithmetic average but only around -30%. Comment thoroughly and explain.

16) Fig.3: It should be clearly written in the caption of the figure and the text that the AK of the "fuse profile" comes from the fusion of 118 profiles and the other AK are for single measurements.

17) Fig. 3: A full description of the 4 instruments and their characteristics should be given.

18) Fig. 3: S4 TIR: DOF of 5 is too high for the IRS sensor.

19) Fig. 3: Why only 4 curves are displayed instead of 5 AKs (4 sensors + fusion result)?

20) Fig. 3: Results not explained: DOF of 9.5 how do you explain this in physical terms? Where does it come from? Having only a few S5-derived profiles and more than a hundred of S4-derived, what Is the influence of having an asymmetric number of profiles from one or the other instruments?

21) Large domain section: the title indicates 0.5° x 0.625° which is not large.

22) L193-L195: The sentence is not clear. Please reformulate. What is fusion grid-boxes?

23) L200: The explanation of variables should also provided in the caption of table 2

24) Please use Table and not Tab. Same Figure and not Fig. in the caption.

25) L195-198: What happened to S5-TIR? Why it is not here?

26) Table 2: this nomenclature is not clear S4:TIR+UV1_S5:TIR+UV1. Please reformulate here and elsewhere. What is the meaning of ":" and "_" in a name. They should be avoided in the names and the true names for the TIR sensors should be used.

27) Figure 4: the fraction of clear sky measurements seems very reduced. Although they exist in reality, no measurements with a small cloud fraction are considered? This should be clearly stated.

28) Why S5UV1 are only available over Northern Africa? Why we do not have S5 pixels near Greenland? This should be explained thoroughly in this paper since is a major dataset of the paper and not referred to previous papers.

29) L217 and L230: Why do you justify again avoiding the use of averaging without any quantified and clear statement. This method of arithmetic average should be explicitly

included in the comparison every time and compared in terms of error and performance.

30) L220: What is the meaning of pure number? Without units?

31) Fig.5 shows that SF DOF is at most 1.9 and this seems to be the case for Fig. 3. This means that the example of Fig.3 is not a typical case with SF DOF around 1.5 but the maximum performance of the fusion. The choice of this example should be reviewed, and typical case should be taken, but not the best.

32) I do not understand why the best performance is found for the use of the two S4 products, since the performance of the TIR sensor of S4 is not the best, as compared to that on S5.

33) Fig.5 caption: it should be explained that is the product of combining a large number of measurements. I recommend not to use N but Number in the axis label

34) Figure 6: What is the link between SF for AK and for Error? It seems that large SF AK correspond to smaller SF Error and vice versa (looking at green and purple dots). This should be explained and clearly quantified.

35) Table 3: it seems strange that no results of the 1x1° cells are provided in the text. This should be commented and at least provided in terms of a table and compared to the smaller cells.

36) L293-295: I do not think that this is not true. The fusing products come from L2 products; they are intrinsically dependent in a priori information of these products.

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