**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Methanol from TES global observations: retrieval algorithm and seasonal and spatial variability" *by* K. E. Cady-Pereira et al.

T. Stavrakou (Referee)

jenny@oma.be

Received and published: 13 July 2012

A detailed description is provided of a new retrieval of methanol profiles and Representative Volume Mixing Ratio (RVMR) from the TES instrument. The retrievals appear to be most sensitive in the low troposphere. The number of pieces of information, the vertical resolution, the limit of detection and the errors are thoroughly discussed. Comparisons with GEOS-Chem model methanol RVMRs are also presented, pointing to likely emission underestimations in many areas of the Northern Hemisphere as well as over in the Southern Tropics during spring (i.e. during the burning season). These results seem to contradict partly a previous study using satellite (IASI/METOP) retrievals.

I agree with the authors that more validation data is urgently needed to (in)validate the satellite retrievals.

The manuscript is generally well-written and the methodology is sound. The topic is relevant to Atmospheric Chemistry and Physics. I recommend publication in ACP, if the authors address the following minor comments, mostly related to the presentation of the method and the interpretation of the comparisons with the model :

p. 11824 l. 8: The abstract states that TES provides "generally between 0.5 and 1.0 pieces of information". But in section 3.1 (p. 11831) we learn that about half the retrievals have DOFS < 0.5. Please clarify.

p. 11829 l. 14-15: please describe briefly the procedure used for the evaluation. Also describe shortly the aircraft measurements used here.

p. 11830 l. 14: "the a priori profiles are chosen based on location and month" : unclear, please explain more explicitly.

p. 11830 l. 22: "the variability is reduced in order to obtain tighter constraints...": please be more explicit. Explain the nature of this reduction. Also, why is the covariance matrix calculated in log space?

p. 11831 l. 18: it might be useful to provide separate figures for the DOFS over land and over ocean.

p. 11832 l. 6: as the other referee, I think the RVMR should be more precisely and explicitly defined. Also I'm not completely convinced that using the RVMR improves comparison with independent data or models, as discussed by the other referee.

p. 11837 l. 10-15: GFED2 is unavailable for year 2009, but other biomass burning inventories (e.g. GFED3) are available, which could be used to assess the representativity of the GFED2 data used in GEOS-Chem for 2009. This is of some importance given the strong interannual variability of those emissions in e.g. South America.

p. 11837 l. 17-18: "Figure 11 shows that TES often reports higher CH3OH concentrations than predicted by GEOS-Chem...": That statement is a bit simplistic. It is true that TES methanol is often higher than in GEOS-Chem in the Northern Hemisphere. But the opposite is also true over many areas, in particular over forests (over Canada, the S-E US, the Siberian taiga, etc.). It is quite striking that, similar to IASI vs. IMAGES in Stavrakou et al. (2011), the strongest model underestimation occur over dry/arid places such as Central Asia and the Western US. This should be mentioned.

p. 11837 l. 21-22: "higher values during the biomass burning season". Also somewhat simplistic. Here it could be useful to compare the spatiotemporal extent of those high values with available biomass burning proxies (MODIS etc.) or inventories.

p. 11838 l. 2-6: Comparison with the results of Stavrakou et al. (2011) are difficult due to the possibly different a priori emissions (Millet et al. vs. MEGANv2). Moreover, note that the IMAGES optimization in Stavrakou et al. was also evaluated against aircraft measurements from several campaigns.