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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Zugspitze radiative closure experiment for quantifying water vapor absorption over the terrestrial and solar infrared. Part I: Setup, uncertainty analysis, and assessment of far-infrared water vapor continuum" *by* Ralf Sussmann et al.

Anonymous Referee #3

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This paper presents a nice description of the set-up at Zugsptize and an application of the observations at this site to determine FIR H2O continuum coefficients. The paper will be a useful reference to the observations at this station and make a solid addition to the literature on the experimental determination of the H2O continuum. We will recommend publication after improvements are made to this manuscript, especially those marked "major" below (which constitute the "specific comments" in this review).

pg 2

17 – Some species have collision-induced absorption so this isn't strictly true.

20 – "RRTM" is the full name of the code

23 – "aliased" is not the proper word. Maybe "which potentially introduces biases into applications ..."

25 – "e.g." should not begin a sentence

30 – Why not say H2O-air and leave out the "mainly"?

30 – Sounds better with "still a definite continuum theory does not exist..." 3 1 – It is probably not true that a consensus has been reached that both processes contribute appreciably, so it would be better to say "two possible physical processes".

pg 3

2- The wording here should be thought through more carefully. The foreign continuum would be a "dimer" of an H2O molecule with an air molecule, so perhaps not strictly a "water dimer".

3 – The Pfeilsticker et al. result is not viewed as very credible. Perhaps reference here the Ptashnik paper and another CAVIAR paper.

7-8 – I think MT_CKD is slightly different than as described here. The model is built off of a sum of monomer lines, but the "collision-induced term" (it's more appropriate to refer to it as in the Mlawer et al 2012 paper as due to a "weak interaction") is a presumed collisional complex between a monomer and another molecule, perhaps more of a quasi-stable complex.

13 – MT_CKD coefficients are uncertain everywhere, so perhaps say "are more uncertain".

14 – don't begin sentence with "e.g."

16 – 19 – This is a pretty old study. Also consider discussing the results of d'Angelis et

al. 2015

pg 4

5 – “measurement’, not “measure”

18 – Since the RHUBC-I results are so relevant to this study, it makes sense to list that campaign too 1 9-20 – Mlawer et al. presented such a closure experiment at the 2014 HITRAN meeting.

(major) 19 through pg5, 9 – This is one of many places in the paper where details pertinent to the NIR analysis are provided. These places detract from the focus of this paper, which is on the set-up at Zugspitze, which pertains to all experiments, and the FIR spectroscopic studies. These many text sections should not be in this paper, but in the one about the NIR analysis. Restrict mention of non-FIR material to aspects of the instrumental set-up at Zugspitze.

pg 5

10 – “maturate” is not a word. Perhaps “advance”.

pg 7

4 and elsewhere – this instrument is abbreviated “ER-AERI” by its developers

7 – the regular AERI (not the ER) was used in RHUBC-II in the Atacama

10 – front end is two words

11 – “... and two blackbodies..” This part of the sentence is poorly written.

22 – Remove “for numbers”

pg 9

10-15 – What O3 profile was scaled to to agree with the column measurement? MLW? Was it truncated below the Zugspitze altitude to get the 0.982 factor? This is unclear.

14 – The MLW and US standard are different profiles.

15-26 – This explanation should be improved. The phrase “has been used for routine operations” is particularly unclear.

22 – Is “stropheric” the correct term?

28 – Change “comparably high” to “comparable”.

Sec 4.4 – The authors should move this to the paper on the NIR. This material is not really relevant in this paper.

pg 11

3-7 – Again, this NIR information should not be in this paper.

9-14 – The material covered on pg 12, lines 23-28, should be in this section.

20 through pg 12, line 6 –

There are a number of aspects that could be improved about this section: 1) it is odd that Appendix B has only figures and no text. If these figures were part of a supplemental section, then this might be an acceptable space saver, but it doesn’t seem like that is the case. Either move the figures to the main text or move the text with the details about this analysis to Appendix B. 2) there is no context to understand Figure B1 since the reader doesn’t know if 200 ppm is a large or small percentage of the total H2O abundance. Could a second panel be added to that plot with the average H2O profile? 3) Fig. B2 is hard to interpret. Is what’s plotted the change in radiance for a one percent change in that layer’s H2O? I’m guessing the sign is different for the first layer (at least I think it’s the first layer – the two red colors are hard to distinguish) due to temperature inversions? It seems strange that the magnitudes of the derivatives for the first and second layers are so different since the H2O amounts probably aren’t that different. 4) From the text, it seems that the H2O profile uncertainty shown in Figure 4 is not simply due to the diagonal term in the covariance matrix but the layer-to-layer

correlations are taken into account. Is this correct? If so, it's unclear from the text how the math works. 5) ln 27 – Change "later closure experiment (Sect. 7)" to "the closure experiment described in Section 7" (assuming that's what is meant). 6) ln 29 – "set up" is 2 words 7) 28-29 – "mean of the moduli of the difference profile vector components" is hard to understand

pg 12

8 – What is "resimulation profile"? Assimilation?

