The authors have done a good job addressing most of the initial comments from Reviewer 1 and Reviewer 2. However, I have a few comments that still need to be addressed:

Comments:
1. Abstract: in light of the author's determination that they cannot infer any information about local sources and sinks, line 25 in the abstract should be updated.

2. Section 4.1 and Table 2 (Allan Deviation, Precision of measurements): It would be helpful for the authors to present the Allan deviation as a figure rather than a table. If desired, there exists commercial software to calculate these (e.g. Stable32 and presumably others). Additionally, it would be helpful for the authors to restrict their Allan analysis to time periods when the CO_2 and CH_4 is well-mixed. The purpose of the Allan deviation is to provide information about the instrument (i.e. is it dominated by white nose or colored noise? Where does it flatten out and start increasing? Based on the time scale, can the factor that sets that turnaround point be determined?) rather than the long-term atmospheric variability.

3. Section 4.1, Open path – in situ bias: The authors quote two offsets for TCCON CO_2 and CH_4. One is listed as the "network-wide bias" and the other is "network-wide bias of X_{CO2} and X_{CH4}". Please clarify the difference between these two biases.

4. Section 4.2 CH_4 and Figure 11, especially the insert: The authors provide no explanation for the discrepancy between the OP and in situ instrument. The in situ instrument seems to show a diurnal cycle of about 50 ppb for CH_4, but the OP instrument seems to wander all over the place. Sometimes it is higher than the OP instrument (e.g. time periods on 21 Aug. and 22 Aug.) and sometimes it is significantly lower (e.g. 14-17 Aug.), but it does not seem to show any sort of trend or correlation with the OP instrument. This is in significant contrast to CO_2 and Figure 9 (especially inset) where it is clear that there is a tight correlation between the two instruments but an offset between them. The authors need to provide some discussion of this. Is the CH_4 spectral region affected more strongly by stray light than the CO_2 spectral region and variations in stray light could be causing this? (Table 3 seems to suggest that there is indeed an enhanced stray light effect.) Does the OP instrument light path cross anything that might be a CH_4 source (or sink) that would disperse by the time it reaches the in-situ instrument? The largest differences seem to occur when the wind is out of the SE (according to Figure 12b) but there is also a tight correlation at 330 degrees on ~1 Aug. time period (again according to Figure 12b). Based on Figure 4, the H_2O interference in the CH_4 retrieval window seems to be quite strong. Does the discrepancy between the OP/in-situ instrument correlate with water concentration or relative humidity?

5. Section 4.2 regarding the diurnal offsets: In addition to the temperature possibly causing diurnal offsets, it seems that stray light should also have a diurnal cycle. Have the authors tried correlating the (OP – in situ) quantity that varies diurnally with e.g. O_2 enhancement, or some other measure of stray light? (This would of course not explain the wind dependence observed for CO_2 though.)

Editorial comments:

1. Figure 7: the inset only has one tick mark labeled in the updated figure so it is not possible to tell the time scale of the spike width. Please update to include at least one other tick label.

2. Section 4.2: change to "For corrected CO_2…" and "For corrected CH_4…" to clarify that these are the resulting offsets after the high windspeed correction.

3. The authors should double check their manuscript for subscript errors.

4. The authors should check their references for superscript and subscript errors.

5. Table 4: The authors should clarify that this refers to their *uncorrected* offsets.

6. Figure 4: It would be helpful for the authors to also plot the other species that are retrieved in each window.