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Atmospheric Measurement Techniques Discussions

Interactive comment on "Investigating the long-term evolution of subtropical ozone profiles applying ground-based FTIR spectrometry" *by* O. E. García et al.

Anonymous Referee #1

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General comments

This paper addresses a relevant question within the scope of AMT: the authors investigate different retrieval setups for ozone measured by FTIR spectrometry, in order to examine if the currently widely used (so-called "NDACC" in the paper) retrieval setup can be improved. Especially, they compare the long-term evolution of ozone partial columns using the different retrieval setups. Knowing that FTIR ozone measurements have been recently used for trends studies at several stations using approximately the "NDACC" strategy (Vigouroux et al., 2008; WMO 2011), it is of scientific interest to

learn if this FTIR contribution to the ozone monitoring can be improved. Furthermore, the authors carefully compare their different ozone time-series with ECC sonde measurements. They also compare the trends obtained by the two different techniques, which gives confidence in the FTIR ability to provide reliable trends.

Before the paper can be accepted for publication in ACP, I have some major comments and questions (mainly comments 3; 5.1; and 8) in order to clarify the conclusions reached by the authors. I think these questions can rather quickly be answered by the authors and will not affect the publication of this paper in AMT. Other comments and technical corrections are included together with the major ones, following the structure of the paper.

Specific comments

1) Title

Since a lot of work has been done also on ECC sonde measurements (consistency of the time-series / comparisons with FTIR / trend estimation), the authors could maybe include this in the title (the importance of using 2 different techniques in the paper is expressed by the authors in the first paragraph of Sect. 6.1). This could give more visibility for the paper to the ECC sonde community (this is a suggestion, the authors can decide).

2) Abstract

- l.9-12: "our theoretical calculations indicate that a very precise knowledge of the instrumental line shape is mandatory for a precise g-b FTIR remote sensing of stratospheric ozone". The authors write at other places that a very precise knowledge of the ILS is mandatory for precise determination of the upper stratosphere ozone layer (Sects. 3.4; 3.5; 6.1; conclusions). It is a very interesting point, and I wonder if the authors could demonstrate this in a more precise way. They show that the ILS is a dominant source of error, and they give the error contribution in the case of a precise

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knowledge of the ILS (cell measurements). Could they give a rough estimation of the error on partial columns due to the ILS in case, no cell measurements being available, the ILS would be included as fitted parameters in the retrieval process, and/or in case the ILS is assumed to be ideal?

- l. 17-20: when one reads the sentence, one expects that the trends derived from ECC dataset are also -0.3%yr-1 and +0.3 %yr-1, which is not the case. The trends from FTIR and ECC agree within their error bars, and the FTIR trends are -0.3%yr-1 and +0.3 %yr-1. I think also that it is worth to mention in the abstract that these trends (from FTIR) are statistically significant (and/or to give the confidence intervals).

- last sentence: I think the authors should be more careful when they link their observed trends to the increased circulation in response to climate change. Indeed what they observe, especially in the lower stratosphere is linked to change in the Brewer-Dobson circulation, but within a such short term period, this can also be due to the inter-annual variability of this circulation (WMO 2011) rather than long-term change due to climate variability (or it could be a combination of both). Also, in the upper stratosphere, if the effect of climate change is indeed found to increase ozone in that layer, it is certainly combined with the effect of the decrease of EESCs (Fig. 3-21, WMO 2011). In Hegglin and Sheperd (2009), the authors avoid the effect of EESCs by showing the differences between two periods where the EESCs are supposed to be equal. But the present paper deals with data that are in the decreasing part of the EESCs time-series, so it is expected to see an impact in the upper stratosphere.

3) Introduction

- First paragraph: as for the abstract, the discussion and references on ozone expected trends should mention the effect of declining EESCs in the upper stratosphere.

- The introduction is incomplete for the good understanding of the context of the present paper: previous long-term evaluations of ozone partial columns time-series have been made at several FTIR European stations (Vigouroux et al., 2008; updated in WMO

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2011). The Izaña station is one of these stations. The present paper deals with one additional year of data compared to WMO 2011. This should be mentioned, together with the clear statement of one of the scope of the paper: examine if the "NDACC" retrieval setup can be improved, especially for the measurement of the long-term evolution of ozone.

4) Section 2

No comment.