7-22 – Was there no met tower at the site with a direct temperature measurement? Was the AERI T retrieval up to 3.5 km limited to just opaque CO2 spectral regions? Was the uncertainty in the retrieval itself (a posteriori) accounted for in this analysis? What about the uncertainty in the AERI temperature retrieval due to spectroscopic uncertainty? (major)

23-28 – Using the line parameter uncertainty codes is likely to cause a significant underestimation of the actual uncertainty. These codes come from HITRAN and aren't necessarily reliable. It is recommended that the authors get better estimates of the the uncertainty by comparing values in recent databases, such as HITRAN 2008 vs. HITRAN 2012 (and for widths, the values in Delamere et al.). For widths, differences of $\sim$20% are common – it might be reasonable to assume that for all lines in this uncertainty analysis. Also, given the low temperatures at this site, the uncertainty in the temperature dependence of the widths should also be accounted for, and it is unclear if it has. These values have had some very large changes from HITRAN 2008 to HITRAN 2012.

pg 13

5-9 – It is puzzling that the supposed dominant role of the H2O line parameters is mentioned first when, for the regions of interest in terms of continuum derivation, the dominant uncertainty is the H2O column (as shown in blue in Fig 4c). This paragraph

should be reworded to emphasize the key conclusions of the uncertainty analysis as it pertains to the continuum.

29 – It's unclear what the threshold of LWP < 100 has to do with snow accumulation on the LHATPRO.

pg 14

10 – Figure 4c indicates that the total uncertainty in the continuum channels between 400-500 cm-1 range from 2-3 radiance units. However, in Figure 6b it doesn't look like the underlying gray uncertainty is that far from the red parts of the residual curve, especially from 470-500 cm-1. Also, please define the residual to be obs-calc or calc-obs.

pg 16 (major) The authors have chosen to obtain the H2O column by a retrieval in the FIR, which presents the possibility of circularity since the same instrument, uncertain line parameters, etc. are being used the the column determination and the continuum derivation. Also, the continuum itself is an element of the column determination. Clearly the authors must believe that this provides a better estimation of the column than alternative sources for the column. Similar closure studies (e.g. Turner et al., Delamere et al.) have derived the column from microwave measurements near lines that have line parameters with low uncertainty, removing potential circularity and lowering a key source of uncertainty. No details are given for the column retrieval by the LHATPRO, but it may use a similar approach as used in these other closure studies. It would be interesting for the authors to provide the rationale for their choice for determining the column. Why do they feel it provides a better value than the microwave? How different are the column values obtained from each approach? Is this difference a good estimate of the uncertainty in the column amount they are using? A plot should be provided with these differences. What is the method used in the LHATPRO retrieval?

The definition and description of type-i and type-ii uncertainty (bottom of page) should be moved up to points ii or iii, instead of its current placement in point iv.

(major) The uncertainty due to the line parameters is likely underestimated here. First, as above, the width errors should not be obtained from the error code in the line file. Second, the temperature dependence of the widths need to be accounted for (assuming they are not). Lastly, comparing HITRAN 2012 to the aer line file that is used in this study shows that the width differences are not equally likely to be positive as negative – the signs of the differences are usually the same. Therefore, assuming that the resulting uncertainties are uncorrelated between different spectral regions is not appropriate. Although this type of correlation between different spectral regions may appear to be more accidental than other correlated errors (e.g. T profile), since the widths tend to come from calculations that are constrained to observed values, there may be a clear reason why they would generally be high (or low) in a particular version of the database. Reclassifying the line parameter errors as correlated (or somewhere in between correlated and uncorrelated) would increase the uncertainty in the column estimation.

19 – It is better to refer to this as “uncorrelated between wavenumbers” and “correlated between wavenumbers” rather than the current wording.

pg17

11, 19 – “ensues” is not the correct word

pg 25 Table C1 contains key results and should be moved from an appendix into the main part of the paper

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