

Interactive comment on “The First Evaluation of Formaldehyde Column Observations by Pandora Spectrometers during the KORUS-AQ Field Study” by Elena Spinei et al.

Anonymous Referee #3

Received and published: 21 May 2018

The manuscript “The First Evaluation of Formaldehyde Column Observations by Pandora Spectrometers during the KORUS-AQ Field Study.” by Spinei et al. reports HCHO total column densities (VCDs) derived from a Pandora spectrometer measurements during a field study in Korea. The results are compared with ground-based and airborne in-situ measurements. In general, reasonable agreements of HCHO VCDs and surface mixing ratios are found between different measurements. The manuscript provides useful information concerning the data quality of Pandora HCHO measurements. Therefore, it is worthwhile for publication on AMT. It would be better if the manuscript can be improved by addressing my following comments.

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General comments

1. The introduction part emphasizes the importance of HCHO on atmospheric chemistry. However, I think it is also important to describe the current status of Pandora HCHO measurements (e.g., advantages, drawbacks, uncertainties, etc.), especially the necessity of performing inter-comparisons.
2. I suggest that the authors consider to reorganize the manuscript in the way that the methodologies, the results, and the discussions are put in individual sections. Within the current structure, I feel the readers could be messed up with informations such as how to convert mixing ratios to VCDs, how the measurement uncertainties are estimated, but possibly be distracted from the major concern, i.e., what the inter-comparison looks like and the reason for disagreement.

Specific comments

Line 18–25, Page 7: The AMF is calculated from a geometric estimation. If considering the effect of aerosols and clouds, how large would the AMF change?

Line 17, Page 23: As the authors described, the day to day agreement between HCHO VCDs derived from the three techniques varies a lot. The authors explain this as a result of “spatial and temporal heterogeneity”. I think the authors should spend more efforts on digging out the exact reason. Would it be possible for the authors giving some estimates on the heterogeneity of HCHO distribution? In Page 16, the authors has checked the spatial heterogeneity and only found a difference of less than 20%. Considering there were not time lag between the three techniques, I could not understand how temporal heterogeneity come from. Could it be the uncertainty of the Pandora measurements was underestimated under some circumstances? And could

it be the AMF for the days with large discrepancy been under-/over- estimated?

Line 23–24, Page 25: It is unclear to readers how the number -11% and -19% is calculated. Why DC-8 measurements could underestimate HCHO mixing ratio? The comparison between the ground-based and the airborne in-situ measurements were not performed exactly side-by-side. Therefore, in my opinion, the slope of the linear regression in Fig.5 does not really indicate that DC-8 measures lower values as described in Line 16.

Section 6, Page 28: I suggest the authors to re-write this part. (1) Separate the discussion and the conclusion. (2) Give concise conclusion by providing major findings.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-57, 2018.

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