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Atmospheric Measurement Techniques Discussions

Interactive comment on "Long-term evolution and seasonal modulation of methanol above Jungfraujoch (46.5 N, 8.0 E): optimisation of the retrieval strategy, comparison with model simulations and independent observations" *by* W. Bader et al.

Anonymous Referee #1

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This paper describes a long-term (17 year) dataset for methanol at the Jungfraujoch research station. The description of the dataset includes details of the retrieval strategy, error analysis and some analysis of the results. The subject matter is appropriate for AMT. This long-term record provides an interesting addition to the existing range of satellite, aircraft and ground-based measurements and is potentially valuable for model evaluation.

Nonetheless, I believe that there are a number of areas where the manuscript ought to be improved.

Major comments:

For the bulk of the analysis, the authors state that only spectra with solar zenith angles between 65 and 80 degrees were used. However, the authors then go on to devote a section of the paper to the diurnal variation of methanol. For the analysis of diurnal variation, the authors use cases with SZA as low as 30 degrees. The authors do state that the diurnal variation analysis was limited to cases where the DOFS > 1. However, the use of different subsets of the dataset for different parts of the analysis is not well justified. Are the cases with SZA between 30 degrees and 65 degrees of questionable quality or not? If these cases are OK, then why exclude them from the main analysis? More justification/explanation is needed here. (As an aside here, Figure 5 shows 3 lines per plot, but I did not find explanation of why there are three lines.)

The paper would benefit from improvements to Section 3, "Data characterization and error budget". On pages 5/6, the authors state that two different values for the SNR were used in the two different fitting windows, "since the fitting quality is significantly different in both windows". This SNR is presumably what feeds into the "measurement noise error" listed in Section 3. Based on the text here, I'm not sure that the "noise" here is truly noise. Is there no way to quantify the instrument noise independently of retrieval fitting quality? Couldn't the difference in fitting quality arise from interference from ozone (or some other molecule)? If so, then presumably it shouldn't really be characterized as "noise"? The discussion of the spectroscopic uncertainty associated with ozone is also rather vague. When the authors discuss the uncertainty associated with ozone in the HITRAN 2008 compilation, are they referring to intensity uncertainties? Width uncertainties? Which line parameters were "incremented" in the sensitivity test?

The value of the comparisons of the Jungfraujoch data with ACE-FTS 10 degree zonal

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monthly means is unclear. The authors clearly state that the ACE-FTS zonal means are capturing events that are not seen at the Jungfraujoch. The differences in March-May are huge. In this case, why bother to compare with the zonal mean? Why not be more selective in the ACE-FTS cases used in this comparison, and make some attempt to select the ACE-FTS cases that sample airmasses that are also likely to be sampled by the Jungfraujoch observations?

The paper mentions high methanol in air masses from the south in the context of the in situ campaign data. It would be interesting to see further expansion of this sort of analysis in the context of the seasonal variability of the Jungraujoch FTIR retrievals, although I appreciate that detailed analysis along these lines may be outside the scope of this particular paper.

Minor comments:

Page 2, line 29: “combined to” should read “combined with”.

Page 2, line 29: I would argue that the space-based estimates of methanol should be called retrievals or measurements rather than “detections”. “Detection” implies a lack of quantitative information.

Page 3, lines 4-5: “uncertainties in our knowledge of the methanol global sources and sinks in the atmosphere”. Firstly, I would suggest removing “in the atmosphere” from the end of this sentence. Secondly, this paper focuses on the Jungfraujoch dataset. While this is an interesting dataset, it represents a very specific location, and is not going to shed much light on the global sources and sinks. This paragraph is a list of statements. The authors might consider re-writing it to emphasize what their dataset brings to the suite of existing data.

Page 3, lines 12-15: What is the main point of this sentence? What is the “first” here? The first ground-based time series? (Presumably not, based on other references cited here.) The first time series at the Jungfraujoch? Perhaps, but that is perhaps not what

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makes this dataset special. I would have thought that what makes this dataset special is the length of the record, and the location of the Jungfraujoch station, ie where the airmasses come from and what sources are represented in these airmasses.

Page 3, line 29: “homemade spectrometer”? Homemade implies that some individual built it in their garage or something similar. Consider an alternative description? How about custom-made?

Page 4, line 18: “consists in” should read “consists of”.

Page 4, line 24: “have been added” should read “were added”.

Page 4, line 30: “illustrated on” should be “illustrated in”.

Page 5, lines 11-12: Should read, “Methanol features are much weaker, with mean absorption of 1.7 and 1.8 % for the “1008” and “1037” windows respectively.”

Page 5, line 13: “associated to” should read “associated with”.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4659, 2014.