

Interactive comment on “Global upper-tropospheric formaldehyde: seasonal cycles observed by the ACE-FTS satellite instrument” by G. Dufour et al.

Anonymous Referee #2

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The paper by Dufour et al. reports on satellite observations of upper-tropospheric formaldehyde by the ACS-FTS instrument. This study comprises three major sections: 1) The description of the retrieval itself and an estimation of the error budget depending on altitude. 2) The comparison to independent measurements from aircraft and from a different satellite instrument (MIPAS). 3) The comparison with two CTMs and its discussion.

In general the paper is an interesting and scientific important piece of work and merits publication in ACP. However, similar to reviewer #1 I have some serious concerns about parts of the discussion. The authors should rephrase the whole paper in a way to

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clarify that the presented data set has several limitations, not only in terms of accuracy and spatial resolution but in my opinion also in terms of spatial and temporal coverage. Therefore it seems to be very difficult to draw any serious conclusion on possible problems in our understanding of HCHO chemistry in the upper troposphere on a global scale. However, since ACE-FTS provides the first HCHO data set from space for the upper troposphere it is very valuable for case studies in particular in high latitudes.

In the following, I try to exemplify my concerns without repeating the issues which have already been raised up by reviewer #1.

Section 2, ACE-FTS measurements: Here the authors should give more details on the coverage of the measurements. It is not sufficient to mention that the majority of the observations are over the Arctic and Antarctic. HCHO is the major intermediate in the degradation of VOCs in the troposphere with most of the source regions close to the equator. How many times ACE-FTS has sampled the African or the South American rain forests? What about a table or simple 2D-plot with number of observations for the entire period (or splitted up in years) versus latitude band in 10 or 20 degrees steps? Is there any latitudinal variation in the altitude resolution? What is the variation of this resolution with height?

Section 4, ACE-FTS HCHO observations: The authors try to give explanations for several features in the ACE-FTS time series with some of them very hard to understand for the reader. E.g. p1062, l16: "The largest values are in the southern tropics and subtropics with maximum values during the JJA and SON periods near Africa and South America. They reflect the impact of biomass burning in the upper troposphere through either the direct injection of emitted HCHO or of precursors emitted by fires." Sorry, there are some higher values close to Africa and South America (Figure 6), but almost none of them close to biomass burning regions. I'm not going to say that there is no relationship between biomass burning and higher HCHO levels in that region but ACE-FTS observations are too sparse to support this assumption without any further evidence. Moreover, I would conclude from Table 5 and Figures 6 and 7 that

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highest values were found in "North America" and "Europe-Russia" in JJA. In order to study temporal variations in more detail, the authors have defined four different regions. These regions should be marked for better illustration in one of the global plots (e.g. Figure 6). For the selected areas the sampling is quite different depending on season. E.g. in JJA there are much more measurements in the southern parts of both "North America" and "Europe-Russia" regions than in MAM, when most of the observations are carried out close to the Arctic. This will introduce an additional seasonal bias and is not related to changes in emissions. Similar to reviewer #1 I have problems with the simple statistical error reduction by the square root of the number of observations when considering inhomogeneous regions like "Europe-Russia" (covering 35 to 80 degrees North!). What is the typical variance of the observations?

Section 5, Comparison with two state-of-the-art CTMs: When the authors claim that the inter-annual variation in the HCHO is weak (I would support this), what is the reason for not averaging the data for the different years in Figure 9? Why sometimes data points in the observations (Figure 9, upper panel) are missing, while the model data are shown (e.g. Indonesia hot spot in SON)? A more general comment: Again I would rather prefer to focus the whole discussion on those regions where the observations have a good coverage than to speculate on universal things. E.g. that GEOS-Chem has in global average a bias of only 2% to the observations seems to be more a godsend than scientific well-founded. For the negative bias between LMDz-INCA and observations in southern latitudes: why this should be related to the lower methane calculated in this model? What is the difference to high northern latitudes during wintertime where modelled methane concentrations are even lower compared to GEOS-Chem but the agreement to the observations much better?

Minor points: Table 5, Units missing; Figures 4 and 5 are dispensable; Figures 6 and 9 need a much better output resolution (e.g. 300dpi)

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1051, 2009.

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