RESPONSE TO ANONYMOUS REVIEWER #1

We would like to thank the reviewer for his/her comments. We have done our best to address each of the points as detailed below.

Note: All reviewer comments in *italics;* all responses by the authors in normal font.

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 occurrence of the dust events it is suggested to perform the same analysis on broader time scale (e.g. months).*

We believe that the reviewer misunderstands the GEOS-Chem AMF calculations. The GEOS-Chem AMF values are calculated daily and then the columns are averaged, so the timescale issue does not hold.

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?*

We are somewhat surprised by this comment as, according to our study, marine isoprene and monoterpenes do not seem to be probable sources for the enhanced HCHO concentrations over the Mediterranean Sea. We agree with the reviewer that transported primary and/or the secondary formed HCHO, formaldehyde emitted from biomass burning, as well as intense ship traffic can all contribute to increased HCHO levels over the Mediterranean and deserve further investigation. However, this is outside the two main objectives of the current paper that are: i) consistency of spatiotemporal variation of enhanced HCHO vertical columns over the Mediterranean Sea, and ii) assessment of two potential causes of the problem - satellite retrieval error and ocean biological sources of VOCs.

In the updated manuscript, we have compared time series of HCHO vertical columns and net primary productivity (as a proxy for marine biological sources) averaged over the Bricaud et al. (2002) subregions of the Mediterranean Sea.

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?*

We agree with the reviewer. The updated manuscript uses the OMI data retrievals only. The title has been adjusted accordingly.

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 [Chl-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 (0.25x0.25°) OMI-VCD (HCHO) data vs. the VGPM and CBPM primary production.

The figure requested by the reviewer is now included in the updated manuscript.

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 identify regions with clearer marine biogenic sources of HCHO.*

Monthly averages of all the months and analysis with Bricaud et al. (2002) subregions of the Mediterranean Sea are included in the updated manuscript.

Other comments:

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

The use of SCIAMACHY data to compare with that of OMI and associated discussion of the DOAS technique has been removed.

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?*

Short explanations of "shape factor" and "scattering weights" have been added to the updated manuscript. The online supplement also includes two sample HCHO profiles from GEOS-Chem model used in AMF calculations.

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

The cross sections and references for the OMI sensor are included in the updated manuscript.

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

References for the LIDORT model have been provided in the updated manuscript, while with the removal of SCIAMACHY data the discussion of DAK and FRESCO models becomes irrelevant.

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

The detection limit of OMI vertical columns is included in the updated manuscript.

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

This has been clarified in the updated manuscript.

7) *P17917 L4 Please add "the" at: study addresses "the" following*

This has been corrected in the updated manuscript.

8) *P17926 L12 Please correct the SCIMACHY to SCIAMACHY*

The updated manuscript does not include the SCIAMACHY data.