5) Section 3

5.1) General comments on Sects. 3.1; 3.2; 3.3 to clarify the conclusions about the different retrievals setups:

a) One information is missing before making conclusions based on Tables 2 and 4: how did the authors choose their Tikhonov constraint? By tuning the regularization strength, one could reach with setup A and B (DOFS=3.84 and 4.10 in Table 2), the same DOFS than setup C (4.20). So what was the criterion to choose the regularization strength (minimizing the total error?)? Since the smoothing error is the dominant error, it is important to explain how the authors obtained the DOFS for the Tikhonov setups A and B, especially when comparing with setup C.

b) Is the Tikhonov constraint the same for setup A and B? In case the answer is yes, it is interesting to note that the DOFS is increasing at all layers when the temperature is retrieved, and I would be curious to know if the authors observe the same increase when they use OEM: could they give this information with one sentence (DOFS setup C compared to DOFS setup C without temperature retrieval)? The test could be done on a small set of representative spectra – no need to run the entire time-series. Also the errors are decreasing at all layers by using the temperature retrievals and I would like to know if this is the case also for the OEM retrievals (probably the answer will be yes). Then the authors can indeed recommend strongly to the FTIR community the

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use of the temperature retrievals to obtain more precise ozone partial columns.

c) Two changes have been made between setup B and C: the use of a realistic Sa matrix instead of Tikhonov regularization and the use of an inter-species constraint between the different ozone isotopologues. Then, I would like to know which one of these two changes made the largest impact on the differences observed in DOFS and TRE. Did the authors make an intermediate setup between B and C?

d) It is clear from Table 4 that the setup C gives a better precision than setup B in the two lower layers, especially in the troposphere. However, the precision is worse in the upper layer, which is also a layer of scientific interest for the study of ozone recovery. So, I would advise the authors to be more nuanced when they write that setup C is the optimal one. Also, what about this Sa matrix at the altitudes above the ECC sonde measurements? Could the authors find another climatology for these altitudes (from satellite measurement)? This loss of precision in the upper layer is due only to the use of OEM or could it be improved by using also an appropriate climatology above the altitudes of the sondes?

In agreement with the better precision in the lower layers, "setup C" is better when comparing with ECC sondes (Fig. 9). Also, the Brewer comparisons are improved with setup C (even if the temperature retrievals have a larger impact on the precision of total columns). So, to strengthen their conclusion on setup C, I would add in Table 4, the errors for the total columns.

The discussions about the better (or worse) precision of one setup compared to another are valid in the case of a Tikhonov constraint chosen in order to minimize the total random error. Otherwise one could ask himself if it would not have been possible to obtain similar errors values for setup A and B than for setup C by tuning the constraint. (so same question than comment a): how was chosen the Tikhonov constraint ?)

e) How SE (Table 4) is calculated? Using the same Sa matrix for each setup? It should be explained in the text, especially since these values of SE are used to determinate

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the best retrieval setup.

f) Fig. 4 – Table 4: Maybe the authors should mention that the errors profiles plotted in Fig.4 are the diagonal elements of the error matrices of the different contributions, and that these error matrices have off-diagonal elements. It would be very helpful for the discussions to include in Table 4 the significant contributions to the partial columns errors (temperature, noise, ILS).

Maybe giving a Table (or additional column in Table 4) for the contributions of the systematic errors could help also for the discussions on the possible effect of ILS on the trends (if the authors try to go deeper in the discussion about the ILS, as suggested in comment 2).

5.2) Section 3.1: context of the work

After a complete description of the context in the introduction part, the authors should explain in Sect. 3.1, what was the strategy used at Izaña in Vigouroux et al. 2008 and WMO 2011. And maybe (if not already done in the introduction), they could say a few words on which station uses which strategy in this previous work on ozone trends.

5.3) Section 3.2:

- p.3437, l.15: The avks are usually described as the rows of the matrix A (Rodgers, 2000) not the columns.

- p.3438, l.8: "When interpreting the FTIR time-series it is important to consider the time evolution of avks": I did not find if (where) the authors took this into account in their trend study.

5.4) Section 3.3:

- p.3438, l.22: "The uncertainties are split into statistical and systematic contributions, 80% and 20% respectively. . .": How these numbers are obtained ? (same question for Fig.4: how is made the distinction between random and systematic part of the errors?)

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The authors should explain more or give a reference.

5.5) Sections 3.4 and 3.5:

a) Maybe (only suggestion) change the titles into:

- 3.4 Long-term consistency of FTIR measurements

3.4.1 ILS

3.4.2 Comparison between. . .

OR

- 3.4 Long-term consistency of the ILS

- 3.5 Comparison between. . .

b) When a significant bias is observed between 120M and 125HR ozone measurements (for the 31-42 km), is it taken into account for the trends calculation? Are the columns corrected? This should maybe be said / justified.

