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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Interpreting elevated space-borne HCHO columns over the Mediterranean Sea using the OMI and SCIAMACHY sensors” *by* A. Sabolis et al.

Anonymous Referee #1

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This manuscript focuses on the interpretation of the space-based OMI and SCIAMACHY vertical column densities of HCHO over the Mediterranean Sea. The authors divided their results into four sections: a) the correction of the OMI-AMF based on reduced dust emissions, b) the potential interpretation of the elevated VCD(HCHO) levels with the Net Primary Production (NPP) and the estimation of the marine isoprene emissions based on Palmer et al. (2006) assumptions, c) the comparison of the OMI-VCD(HCHO) with previous in situ and remotely sensed observations and d) the “direct” comparison with the respective SCIAMACHY VCD(HCHO). In most sections it is a well written paper; however the conclusions drawn are vague and further work has to be conducted before it is accepted for publication in ACP.

Main points:

1) As the authors stated, they reduced the GEOS-chem dust emissions by a factor of three following the recommendation of Generoso et al. (2008). Generoso et al. (2008) scaled the dust emissions -produced by the DEAD scheme - according to a previous study (Laurent, 2005) which corresponded to the climatology of the period 1996-2001. This reduction of the dust improved the agreement between the monthly mean simulated GEOS-chem AODs and the observed AODs (AERONET stations). Nevertheless for smaller time-steps and during dust episodes there was a clear overestimation of the simulated AODs close to the source regions (30%) and an underestimation in the remote regions in comparison to MODIS AOD. Given the fact that the computations of the OMI-AMF were performed on an 8-day basis the above will impact on the results. For this reason and due to the random occurance of the dust events it is suggested to perform the same analysis on broader time scale (e.g. months).

2) I am a bit skeptical on the validity of the conclusions drawn regarding the possible marine sources of HCHO over the Mediterranean Sea. There are several potential factors contributing to the observed columnar levels of HCHO over the Mediterranean Sea. In order to isolate the marine biological sources of HCHO someone has to take into account the following:

i) the transported primary and/or the secondary formed HCHO downwind terrestrial sources (e.g. urban areas, forest). ii) the HCHO emitted from biomass burning; it is known that dry summers make the Mediterranean region prone to fires. iii) omit the regions with intense ship traffic. Notably, recently Marbach et al. (2009) observed an enhancement of the mean VCD(HCHO) related to shipping emissions. Is it possible to isolate the ambient conditions that favor local productivity (e.g. low wind speed) and exclude the dust events that impact on the AOD and subsequently on the AMFs?

3) In my opinion the comparison of the OMI and the SCIAMACHY VCD(HCHO) does not provide any valid result and can be omitted from the text. Such a comparison could

have been meaningful if both the slant and vertical columns were retrieved with the same algorithms. In that case the observed differences could give important information concerning the diurnal variability of HCHO.

Could you please provide a physical explanation of the late winter-early spring peak of SCIA-VCD(HCHO) (Figure 5) which is not seen from OMI?

4) Instead of using the Vertically Generalized Production Model I would recommend to use the Carbon Based production model. The reason is that the latter accounts for the phytoplankton carbon biomass thus replacing chlorophyll as the metric of biomass. The VGPM is a chlorophyll based model. However it is known that [Cl-a] has a subsurface maximum below 30 m which is about 4 times higher that the surface level leading to large observational uncertainties.

It would be interesting to present a x-y plot with all the filtered (see comment 2) gridded (0.25x0.25o) OMI-VCD(HCHO) data vs. the VGPM and CBPM primary production.

5) To strengthen your conclusions I would suggest including in your study monthly averages of all months for the period 2005 – 2007 additionally to the four months presented here (Jan-Jun-Jul-Aug). Moreover I would highly recommend dividing the Mediterranean Sea into more regions (as proposed by Bricaud et al., 2002). This would give the advantage to identifyregions with clearer marine biogenic sources of HCHO.

Other comments:

1) Please give a brief explanation on the difference of the non-linear least squares and the DOAS technique (P17917 L24-25)

2) Please give a brief explanation of the “shape factor” and the “scattering weights”. Could you please provide a figure with a typical “Mediterranean” vertical profile of HCHO used in the AMF calculations? How different is this a profile in comparison to an urban profile?

3) Please name the absorption cross sections used in the analysis (including the proper references).

4) Please provide references for the LIDORT model (P17918 L15), theTM4 model (P17918 L16) and the DAK and FRESCO models (P17918 L16-17).

5) Could you please add the detection limit of the vertical columns of HCHO for cloud free condition (for both instruments?)

6) Please use vertical or slant columns instead of just using columns (e.g. P17914 L12, L16, L23; P17916 L27 etc.)

7) P17917 L4 Please add “the” at: study addresses “the” following

8) P17926 L12 Please correct the SCIMACHY to SCIAMACHY

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17913, 2011.