General comments

The paper entitled *High-precision atmospheric oxygen measurement comparisons between a newly built CRDS analyzer (Picarro G-2207) and existing measurement techniques* describes a Cavity Ring Down Spectrometer devoted to the determination of oxygen concentration in air and to the delta ^{18}O isotopic ratio measurement depending of the instrument mode. The performances of the instrument are tested in laboratory by comparing measurements of well-known samples with results obtained using other techniques (IRMS, paramagnetic technique, Licor instrument) on the same samples. In the field measurements and comparisons are also provided by the authors at the High Altitude Research Station Jungfraujoch and at the Beromünster tall tower. The paper is well-written and detailed. The paper is now much easier to read thanks to the new way the experimental description and the obtained results are presented. The performances reached by the CRDS instrument are at the state-of-the-art for optical methods. The paper is fully in the scope of AMTD and is well-suited for a publication in this journal but needs some significant corrections (see the comments below).

Main remarks:

Part of the work in Fleisher et al. 2015 to which the authors refer in lines 135-141 seems to not be published. If authors have a published reference corresponding to Fleisher et al. 2015 they can let the text as it is but if they are not able to give a published reference they have to remove the lines 135-141 as well as the lines 144-146 and the reference.

The concentrations reported in Figures 8, 12, 13 have to be given in the same unit (per meg or better in ppm) to facilitate the comparison. For example in Figure 13 it will be better to plot the two layers (the paramagnetic and the CRDS measurements) on the same graph using the same units.

Specific comments

P2, L33: The authors have to recall the definition of *per meg* unit.

P2, L43:The authors have to recall the definition of *the oxidation ratio* and give some explanations.

P4, L86: The authors have to specify the reference of the pressure gauge, the proportional valve and acquisition board allowing stabilizing the pressure at the level of 3×10^{-5}!

P4, L91: The authors should give the typical ring-down time they have.

P5, L104: Give here the FSR value instead of p7.

P6, L125: The authors should cite the following reference: Tran et al., JQSRT (2019) 222-223, p108-114.

P6, L142-144: This sentence is strangely written and should be reformulated.

P7, L146-149: The excess noise observed by the authors is probably due to the fact that shorter ring-downs have less data points available for the fit. This should give a noise level increased by a factor of $\tau^{(-3/2)}$ where τ corresponds to the RD time.

P7, L158: Put also on Figure 1 the residuals when a Voigt profile is used. Moreover, the residuals observed on this figure seem to be due to a frequency shift and not to the limitations of the Galatry model. In the description of the Galatry profile, the authors don't mention the collisional line-shift parameter. Is this parameter taken into account for the calibration procedure? What about line-mixing effect?

P7, L166: *frequency of narrowing collisions* should be replaced by *velocity change collision rate*.

P7, L167: The authors should be more precise: …σ_D is the **1/e** Doppler **half**-width of the transition…

P8, L169-170: The units of k_B, T, M and c have to be given.

P8, L176: Cite HITRAN2016 instead of HITRAN2013.

P8, L177: Give the uncertainty reported in HITRAN data base.

P8, L178: Change the reference if required (see my comment above).

P10, L217-219: Not true for the line near 7878.45 cm^{-1} where the water transition dominates.

P10, L219-220: Three lines of HDO are missing in the figure but are present in HITRAN2016.

P10, L222-L224: This is not true as HDO lines are present.

P11, L245: *the z-parameter was constrained to be proportional to y, based on earlier measurements.* This is not clear for me. What are these earlier measurements and how they show that? Same remark for line 250.

P11, L251-253: How the missing HDO lines will be treated as the HDO isotopic abundance is not determined from the 7817 cm^{-1} window.

P12, L277: *sc**c**m* instead of *scm*.

P13, L299-308: Maybe adding equations will make this paragraph easier to understand.

P14, L326-329: What about the water lines in that spectral region (especially the H_2O line near 7881.98 cm^{-1})?

P19, L436: It is strange that only the last one minute of data was considered to determine the concentration for each standard by CRDS as each standard was flushed during 2 hours before.

P20, L464: *DAS* has to be defined.

P20, L474-478: It would be very interesting to know the origin of the problem and how the manufacturer solved it. As people from *Picarro* are co-authors of this paper it should be easy to have such information's.

P27, L631: *An excellent agreement was observed for measurements from both instruments…* Not so easy to check on Figure 18 (see the comment below).

P27, L634: …*contains large amount of water **and** CO$_2$ in addition to O$_2$…*

Figure 5, lower panel: Add the missing HDO lines (see HITRAN2016). For example the transition $4_{1\,3}$-$3_{1\,2}$ of the 111-000 band near 7878.500 cm^{-1} has an intensity of 3.47×10^{-27} cm/molecule.

Figure 14: What is the scale on the left?

Figure 18: The peaks corresponding to the flasks are not observed at the same time for the CRDS and IRMS. Moreover the delay between both experiments varies from one peak to the other. What is the reason for that? It will be better for comparison purposes to plot the two layers on the same graph.