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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Bromocarbons in the tropical marine boundary layer at the Cape Verde Observatory – measurements and modelling” *by* L. M. O’Brien et al.

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Open comment to the Authors

I have now read all of the referee8217;s comments, and your detailed replies, as well as the revised manuscript (not itself available on line). All of the queries raised by the referees seem to have been dealt with satisfactorily in your responses, and in nearly all respects the revised paper is now ready and suitable for publication.

I do, however, have one remaining concern. This is the somewhat high absolute values that you report for many of the halocarbon species measured, and in particular the elevated 8216;background8217; levels which, presumably, should be regionallyrepresentative rather than locally-influenced. This is quite an important issue, not least for the modelling outcomes that you report based on these concentrations, but also because there is much current interest in compiling global observations (especially in the tropics) of very short-lived substances (VSLS). It is notable, for instance, that bromoform (also CH2Br2, CH3I) concentrations seem generally higher than those reported by Carpenter et al. (2007; see your bibliography) for the West African coastal region: a productive region where biogenic halocarbon abundances might be expected to be exceptionally large. Similar findings have been reported Quack et al. (2007; see your bibliography). The observations are even somewhat higher than measurements at Mace Head, Ireland, admittedly a temperate site, but which is almost immediately adjacent to very extensive and dense macroalgal beds (Carpenter et al., 2005, from your bibliography, and also Carpenter et al., 1999). I understand that the coast at Cape Verde is not heavily colonised by macrophytes.

There are several possible explanations of this, including that your measurements are indeed completely correct and accurate as stated! In that case Cape Verde may be something of a 8216;hot-spot8217; in the tropical mid-Atlantic. You have used the NOAA scale standard for most of your reported concentrations. There are at least two issues that could potentially compromise reliance on this standard: firstly the NOAA standard could have degraded over the two years between calibration and the field campaign; and secondly that some coelutant is interfering with the measurements (a more common problem with ECD detectors, as used here, than with mass spectrometers). On the coelutant issue; correlation plots of CHBr3 (for example) versus related species such as CHCl2Br might be illuminating as a non-zero intercept could be indicative of a coelutant (at least one that is not itself related to emissions of the target analytes).

I thought it would be useful to have this discussion now, so that all due assurances can be given as to the accuracy of the measurements reported.

Thank you for your consideration.

Additional reference:

Carpenter, L. J., W. T. Sturges, S. A. Penkett, P. S. Liss, B. Alicke, K. Hebestreit, and U. Platt (1999), Observations of short-lived alkyl iodides and bromides at Mace Head, Ireland: links to biogenic sources and halogen oxide production, J. Geophys. Res., 104, 1679-1689.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4335, 2009.

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