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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ground-based remote sensing of O_3 by high and medium resolution FTIR spectrometers over the Mexico City basin" *by* Eddy F. Plaza-Medina et al.

Anonymous Referee #2

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General comments

This paper presents for the first time FTIR ozone measurements over Latin America, at 2 close but different sites (Altzomoni, a mountain observatory; and Mexico city, a polluted site). Note that ozone FTIR data are available in the NDACC database at Paramaribo (Suriname, 5°48'N, 55°12'W), but to my knowledge these results have not been published. Furthermore, the authors combine the measurements at these 2 sites to obtain a new product: an O3 boundary layer. This could provide interesting new information and would be promising for future investigation of the Mexico city pollution. Therefore, I recommend the publication of this paper in AMT. However, I have some few questions to be clarified, and some specific comments that need to be taken into

account before publication. The 2 main points being the fact that the authors forgot about the smoothing error in their uncertainty budget, and that their "new product" is not convincing to me at present. So it should be at the very least clarified, or omitted in the new manuscript.

Specific comments/questions

1) Section 2.2 (Retrieval method) and 2.3 (Error analysis)

a) Are the baseline (channeling and offset), the ILS, the temperature, the line of sight, and the solar lines fixed model parameters or do you retrieve them ? In the case of UNAM, the Linefit results (Hase, 2012) are given as input in the forward model or Linefit is just used to check the alignment (you specify only for Altzomoni) ?

b) You wrote that all interfering species are from WACCM. Does that mean that this is also the case for H2O ? Maybe the 100% uncertainty taken for H2O interference could be reduced by using e.g. either preliminary retrieval of H2O or 6-hourly H2O profiles from NCEP. However, you obtain very small errors due to H2O in Table 2, so this seems not so important at your sites anyway.

c) Spectroscopic errors: p.6, l.4-5, you say that your spectroscopic errors are coming from the HITRAN line list and in the discussion p. 16, l.16-17, you say that the uncertainty on pressure broadening parameters could be as large as 20% (instead of 5%). Could you explain ? Does this mean that the 5% should not be trusted ?

d) Table 1: I don't understand how the numbers in the column "Statistical/Systematic" are obtained, and how they are applied in the error analysis. Can you explain in the paper ?

e) My main concern about this paper is that one of the dominating random error source is missing in your budget (Tables 2 3; Figs. 3 and 5): the smoothing error. You need to replace your Eq. 2 by, e.g., Eq. 1 of Schneider et al. ACP 2008 ("Quality assessment of O3 profiles…"). And calculate the smoothing error, which is clearly dominant on ozone profiles and partial columns.

2) Section 3: Free tropospheric and stratospheric O3

a) Seasonal cycle at UNAM: you could add the seasonal cycle of the total columns at UNAM in Fig.6 (top panel), for comparison. Same for the different altitudes, if you define common altitudes where both instruments have information. (as you did for Fig.7).

b) Fig. 6 (lower panel): I would prefer 4 different subplots instead of all curves in the same plot. You could also add UNAM seasonal cycle (see comment a)).

c) Seasonal cycle discussion: since you have a model available (waccm), I wonder if the model reproduces well your seasonal cycles. It could be interesting to check this, and maybe add the model seasonal cycle on the plots.

d) For me, since FTIR has low vertical resolution, it would be better to describe your data in partial columns (PC) where you have about one DOFS, rather than at a given altitude point. If you take into account the smoothing error, you will find a larger error on a single altitude point (the dominant random error on the profiles is the smoothing error), than on a PC. This division in PC based on DOFS is commonly used in FTIR studies (e.g. García et al., 2012; Vigouroux et al., 2015; Duchatelet et al., 2010;. . .). I would use 4 PC in Fig. 6 (lower panel) instead of altitude points, and also in Fig.7. Your agreement between Altzomoni and UNAM PC would be probably better using common PC rather than with single altitude points (Sect. 3.2).

