Atmospheric
Measurement
Techniques

Discussions

Interactive comment on "Atmospheric CO_2 retrieval from ground based FTIR spectrometer over Shadnagar, India" by P. Mahesh et al.

Anonymous Referee #2

Received and published: 19 July 2016

The mansuscript "Atmospheric CO_2 retrieval from ground based FTIR spectrometer over Shadnagar, India" by Mahesh et al. describes – to my knowledge – the first ground based column-averaged dry-air mole fraction observations of CO_2 (aka XCO_2) over India. As such, these measurements could be very interesting if carried out over a longer time period. However, the current manuscript unfortunately lacks in a number of technical and scientific aspects.

Major comments

What I find most strange is that the authors basically repeat the approach of the Total Carbon Column Observing Network (TCCON): they use the same principal technique (Fourier-transform spectroscopy) to observe the same species in the same spectral regions. However, there is no mention whatsoever of TCCON nor are any of the dozens

of TCCON-related articles cited. Note that most of these articles appeared in open-access journals and are therefore easily available. So the mansuscript has clearly failed in summarizing the state-of-the-art.

Because they are not aware of the state-of-the-art, the authors fall into several traps that could have been avoided. They seem to use their own code for calculating spectra, radiative transfer and inversion. However, the referenced FASCODE 3 is very old (10+ years) and certainly not state of the art any more. The mentioned LBLRTA code is not referenced at all. While there is nothing wrong about using a different code, I would at least expect a comparison with some well-established code (GFIT, SFIT, PROFFIT). I fear that the authors have tried to reinvent the wheel here.

I am also not sure if the authors are aware of the very high precision and accuracy requirements for XCO_2 retrievals. The expected signals in the total column are in the order of only 1–3 ppm. To make a meaningful contribution, any new observations should have a precision of better than 0.25–0.3% (± 1 ppm). That is a very demanding goal and it takes a number of preconditions to reach. The first is a very stable and well-aligned FTIR spectrometer. The TCCON community does not consider the Bruker IFS125M suitable for this purpose because of its lower stability compared to the Bruker IFS125HR and the difficulties of aligning the interferometer to the necessary modulation efficiency. If the authors think differently, they should prove the stability of their setup, e.g. by providing more information about their instrumental line shape or by showing Allan variance measurements. Another precondition for reaching this level of accuracy is the combination of retrieval algorithm and spectral line data. The combination of the old FASCODE3 and the unknown LBLRTA is not convincing without additional validation. And the spectral data of HITRAN 2012 is known to be not accurate enough for this purpose. Therefore, TCCON observations have to be calibrated vs. aircraft in-situ measurements (e.g. Messerschmidt et al., Atmos. Chem. Phys., 11, 10765-10777, doi:10.5194/acp-11-10765-2011, 2011).

One other issue is that the authors seem to use volume mixing ratio, mole fraction and

concentration synonymously. However, they are different quantities that are not to be mixed up. Mixing ratio and mole fraction are both dimensionless quantities and the numerical difference may be small for many trace gases. It is however not small for gases like oxygen. If n_i is the number of molecules of a certain species and n is the total number of molecules in a given volume of (dry) air, then the mole fraction

$$x_i = \frac{n_i}{n} \tag{1}$$

while the mixing ratio

$$r_i = \frac{n_i}{n - n_i}. \tag{2}$$

For oxygen, the mole fraction $x_{O_2} = 0.2095$ whereas the mixing ratio $r_{O_2} = 0.2650$!

The combination of the factors mentioned above may also be the reason for the incredibly low XCO$_2$ values that the authors report in Fig. 3 and 4. The global CO$_2$ background already went above 400 ppm in 2014. To measure atmospheric XCO$_2$ values between 378 and 393 ppm anywhere in the world in 2016 would require an enormous CO$_2$ sink nearby. The day-to-day variability is also extremely high. It is much more likely that there is a strong bias in the order of 10–20 ppm in the results and the strong variability is the result of instrumental artifacts or an unstable retrieval.

Minor comments

The numerator in Eq. 1 should not be XCO$_2$.

Conclusions

I think XCO$_2$ observations in India would be very valuable and I hope that the following is not too discouraging. However, I do not think that the manuscript can be published as it is. The authors should contact the TCCON community and have someone look at their spectra and retrieval. I feel that the necessary changes go even beyond major revisions. My advice would be to re-submit the manuscript at some later time – possibly with more observations.

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