Atmos. Chem. Phys. Discuss., 12, C7076–C7079, 2012
www.atmos-chem-phys-discuss.net/12/C7076/2012/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Latitudinal distribution of reactive iodine in the Eastern Pacific and its link to open ocean sources" *by* A. S. Mahajan et al.

**Anonymous Referee #2**

Received and published: 18 September 2012

The paper by Mahajan et al. describes observations of iodine monoxide and methyl iodide in the Eastern Pacific. These measurements are used for model and correlation studies indicating non-biological mechanism for the emission of iodine precursors. In general the paper is well written and an interesting piece of work and therefore merits publication in ACP. In particular observations presented here are very sparse and possibly provide new insight into the distribution of reactive iodine over the oceans. However, like referee 1 I have serious concerns about the analysis presented in this study and I therefore suggest a major revision of the paper before publication.

Detailed comments:

- Abstract: Please add LOS information for which slant columns up to 5*10$^{13}$



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molec/cm$^2$ have been retrieved (corresponding to 1 ppt in which layer?). Avoid comments on the comparison with satellite instruments since this has not really proofed with the data set presented here (see comments below).

- Introduction: Some studies are missing, Puentedura et al., 2012 (IO profile retrieval from Tenerife) or Oetjen, 2009 (IO observations from several sites, e.g. from the Maldives).

- Measurement techniques:

  - At least a brief introduction to the MAX-DOAS technique is needed since not all readers of ACP are aware on this. Give references like Hönninger et al., 2004, Wittrock et al., 2004, Roscoe et al., 2010.
  - What is the time for one measurement cycle (all elevation angles in one wavelength region)? "The data were then averaged between 10 to 30 min". Is this for one angle? How often the instrument was switched between UV and Vis? What about wavelength stability while doing this?
  - The IO was retrieved using the HITRAN 2003 cross section for $H_2O$. Why not using the 2009 data set, which accounts also for small water vapour features in the area of the strongest IO band? In fact, the residual shown in Figure 2 of this study exhibits strong variation around 427 nm.
  - Why the authors have not included inelastic Vibrational Raman Scattering by oceanic water in their retrieval? Recent studies (Vountas et al., 2003, Dinter et al., 2005) have shown that this is relevant for open water bodies with low chlorophyll.

- Results and discussion:

  - It is absolutely not clear to me, how the cloud index is calculated. Which wavelengths have been chosen? You have only 40nm in one spectrum, or



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not? Or do you have compared UV and Vis spectra not recorded simultaneously? What is the time resolution for the values?

- Definitely the weakest part of the paper is the description of the steps leading from measured DSCDs to mixing ratios. What is finally used in the further analysis: results from the OE or those from simple geometric assumptions with just one elevation angle? As reviewer 1 I do not see any serious error propagation in the results presented here. What about systematic errors (e.g. cross sections), forward model errors (e.g. a priori, aerosol load), smoothing errors, different wavelength regions used for aerosol and trace gas retrieval?
- p15548,l21: "The retrieved IO is confined within the boundary layer, with a steep gradient showing maximum mixing ratios at the surface." This statement has to be proven by showing profiles e.g. for one day with low and one with higher aerosol load.
- Looking to Figure 3 I would see only 6 to 7 days where DSCDs for all LOS are well separated. This might have two reasons: Either on the other days the IO is not in the lowest layer or the aerosol load is much higher. For the latter one I would assume that the profile retrieval compensate for this effect calculating similar results for the mixing ratio on all days. In fact, IO values shown in Figure S1 are between 20°S and 30°N in the same range even with the (too?) small error bars given here. Therefore the statement on [p15549,l22] that "the estimated amplitude of the change in $IO_x$ around the equator is different to that of the IO DSCDs mainly because of the low $O_3$" is not supported by the data set.
- At least in the supplement I would like to see the aerosol load retrieved from the O4 observations and a comparison between modelled and retrieved $O_4$ slant columns. The time series for the IO mixing ratio has to be shown as well in Figure 5, not only in Figure S1.



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- When IO mixing ratios used for the further analysis in this paper just based on 1° measurements: How sure are the authors that these measurements are not affected by reflection from the water surface (in particular on days with higher wave acitivity)? The FOV of the instrument is 1° which means that just 0.5° uncertainty in the LOS is needed to hit the ground.
- The authors claim that there results are inconsistent with the paper published by Schönhardt et al., 2008. I'm quite critical about that for several reasons. First of all there is a clear statement in Schönhardt et al. that enhanced values seen above the tropical Western Pacific over or close to upwelling regions have to be treated with caution, since the signal-to-noise ratio of the retrieval is poorer than for other regions like the Antarctic. For that reason they never calculated mixing ratios or did other further analysis using the results for low latitudes. Furthermore the cruise track does not really match areas where the satellite (might) see enhanced IO. Most of these regions are closer to the continents. Possibly a minor point: Schönhardt et al. report on observations from September to November while the cruise was in April. I suggest to remove any statements on the comparison to satellite observations or at least rephrase the paragraph taking into account the uncertainties of both data sets. As already mentioned above the statement on [p15552, l27] that the MAXDOAS data set of IO peaks around 20°S is not supported by the data when taking into account the error bars.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15541, 2012.