6) Section 4

- p. 3442, l.10 and l.15: Schneider 2008b instead of Schneider, 2008a

7) Section 5

- p.3444, l14: as shown “in” Fig.9

- The authors have chosen not to smooth the ECC sonde profiles with the FTIR averaging kernels. However, I think it would help their discussion to do so, for example:

a) p.3444, l.25: “The smoothing error might explain a large part of the discrepancy between FTIR and ECC. . .” It would be interesting to see if this discrepancy remains when the ECC sonde profiles are smoothed with the FTIR avks before they are integrated into partial columns. Also in that case, one would expect (from Table 4) that the comparisons would improve between setup A and B, especially in 22-29 km layer

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(since TPE is decreasing), but not anymore between setup B and C, where mainly the smoothing error is improved.

b) p.3448, l.17-21: the authors explain the difference in the FTIR and ECC sonde annual cycle by the smoothing error. This could be proven by applying the avks on the ECC sonde profiles. (idem p.3445, l.18)

c) If the authors could compare the trends of ECC sonde with and without the smoothing, this could give an indication on the effect of the smoothing error on the FTIR trends (their issue about the trend in the DOFS which could lead to an artificial trend – Sect. Conclusions)

8) Section 6

a) Context of the work: the authors should add one sentence to compare the obtained trends between the current paper and WMO 2011 (and to explain the differences).

b) One could expect that improving the precision on the FTIR ozone partial columns (from setup A to setup C) would improve the precision on the obtained trends. However it seems from Fig. 12 that this is not the case (errors bars are similar –even slightly larger for setup C and the 11-21 km layer). This could be due to the fact that the "noise" due to atmospheric processes (see Sect. 6.1) is more important than the noise due to the precision of the ozone retrievals. It is worth to mention this result of the retrievals setups comparison study: the better precision achieve with at least setup B (for setup C – it depends on the answers of the authors to the Sect. 3 comments) has no (or few – not clear with only a figure, and not given numbers) impact on the confidence interval on the trends, in the currently used model.

c) In the troposphere, the values of the trends with the different setups agree well within the error bars. However the conclusion is different: significantly positive for setup A; non significantly for setup B and C. What is surprising is that the larger impact on the trends comparisons occurs in a layer where the theoretical calculations of the random

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errors (Table 4) show the less impact: the temperature retrievals (from setup A to setup B) only improve the TRE by about 3%. Could the authors explain more what is happening at this layer when the temperature retrievals are performed? I guess the retrieved temperatures are more different than the a priori ones (from diurnal radiosondes) in that layer? Are the retrieved temperature realistic (i.e compatible with the radiosondes error bars) in that layer? The ECC sondes give a value closer to the setup A, but the conclusion (non significant trend) is the same as setup B and C. Would it be possible to obtain the trend from the surface data (since at the altitude of Izaña they are representative of the free troposphere)?

d) We see from Fig. 12 that the error bars on the trends obtained by the ECC sonde measurements are larger than the FTIR ones (especially for the 11-21 km layer). This is also an interesting result. Is it due to lower precision (5-10% for profiles as given in the paper) or to a different (lower frequent) sampling of the time-series (or combination of both)?

- p.3445, l.24: techniques (not technics)

- p.3445, l.25: Maybe (suggestion), the authors could be less assertive because some papers have been published on multi-regression models applied to short time-series (ex: Bodeker et al., JGR, 1998)

- p.3446, l.15: "...the bootstrap method, which assumes that the residuals are Gaussian...". I think this is not correct (Gardiner et al., 2008, p.6722, "This method allows the uncertainty associated with any of the model parameters to be evaluated without making any assumptions about the statistical distribution of the residuals").

- p.3447, l.13-16: For the quality – in general - of FTIR ozone retrievals in the upper stratosphere, the authors could maybe refer to Vigouroux et al. 2008: FTIR measurements at Jungfraujoch show very good agreement with Lidar measurements at Hohenpeissenberg.

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- p. 3448, l.8: troposphere (not tropopause)

- p.3448,l.16-21: see comment 7b)

9) Section Conclusions

- p.3449, l. 15: 1999 (not 19990)

- p.3449, l.25 – p.3450, l.2: see comment 7c). The effect of a trend in DOFS could also be tested by artificially decrease the DOFS obtained by the 125HR to the values obtained with the 120M, by tuning the regularization constraint. It would be interesting to know the influence on the ozone partial columns of such a "jump" in DOFS. Could this be tested?

- Since a large part of the paper is about the comparisons between the different setups, the authors should give their conclusions about this part (precision on the data themselves and implication for the trends).

10) Section References

- Barret et al: De Mazière, M (not Maziére, D. M)

- Lazante et al.: analysis (not anayliss)

- Redondas et al.: sensitivity (not sensitiviy)

11) Tables and Figures

- legend of Fig. 6: add that these plots are for setup C.

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