

Atmos. Meas. Tech. Discuss., referee comment RC1  
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## Comment on amt-2020-444

Anonymous Referee #2

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Referee comment on "Long-term column-averaged greenhouse gas observations using a COCCON spectrometer at the high-surface-albedo site in Gobabeb, Namibia" by Matthias M. Frey et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2020-444-RC1>, 2021

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The manuscript describes a new greenhouse gas observation site with a portable FTIR instrument in a remote site in Namibia. The site gives an opportunity to monitor GHG levels on the African main land. In addition, the high surface albedo of the site and the abrupt change in the albedo near the observation site makes the location a unique opportunity for satellite validations.

The analysis of the GHG seasonal cycles at this site and comparisons with GOSAT observations are very beneficial to improve our understanding of the global carbon cycle. However comparisons performed against TCCON and CAMS assimilation model and the conclusions made could be misleading in some cases.

Here are some general comments that I think would help improve the manuscript:

- Although Figure 1 is helpful to demonstrate the terrain around the measurement site I would suggest adding another figure zoomed out that shows where Gobabeb is located as well as the TCCON stations that are used in this study. This will give some better insight to the readers that are not familiar with the region and the TCCON network.
- It is mentioned that for the processing of EM27/SUN spectra PROFAST is used whereas TCCON uses GGG for the retrievals. Have you tried comparing the EM27/SUN retrievals with GGG and see how much difference it would make in the final results?
- Line 140. It is indicated that the accuracy of pressure measurements is 2-3 hPa. First of all what is the precision of the pressure sensor? Second, 2-3 hPa is a large bias. To put it in perspective TCCON requires an accuracy better than 0.3 hPa. Is it possible to calibrate the pressure sensor against a more accurate sensor to improve the accuracy?
- Line 181. It is mentioned that ILS measurements have been performed seven times since 2014 however the dates are not indicated. It is useful to know if ILS has changed after the upgrade done in 2018 to confirm if the changes in the scaling factors after

2018 are ILS related or not.

- Table 1, Figure 7 and 8. The comparison against TCCON sites that are thousands of kilometers apart from the measurement site is a bit misleading. Specifically in Table 1, the term "bias" implies one measurement is closer to the truth. Whereas in reality as you have indicated in the manuscript as well, these sites are probably under the influence of different air masses most of the time.

Specially for methane as the total column value highly depends on the tropopause height it's not meaningful to compare total column values at Goabeb and Lauder as the tropopause height is significantly different at the two sites. Generally speaking, I think the timeseries in Figure 4 is useful to give the reader a sense of the GHG seasonal cycles in the southern hemisphere, but the correlation plots and estimating the biases between the sites in my opinion is misleading and unnecessary.

- Figure 9. This figure is not very informative and as it is mentioned by the authors in the text, the curve shapes do not necessarily represent the diurnal cycle and it most probably is related to retrieval errors due to air mass dependencies, a priori profiles, etc. In my opinion, this figure is also misleading and unnecessary.
- Line 260-264. The authors conclude that based on the better agreement between CAMS satellite assimilated data and the COCCON measurements at Gobabeb the origin of the CO<sub>2</sub> draw down at the beginning of 2017 is from higher levels in the atmosphere.

First, CAMS is a model and there are errors associated with it. So the discrepancy might be due to the in situ CAMS assimilated product that could not capture the seasonal variations of CO<sub>2</sub> occurring near the surface. Second, even if we accept the assumption that the draw down might have origins higher in the atmosphere, 5000 m above surface seems a bit too high. It would be worth while to investigate the trajectories at lower levels for example 500 m and 1000 m above surface to be able to come to a stronger conclusion. You may take into account the instrument column sensitivity in choosing the vertical layers as well .

Minor corrections and comments:

- Line 14. We find a good agreement for the absolute Xgas values and representative diurnal variability [between TCCON and COCCON?!].
- Lines 57-62. The sentence is very long and the reader could be easily lost. Please consider rewording and breaking it up into multiple sentences.
- Line 67. It seems a sentence is missing here to motivate the current work. What is the mission of this project? i.e. investigate long term stability of EM27/SUNs? or quantifying emission strength from the region of interest or something else?
- Line 168. For M-gain observations... the sentence is a bit vague. please consider rewording.
- Figure 5 and 6 . What do different colors represent?