e) Scmp formula (Eq.8): I think you should give the reference for this formula (Rodgers and Connor, 2003). In Rodgers and Connor (2003), Scmp is not only your Eq. 8, you also have the terms S_{UNAM} + A_{UNAM} S_{ALTZ} A_{UNAM} in addition (Eq. 30 of Rodgers and Connor, 2003). Since you do not use Eq. 8 in this Sect. 3.2, I am not sure that you are indeed talking about the complete error covariance matrix of the comparison between the 2 instruments ("error on the differences", are you ? If yes, then you should

use the complete Eq. 30 of Rodgers and Connor (2003).

f) In principle, you should use S_{cmp} (complete formula) to conclude if the 2 instruments are in agreement: standard deviation of the differences (1.2%; 5.2%, and 2.1% for your 3 comparisons) < "random error on the differences". The bias of 8.7% at 17 km is probably not explained by the systematic uncertainty budget on the differences (same spectroscopy, same temperature profile), but this bias might improve if you use PC instead of a point at 17 km.

g) p.11, l. 15: Scov= 100% O3 variability: this is a large assumed variability, especially in the stratosphere. I think you are overestimating then your error on comparison. Why not taking Scov from the model WACCM ? (or any other climatology, e.g. from satellite?)

h) p.11, l. 16-17: I don't understand how you can "require that the square root of. . . is smaller than 10%". If you fix Scov (100% O3 variability + 5 km correlation), and you have A_{UNAM} and A_{ALTZ} fixed from your retrievals, how can you control S_{cmp} ? Sorry, I am missing what you mean here.

i) p.11, l. 26-27: you said that the bias of 2% is due to the different altitudes between the 2 instruments. But if I get right how you construct x^*_{ALTZ}, you are not using zero values from 2.3 and 4km, but the values of xa. Then you should not have some bias (at least not so large). But maybe you calculate from x^*_{ALTZ} a total column that starts only at 4 km ? If yes, please specify in the text.

3) Section 4: Boundary layer

a) Sect. 4.2: Again here, you could reduce the error on the FTIR products by using PC instead of a single value at 2.3 or 4 km (as in, e.g., Sepúlveda et al., 2014) ? These values are correlated to the whole tropospheric column anyway. I would be curious to see Fig. 9 with PC instead of single altitude values.

b) Sect. 4.3: p.13, l.24-30: Note that if you use Eq.30 of Rodgers and Connor (2003), you directly have the combined error on the differences, i.e., your 4-5% value.

c) I don't understand why having a boundary layer product coming from the total columns differences with an error of about 4-5%, would be worse than the combined product that you propose for which you reach 21% ! Therefore, I am really surprised that the correlation with in situ data using your combined product is better than using the total column differences.

d) I am not convinced by the construction of the combined product. So I strongly suggest that you document better what you are doing. Is there any reference that could be added to your calculation ? At present these iterations between Eq. 9 and 11 (why only 2 ?) seem arbitrary. I have the feeling that the DOFS are increased "arbitrarily" in the boundary layer (p.15, l. 29 – p.26, l.1; and p.26, l.9-10). There is one instrument at 2.3 km with "little sensitivity up to 4 km (from 0.05 to 0.12 Fig.10), and one instrument that is not measuring below 4 km, so to me what you are doing by using the information above 4 km from the 2 instruments, removing it (Acom come to about zero above 4 km; Fig 10), and transferring the information to the 2.3-4km is equivalent to use the differences between the 2 instruments. And if you want to do so, the more precise way should be to use the total columns since the errors of the instruments are the lowest for total columns, because the smoothing error is much reduced for total columns. It might be that I am missing something, but then more information and references are needed in the paper to explain why this combined product is not only "artificial", and that real information is appearing in this boundary layer, with an added value compared to differences of total columns.

Technical / minor comments:

- Abstract, l. 6: "three" and "four" should be inverted.

- p.2, l. 21: " of Paris, Viatte et., 2011)" : add the comma.

- p.2, l. 22: Missing "." And "However" instead of "however".

- p.2, l. 26-27: A new FTIR station is measuring now in Brazil (Porto Velho), since July 2016. Although it is not yet an "NDACC" station, I think it's worth to mention it.

- p.5, l. 22: "listed in in Table 1": remove one "in".

- p.6, l. 8: "Observations at Altzomoni" (not Atzomoni)

- p.13, l.30: "expected", not expect.

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