Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-447-RC2, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Measurement Techniques Discussions

Interactive comment on "Quality assessment of the Ozone_cci Climate Research Data Package (release 2017): 2. Ground-based validation of nadir ozone profile data products" *by* Arno Keppens et al.

Anonymous Referee #2

Received and published: 16 April 2018

General Comments:

This paper presents a comprehensive and lengthy assessment of the ozone_cci CRDP of 13 nadir ozone profile data products from both UV-VIS and TIR instruments as well as 1 data assimilation product. The evaluation includes data content studies, information content, and validation against ground-based ozonesonde and lidar observations in terms of median relative biases and the IP68 spread as a function of various influence quantities and relative decadal drift. It is a very useful study and its scope is very suitable for publication in AMT. This paper is generally well organized, the methodology

Printer-friendly version

Discussion paper

is generally very good and valid, and the results are well described. However, some of the sections are difficult to understand. For example, the section of L3 gridding could be made clearer and simpler and Figure 1 could be removed. The results of the vertical sensitivity are very difficult to interpret, and the derivation of vertical sensitivity could be improved. Also, the abstract does not include main conclusions. In addition, some texts need clarifications. Overall, I think that this paper can be published after addressing the comments mentioned here and specific comments below.

Specific Comments:

1. In abstract, no conclusions are given. So what are the main conclusions of this study? Some of the sentences in the conclusions/discussion sections can be paraphrased here.

2. In the introduction, full instrument names should be specified at their first occurrences.

3. In Sections 2.2 and 2.3, it is useful to mention the unit of the retrieved ozone profile for each algorithm: partial ozone column in DU, average ozone mixing ratio in ppbv, etc.

4. Figure 1 and the text on P7 are difficult to follow and confusing. I guess that grid cells refer to those 1 x 1 boxes, but Figure 1 caption says TM5 assimilation grid. Is TM5 grid 1x1 (looks like it is 2 x 3 based on sect. 2.5)? Also grid cells boundaries typically are not parallel or perpendicular to ground pixel edges as shown in the figure. The naming of grid cells based on pixel corners also makes it more confusing as depending on the pixel size, the entire pixel can lie in one grid cell. Also, what is the size of subpixels and how many subpixels for different instruments. I think that this can be described more clearly and also more concisely. The figure does not really help here and can be removed. Basically, each ground pixel is divided into subpixels (size, #), each subpixel contains the same value and uncertainty, then assign the subpixels to grid cells.

CC BY

5. Please put table captions before the tables.

6. P10, L17-21, temperature profiles are not required for conversion between number density and average layer VMR. Assuming a layer is well mixed, then average VMR = 1.25 * (partial ozone column in DU) / (pressure difference of the layer in atm). Please see appendix B of Ziemke et al. ("Cloud slicing": A new technique to derive upper tropospheric ozone from satellite measurements, JGR, 106, (D9), P 9853–9867, 2001) for more detail. The partial ozone column is related to number density and altitude difference of the layer.

7. P11, it is not clear about the three numbers in SPI column separated by ":"

8. P12, L18, even if is difficult to know how much IASI data are screened as a function of latitude and time, the data providers should know on average how much data are screened out due to the use of cloud fraction greater than 13%.

9. P15, L15-16, It is not clear why "quite stable in time" reflects the signal degradation correction? Please clarify it.

10. P15, L18-19, It is useful to explain the lower DFS under SAA: shorter wavelengths with weak signals cannot be used due to SAA, thus significantly reducing DFS in the stratosphere

11. P14, equation (6), based on the text, A_F is provided from the FORLI algorithm, so should the defractionalisation operation derive A(m, n) from A_F rather than derive A_F from A(m,n)? I suggest changing this equation to A(m,n) = A_F * . . .

12. P14, L22, first paragraph of P17, Figure 5: It is not easy to under the meaning of vertical sensitivity. Based on the definition on P14, it is an indication of the fraction of the information that is from the data. But on Figure 5 and P17, the vertical sensitivity values peak in the UTLS with a median value of 3, and are often greater than 1 even below 6 km, which does not seem to be consistent with the definition of the fraction of information from the measurement. Also the vertical sensitivity should not peak

CC BY

in the UTLS, as there is stronger vertical sensitivity in the stratosphere from UV-VIS measurements. Please check DFS at individual layers to make sure this is the case. It seems to me that this concept is not actually a good indicator of the vertical sensitivity or it might depend on how the vertical sensitivity is derived (e.g., from AKM or fractional AKM, what is the unit of state vector, e.g., DU or mixing ratio etc). Based on the definition, when you sum the sensitivity of retrieved ozone at a layer to the perturbations of ozone at all layers, the units of state vector or the weighting of the perturbations at each layer are important. Using mass conserved units like DU or the weighting of perturbations at each layer by a priori error (rather than the a priori or the retrieved profile) might make more sense. Between IASI and UV-VIS retrievals, it is good to convert the AK to the same units and then apply the same concept. Please clarify this on P4. You may also consider the use of DFS at each layer (diagonal elements of AK) normalized to the depth of layer (to account for non-uniform, variable vertical altitude grid) to show the vertical sensitivity, which is straightforward and independent of the retrieval scheme and might be more meaningful.

13. P17, L12-14, it is not clear why the strong sensitivity variability affects vertical smoothing and Eq. 3 introduces a bias. Please make it clearer.

