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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Iodine monoxide in the north subtropical free troposphere" *by* O. Puentedura et al.

Anonymous Referee #3

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This paper reports on first measurement of iodine monoxide in the tropical free-troposphere using multi-axis DOAS instrument. It is well suited for a journal as Atmos. Chem. Phys. but requires more work to be ready for publication. In my opinion, more radiative transfer calculations should be done to convince the reader that the observations are in fact consistent with a FT IO layer. The paper would benefit it the author try to infer consistent information on the IO vertical distribution. It would also be good if an English native could read and correct the text. Several parts are rather difficult to read.

Major comments:

-I think the discussion of page 8 - related Figs 5 & 6 – is incorrect or at least incomplete. The author explains the IO minimum around noon by the formation of HOI. I think that before speculating on a photochemical diurnal variation of IO, it would be good to

further relate the observations to the box AMFs used by the author. In fact, there is one fact contradicting the photochemical diurnal variation: the U-shape of the IO slant columns at low elevations angles is not observed for high elevation angles (70° and 90°). In the text, the discussion on a IO vertical distribution that could eventually be compatible with the observations is far too weak. The arguments given based on the IO/O4 ratio are not sufficient as both species have likely different vertical profile shapes (note this is also the hypothesis made to estimate the IO mixing ratio of 0.18 pptv based on equation 2). I think the author should at least try to infer at which altitude the bulk of IO is. A similar figure as Fig. 5 but showing the box-AMFs could help and convince the reader that the observations are due to a FT IO layer. It is a pity that from the box-AMFs shown in Fig. 7, I can not find any altitude for a presumable IO layer that could reproduce the IO SCD values of Fig. 5.

-Are the box-AMFs shown accounts for the large FOV of 6.5°? If yes, how are they calculated? This is an essential point to interpret the data.

- I found the explanation of the enhancement of IO inside the dust cloud rather fancy. Again, the discussion might be more interesting knowing the IO vertical distribution.

Minor comments:

Abstract:

-Please avoid acronyms in the abstract, unless it is absolutely necessary.

- The author should refer to the Network for the Detection of Atmospheric Composition Change and not to the Network for the Detection of Stratospheric Change.

-I don't understand the meaning of acronym IEA of elevation angle.

Introduction:

- P2, l3: UV-Vis-> UV-visible

- P2, l4: does "active DOAS technique" refer to DOAS using an artificial light source?

If not simply write “DOAS technique”.

- P2, l10: “has a synergetic effect..” could be replaced by “ reinforces the ozone depletion capacity of other..”

- P2, l24: “is supposed to have a small impact”. Pleas also give a range of values for the IO mixing ratios (pptv)

- P3, l4: avoid double parenthesis.

- At the end of the introduction, a sentence is missing, like: ‘In this paper, we report on the first observation of IO in the FT using MAXDOAS measurement at Izana subptropical station. Descriptions of the..”

Station description and meteorology:

Although I think the description of the meteorological conditions is interesting, it is not clear whether it will help the reader to interpret the IO measurements later in the paper. If it is the case, it is probably worth to mention it in section 2.

Instrument description:

- P4, l17: “diffuse radiation”→ “scattered radiation”

- One would expect to have details on the viewing geometry in this section: viewing angles sequence, integration time, etc.

- The lines 10-12, 26-30 of page 5 and 1-4 of page 6 should belong to section 3.1 in my opinion.

- IO retrieval: it is not explained why the spectra are smoothed by a boxcar filter. Is this filter also applied to the cross-sections. Figure 2: what is “ecm”?

Box-AMF calculations: for non-specialist readers, it would be good to explain why we need to calcule box-AMFs. Few sentences should be enough.

Results and discussion:

- P7, l1: It is a strange statement. From Fig 3, I found the day-to-day variability quite high although it is likely smaller than for the other elevation angles results displayed. Please reformulate.

- Figure 4: The author should probably remind that – under Saharan dust events -the increase of zenith sky radiances is due to multiple scattering while the decrease of 0° elevation radiances is due to a decrease in visibility.

- Figure 5: I suggest to add another x-axis with solar zenith angle; arguably more instructive than DOY

- It would be easier for the reader to have “elevation angles colors” harmonized for all figures throughout the paper.

- The Marine Boundary Layer Penetration Index is a concept difficult to understand. Please explain or add a reference

- P 10, l 17: I guess the author means measurements at 70° (almost zenith measurements). Please clarify.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27833, 2011.