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Comment on amt-2022-106

Anonymous Referee #1

Referee comment on "Evaluation of the High Altitude Lidar Observatory (HALO) methane retrievals during the summer 2019 ACT-America campaign" by Rory A. Barton-Grimley et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-106-RC1, 2022

Review of Evaluation of the high altitude lidar observatory methane retrievals during the summer 2019 ACT-America campaign by Barton-Grimley et al.

General comments.

First I would like to mention that this paper show impressive work and results with respect to XCH4 estimate using lidar IPDA and even DIAL methods. The paper is well written and the figures are clear and well detailed which make easy the understanding of the measurements.

Although previous XCH4 airborne IPDA measurements with CHARM-F is mentioned some discussion of performances (precision, resolution, biaises) with respect to CHARM-F and maybe future validation of MERLIN space mission is missing. The paper is quite long and one may think that the HSRL measurements, as much as a detailed geophysical analysis of XCH4 data should be kept for a second paper.

As I said the paper seems to me a little long, however, in the same time, a fundamental part is missing: the consideration and correction of statistical biases in DAOD and DIAL estimates. Such omission can lead to misunderstanding of differences between in situ and lidar measurements. Therefore, some of the results in this paper should be calculate again and corrected and this is the reason why I indicated "major revision". I recommend to make the following correction before consideration for publication.

Specific comments.

- **Instrument:**

- L162. Note that main difference with CHARM-F is the OFF wavelength 1645.86 nm -> 1645.37 nm on the other side of the CH_4 multiplet. Recent spectroscopic data on H_2O absorption lines [Delahaye JQRST 2019] show that minimising H_2O impact on CH_4 DAOD requires to use the OFF-line at 1645.86 nm. This should be maybe indicated or at least taken into account if the authors plan to contribute to a future validation of MERLIN CH_4 space lidar mission

- **Opical depth biais correction:**

- L334 and Figure 6c. Why the biais correction depends on the gain Figure 6c ? I don't see a potential explanation in all is described L334-342.

Moreover, as the main difference of biais correction is shown for the LOLE channel (with the lowest SNR) one may wonder if the necessary correction of averaged optical depth with low SNR have been taken into account in the signal processing (Tellier et al. AMT, 2018) ?

The reviewer notes that nothing is said about statistical biaises in IPDA/DIAL in theoretical paragraph 2.2 and further in the paper which is not acceptable.

If no statistical biais is considered, the a posteriori biais correction used by the authors in paragraph 3.1.1 is clearly SNR dependent and this should be indicated even if it is not obvious in the DAOD estimates.

- L406. Precision performances of HALO should be compared and discussed with respect to Amediek et al. AO 2017 paper and CHAM-F results.

- Figure 9 and L422. XCH_4 noise statistic decreases less than the square root law. We can find a similar result in Amediek et al. 2017. The reason that is suggested by the authors is « harsh operating condition in the C-130 » and « high vibrational environment » which is fully possible and may entail a « degraded laser frequency stability » … what about optical misalignment issue? did the authors make some vibrational tests of their system?

- **Regional scale observations**

- Figure 13. and L530-535. The unexpected result of larger IPDA XCH4 than PBL in situ CH4 is very unusual. This shows that in situ data measured both in the free troposphere and in the PBL may not be sufficient to make a validation of space-based measurement such as MERLIN. Co-located airborne measurement and maybe XCH4 profiling with ground-based lidar should be used to explain such enhancement of CH4 in the free troposphere.

- I think that the second part L550 to L600 is not necessary in this paper (although really interesting!). Also, HSRL measurements seem not to be so essential in this paper as the authors proved that 1.645 µm backscatter is sufficient to give the vertical structure of the atmosphere and even, I guess, the height of the PBL.

- **Advanced CH4 products - Atmospheric profiling**

L 646- 663. and Figure 17

- L 646. The estimate of the DAOD profile is confusing. Did the author slice average the backscattered profiles to 350 m and 15 s first before using equation 6 ? It does not look this way given the variations of DAOD profile in Figure 17b… Signal processing should be clarified here.

- A decreasing DAOD is of course not expected and I agree that this may be a manifestation of low SNR. I have then the same question as for IPDA measurement: did the authors make an estimate of the statistical biais on the DAOD (and thus a correction) due to the non-linearity of equation 6? this question is linked to the question just above giving that an averaging enables to increases the SNR and then to decrease such biais… once again please read Tellier et al. AMT 2018 but this issue was also mentioned in early DIAL measurements with high precision such as for CO2 (Gibert et al. JTECH 2008)

As for IPDA, a correction of DAOD with statistical biais is a basis for modern DIAL measurements and the authors should includes and discuss in details the impact on SNR on their measurements. This is to my mind essential.

However the authors should be aware that the correction of DAOD with SNR linked biais might not be sufficient to remove entirely the decrease of DAOD seen in Figure 17b. At low SNR, especially for ON line signal the impact of Pb removal in Equation 1 may entail other issue due to the electronic baseline and linearity of the detection.

- L 658. A linear regression on the DAOD that is not weighted by error bars on each DAOD point is biased for the reason mentioned above and non linearity of equation 6. Gibert et al. AO 2006 used such likelihood estimate to make accurate XCO2 measurement in the PBL. In Figure 17b the DAOD will then not impact so much a likelihood calculated slope coefficient and I expect that there will be a better agreement with IPDA and in situ DAOD.

In conclusion the difference is not at this point explained by spectroscopy as the authors wrote (this sentence should be removed) but clearly by the non consideration of statistical biaises in their estimates.

- L710 - 720. Of course what is mentioned above should be considered in all this paragraph, i.e. the different statistical biaises should be corrected before the comparison of PBL XCH4 using the cloud slicing method and the DIAL profiles.

As already said before, spectroscopy induced error should be mentioned, if necessary, only in a second step.

Technical corrections

- L390. please remove one « is » in the sentence.

- L696. Please add an error bar for each retrieval IPDA ground, cloud, PBL

- 5.1 paragraph. As there is no 5.2 paragraph I guess that this title should be removed

- Conclusion must be re-written in agreement with statistical bias corrected results.