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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the Loss of Processed Natural Gas Within California's South Coast Air Basin Using Long-term Measurements of Ethane and Methane" *by* D. Wunch et al.

Anonymous Referee #1

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The work under consideration "Quantifying the Loss of Processed Natural Gas..." by D. Wunch et al. investigates the origin of increased methane emissions in California's SoCAB (South Coast Air Basin) region using remote sensing observations of carbon monoxide, ethane, and methane. Overall, the paper is very well written and fits nicely within the scope of ACP. I recommend publication, but would like to ask the authors to address a couple of issues in a revised version of the manuscript.

Observations from two different spectrometers are used in this work, the MkIV spectrometer covering a much longer period, but with sparser observations, and the Caltech TCCON spectrometer. I am a bit concerned with respect to the compatibility of the derived gas anomalies. The point is not primarily the spectral measurement itself, but the

significantly different sampling strategies of the measurements (MkIV observations are constrained to local noon). It would be instructive to demonstrate that an analysis of a reduced TCCON dataset (local noon observations only) generates compatible gas anomaly values, or whether the sampling strategy can introduce a significant bias.

The determination of gas anomaly values from the difference of afternoon and morning values is in principle a convincing approach. However, as small changes are derived from differences of much larger column values, I wonder whether the heating of the boundary layer during the day might also mimic a gas anomaly contribution? Is the analysis performed assuming a constant temperature profile? Is the heating effect a significant disturbance?

I have problems to understand that in Figure 2 the slope of the red dashed line differs between the top and bottom panels. If the slope is a function of time, why then is the slope in the upper panel so well defined (it encompasses data from several years, correct?).

The error bars on the symbols in Figure 3 are difficult to read. It seems that while the 2013 and 2015 results from MkIV and the TCCON spectrometer agree nicely, the discrepancy in 2014 is much larger than the indicated error bars. Is this a sampling issue (dates of observations used?)

The scatter of the FTS deduced ethane to methane ratios in Figure 5 is large. The error bars on the individual data points are quite variable and especially in 2015, the scatter between the data points is much larger than the individual error bars. Why? Does this imply that the uncertainty budget is dominated by a sampling statistics issue? What is the level of significance for the derived slope value? Does the regression fit take into account a weighting of data points accoring to the individual error bars? The figure might suggest a superimposed peak of high ratio values in the mid of 2013.

In Figure 7, the claimed steady rise of the slope during the observation period is hardly recognizable (due to the overlap of data points), perhaps a subdivision in several pan-

els spanning fractions of the whole period would improve the readability.

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