14. P17, L17-18 and also in Fig. 5 caption, it is not clear which is direct and centroid offset between 2nd and 3rd rows. Please make it clear in the figure caption. Also please mentioned the dotted lines in the figure caption.

15. P17, L31, in “decreases first to about 20 km”, it seems to me from the figure that the maximum median FWHM is $\sim$20 km, so should it be a smaller number here?

16. P18, first sentence, “slant column density” should not be parallel to “the sensitivity” because the larger slant column density, the smaller the sensitivity from surface to the lower stratosphere. The real reason is because, the larger slant path length or slant column density, the fewer photons penetrating into the troposphere, the smaller the sensitivity in the troposphere, and the larger the resolving length values.

CC BY

17. P20, L4, suggest changing “quarter” to “season”

18. P20, L24-25, The sentence “This is with the exception of the Metop-B GOME-2 and IASI instruments however, that have not been used for drift studies” is difficult to understand. Suggest changing to “So Metop-B GOME-2 and IASI instruments are excluded for drift studies” and moved it after “for this drift assessment”

19. P20, L29, in addition to latitude and season, the influence quantity of time should be added.

20. P21, the sentence above Eq 9 is difficult to read. Suggest changing to “Yet both approaches introduce similar spatial and temporal representativeness errors into the difference statistics because taking (monthly) averages as a bias estimator âŇľ∆xâŇł yields comparable outcomes: ”

21. P22, L15, is the ex-ante uncertainty from the retrievals for random noise errors or for both random noise errors and smoothing errors? As the averaging kernels are applied to reference data to remove smoothing errors, ex-ante uncertainty of random noise errors should be shown here. Please clarify this.

22. P22, L26, suggest adding “because the retrievals only include random noise errors and smoothing errors in the ex-ante uncertainty” after the “nadir ozone profile products”

23. P22, L26, OMI is not an exception in that the total satellite measurement uncertainty is underestimated because the ex-ante uncertainty should be compared to comparison spread or the quadratic sum of comparison bias and spread rather than the comparison bias only.

24. P23, L1, it is not generally true that there are smaller biases for larger total ozone columns based on the figures as the biases often increases when the total ozone increases from 300-400 to 400-500 or 500-600 DU (very clearly for GOME and OMI retrievals).

25. P23, L3-4, the relationship between SZA/DFS and the biases are altitude-

CC BY

dependent. From the figures, the biases are typically smaller at larger SZAs/DFS in the troposphere, but are larger at larger SZAs/DFS. Larger SZAs typically lead to larger total DFS due to the increase of DFS in the stratosphere and often lead to smaller DFS in the troposphere due to reduced photon penetration. Smaller biases in the troposphere at larger SZAs/DFS could be due to the reduced retrieval sensitivity in the troposphere (i.e., retrievals are closer to the a priori). So the causal relationship is not as straightforward as larger DFS means better retrieval sensitivity and therefore smaller biases.

26. P33, L11, based on figures, the spread is not always enlarged in the L3 comparison. Instead, the spread is typically significantly reduced below ∼6 km. This should be mentioned and explained.

27. P34, L1, says “GOME L3 data show a negative above-tropopause bias of 5-10%”. But based on the first panel of Fig. 11, I see mostly positive biases above 100 hPa, especially with large positive biases of 20% around 70 hPa and positive biases of 40% around 8 hPa. Please clarify this.

28. P36, L11, again the spread values for the L3 comparison can be smaller below ∼6 km, which should be mentioned.

29. P40, L7-8, it is useful to explain to the readers why there is stronger tropospheric reduction to 20% and why the drift is small (e.g., due to bias correction).

30. P45, last line, the 30% negative should be 30% positive in Antarctica as shown in last panel of Fig. 16. Also please change Table 5 correspondingly.

31. In table 5, suggest changing “Vertical resolution (6 km to troposphere)” to “Vertical grid/ resolution”, changing to “115 km2”, “230 by 345 km2”, “12 km2”, “115 km2”

32. In Figs. 6-10, 14-15, change second bracket from “[“ to “]” in Fig. captions

Technical Comments:

1. P2 last line, and P8 L25, change “Keppens et al., 2015” to “Keppens et al. (2015)”

CC BY

2. P4, L6, change to “time series”

3. P4, L13, P5, L15, P6, L13, change “priori” to “a priori”

4. P9, L14, change “beyond” to “above”

5. P11, change “prior” or “a-priori” to “a priori”

6. P11, suggest changing “averaging kernel smoothing of the FRM” to “smoothing the FRM with averaging kernel”

7. P12, L6, suggest changing “relative amount” to “percentage”

8. P12, L18, suggest changing “delivery. E.g.” to “delivery, i.e., “

9. P13, L10, change “prior” or “a-priori” to “a priori”

10. P14, L5-6, change to “help understand”

11. P14, L16 & L26, change “prior” or “a-priori” to “a priori”

12. P21, L13, suggest changing “Thanks to” to “Due to” to make it formal.

13. P21, L21, change “vertical averaging smoothing of ground-based reference data” to “vertical smoothing of ground-based reference data with averaging kernels”

14. P21, L29-30, change “smoothing difference error” to “retrieval smoothing error” and “Tropics” to “tropics”

15. P23, L6, change to “instruments except for GOME-2B”

16. P33, L12, change to “lack of”

17. P48, L10, change to “7 km by 7 km” or “7 by 7 km2”

18. P49, L11, change to “equivalent to”

19. P49, L12, suggest changing “as an alternative” to “along with”

Printer-friendly version

Discussion paper

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-447, 2018.