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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Iodine monoxide in the Western Pacific marine boundary layer" *by* K. Großmann et al.

Anonymous Referee #1

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This is an important paper which adds significantly to our understanding of iodine chemistry over the remote low-latitude oceans, in this case the western Pacific. The important conclusions are: 1) that the IO radical is present at levels which produce significant ozone depletion; 2) the IO is highest in the tropical (warmest) part of the cruise; 3) there is no correlation with chlorophyll; and 4) the observed IO cannot be accounted for with the measured iodocarbon flux i.e., an additional source of iodine (postulated here to be I2) is required.

As the paper points out, these conclusions have been tentatively reached in other recent studies (e.g. a ground-based campaign at Cape Verde, and a cruise in the eastern Pacific). However, this study confirms the earlier work, and extends our knowledge of the global distribution of IO (which it seems cannot be achieved from satellite observa-

tions over the ocean).

My criticism of the paper is the length of the discussion devoted to the Max-DOAS technique for measuring IO, compared with the detail devoted to discussing the science, which leaves the paper unbalanced (I note that Dr Gomez Martin makes the same point in a comment). Max-DOAS is basically not a very good technique for making vertical profile measurements – which the authors demonstrate quite openly. In spite of all the correction factors which are applied, there is very little vertical information contained in the signal (the averaging kernels in Fig. 3 demonstrate this, as well as the text on page 27488, lines 8-10). All one can do is show that most of the IO is close to the surface, and use the lowest elevation angle measurements to estimate a mixing ratio. Of course, until some other measurement method is available, Max-DOAS is all we have so this is not a criticism of using the technique per se. The comparison between the Bremen and Heidelberg dSCDs in Figure 6 shows how sensitive the spectral deconvolution is to the various spectra that are included in the fit. It is not quite clear why this figure is included, since no further details are given about the Bremen instrument. Is this figure supposed to increase one's confidence in the technique – for this reader it had the opposite effect?! If the Bremen IO data is used subsequently in the paper, this is not made clear.

The modelling part of the paper complements the measurements very well. The method used to convert the modelled vertical profiles into the "degraded" vertical profiles that a Max-DOAS would measure is a clever approach to deal with the lack of vertical resolution in the Max-DOAS. One interesting thing that this exercise reveals is that the relatively higher IO observed early in the morning during part 3 of the cruise is consistent with I2 emission being a substantial iodine source.

Overall, this is an impressive piece of work which should be published after the authors consider the balance of the paper (see above), as well as the (mostly minor) points listed below.

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Minor points

page 27480, line 1: complemented, not corroborated.

page 27489, line 5: unstable, not instable

page 27491, lines 24-28: this is an interesting observation, which should be discussed again later on in the modelling discussion.

page 27492, line 26-27: this is another interesting finding. Is the interpretation that even elevated CH3ICl emission does not compete with I2? This should be discussed.

page 27497, line 7: the reaction of O3 with DOM does not self-evidently make iodocarbons; need to explain where the iodine comes from.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27475, 2012.

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Interactive Comment