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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Iodine oxide in the global marine boundary layer" *by* C. Prados-Roman et al.

Anonymous Referee #2

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The manuscript by Prados-Roman et al. combines field observations of iodine monoxide with a 3D global model analysis of the most likely sources of reactive iodine in the marine boundary layer. The data originates from multi-axis DOAS measurements acquired during the Malaspina global circumnavigation in 2010. IO mixing ratios averaged in the lowest ~600m of the atmosphere are reported. In addition, IO data from earlier field experiments are included in the study. The global 3D atmospheric chemistry model CAM-Chem with various oceanic iodine source parameterizations was used to compare to the data. The parameterization including organic iodine precursors and an inorganic ocean surface source of I_2 and HOI, according to the parameterization of MacDonald et al., appears to match the data best. The authors thus conclude that an abiotic marine surface source, which accounts for 75% of the emitted iodine, is globally active.

This is a well written that presents interesting data and model results and carefully argues for the presence of an abiotic iodine source at the ocean surface. However, there are a number of issues that require more detailed explanations before the manuscript can be published in ACP:

1) A number of filtering procedures were applied to the data. It appears that after the filters have been applied, no IO DSCD observations below ~1×10^{13} molec. cm^{-2} remain (Figure 3b). The insert in Figure 3b seems to indicate that all data with a 10°were excluded, even in scans where lower viewing elevation angles passed the filters. The exclusion of the larger viewing elevation angle data is rather counter-intuitive as MAX-DOAS retrievals often lead to smaller, or at least similar, residual RMS for larger elevation viewing angles and no other filter should remove these data points if the smaller elevation angles passed the filters. This must be explained in more detail. While the reported IO mixing ratios were only derived from the 2°observations, the results in Figure 3b open the question on how appropriate the filtering procedures were. The fact that only 2°elevation angle data was used to derive the mixing ratios should be mentioned in the main text and not just in the supplement.

2) One of the main factors in converting MAX-DOAS column densities into mixing ratios is the assumption of the boundary layer height. The accuracy of the assumption of a 600m high boundary layer and the height of the boundary layer in the model merit a more detailed discussion. Ideally, the comparison between the observations and the model should be made using a vertical column density, perhaps in the lowest 1000m of the atmosphere, as this quantity would eliminate the boundary layer height uncertainty and thus be more closely related to the emissions.

3) Does the 3D model include clouds? If so, was model data filtered in the same way as observations? If not, could there be a bias in the model as cloudy days were excluded from the data? This needs a more detailed explanation.

In summary, this manuscript is well suited for publication in ACP. I recommend publica-

tion after the issues described above have been addressed.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22217, 2014.