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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term tropospheric formaldehyde concentrations deduced fromground-based fourier transform solar infrared measurements" *by* N. B. Jones et al.

Anonymous Referee #2

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General comments:

The manuscript by Jones and co-workers presents an interesting analysis of an 11-yr time series of ground-based FTS measurements. The topic of the manuscript is clearly within the scope of ACP. The authors derive factors like the annual cycle and the trend of HCHO from measurements which can be compared to model calculation. They also provide important data for the validation of satellite based HCHO sensors. However, their analysis and manuscript could be improved significantly. I recommend publication in ACP only if the following points are adequately addressed.

Specific comments:

The introduction on the HCHO chemistry focusses polar regions above snow surfaces, which might not be so important for Lauder. It would be helpful for the discussion of the HCHO sources to also indicate thephoto-chemistry of (biogenic) VOCs as the main precursors for HCHO. Moreover, in section 3.2, the biogenic precursor isoprene is discussed, but not snow and ice surfaces.

Why was the function for HCHO(t) taken in the form of Equation 1 with 7 fitted parameters? Later it is stated that the semi-annual and square parameters are not significant. Therefore, I suggest to use a simpler function. Gautrois et al. (2003) discussed the form of the function for seasonal cycle and trend; they applied Bootstrap re-sampling to independently determine the estimated standard deviations of the seasonal and trend part of their fitting function. Table 3 (Total column vs. FTS) already adverts that re-sampling would increase the errors of the parameters.

Figure 6 shows the correlation of HCHO by FTS versus GOME. The figure would improve if the statistical errors were included as error bars. How was the line fitted to the data, what are slope and intercept of the regression line? Were the errors in both coordinates considered? Please refer to Press et al. (1992) for a discussion of linear regression with errors and the question if the data are consistent with a linear model.

The section on model calculations is not very helpful and does not justify the conclusion that "... high HCHO values cannot be explained by oxidation of CH_4 alone." I suggest that the authors run their simple box model with included isoprene chemistry for one year (with spin-up) and perform a sensitivity study with respect to the level of NO_X (10ppt, 100ppt, 1ppb, ...), isoprene (off, summer 1 ppb, winter off, ...) and other significant parameters. Pöschl et al. (2000) and Karl et al. (2006) developed a condensed mechanism for the isoprene oxidation with HCHO as one of the products. The HCHO output of the different runs could then be compared to the measurements in an additional figure. Alternatively to own box model calculation on the basis of estimated

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input parameters, the authors could consider to compare their measurements to existing global model (see e.g. Abbot et al. 2003). In addition these models could be compared to the profile information from the presented measurements.

Minor comments:

1. Figure 2 and 3 could merge into one figure sharing the time-axis.
2. Please use HCHO or CH_2O in the entire manuscript.
3. "Long-term" should be specified when used in the abstract.
4. Table 3: How can the units of a_1 and a_2 be molecules cm^{-2}? Was the fit done with error weights?
5. Figure 5: red line is nearly invisible.
6. Figure 6: x-axis and y-axis could cover the same range, or introduce a 1:1-line.

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