

Interactive comment on “Air-sea fluxes of biogenic bromine from the tropical and North Atlantic Ocean” by L. J. Carpenter et al.

L. J. Carpenter et al.

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We thank both referees for their careful reading and considered review of this manuscript which we believe has led to significant improvements. We would also like to point out we have slightly revised some of the numbers in this manuscript. Firstly, we reanalysed our permeation tube weighings for 2007 and based upon the new calibration factors, have increased the CH₂Br₂ concentrations for the RHaMBLE campaign by 12%. Secondly, we came across an error in the calculation of kw for the RHaMBLE cruise. Correction of this error has led to about 13% decrease in the fluxes of CHBr₃ and CH₂Br₂ for this campaign (i.e. the CH₂Br₂ fluxes remain more or less as they were). This does not change the conclusions of this manuscript.

Referee #2

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General comments: We have included more information on Manufacturer's details and also added a few sentences (beginning of section 3.3) on how the fluxes were calculated.

A larger array of data is now included in section 3.3 (see also reply to Ref#1, point 17). In section 3.5, when comparing our global estimates with other studies, we refer only to those studies which have either encompassed a very large data set (Butler et al.) or are a review of emissions (Quack and Wallace, 2003) for obvious reasons.

Units: flux units have been corrected.

Specific comments:

Minor comments relating to Abstract have been dealt with. There is already some discussion of CH₂Br₂ in the Abstract.

P18411, L5. Done.

P18411, L13. "fuller" replaced by "better"

P18413, L16-18. These typos are not in the original document, presumably they occurred during the ACP conversion of the file.

P18413, L19. Done

P18414, L9-12. See reply to point (11) of reviewer #1.

P18415, L15-18. Done.

Other data have been included including Klick and Abrahamsson (1992) and Quack and Wallace (2003). There are now at least 8 studies discussed, in addition to the review of Quack and Wallace (2003).

P18417, L1. Done.

P18417, L4-7. The discussion of CH₂Br₂/CHBr₃ (in air) slopes in Zhou et al. focuses on differences due to atmospheric effects (e.g. dilution and photochemical de-

cay) rather than differences in the marine emission ratio. It is clearly the latter point which is relevant to the discussion here.

P18418. The last few sentences of section 3.4 have been reordered and a summary sentence added.

P18418, section 3.5. Our fluxes for the Mauritanian upwelling are in accord with those of Quack et al. (2007a) (this point is now added, was missing before), and these we believe represent the only published flux measurements in this region, so it is reasonable to use them alone. As for coastal data, our approach means that we need to know the ocean depth corresponding to surface measurements, and this data simply hasn't been published. However, we have added a section in 3.3 that shows that the coastal data we have is in line with other measurements and, extrapolated to very shallow depths, is in accord with measurements made in seaweed beds.

P18418, L24 and 26. Corrected.

General comment on Figures - most of them are as large as they can be whilst still fitting within the margins! We feel that they are perfectly readable, however are willing to adjust them if the Editor views this as necessary.

Figs 1 and 5. The x/y (lat/long) scales and ranges for the halocarbon data and for the Chl-a image are indeed slightly different (we cannot change the scales of the satellite image), but since the satellite image is intended only to broadly convey areas of high biological activity, there is little to be gained in an exact match.

Figure 4. We have removed Fig 4 and discussion thereof - see point (13) of reviewer #1.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18409, 2008.

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