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Biogeosciences Discussions

Interactive comment on "Modelling the vertical distribution of bromoform in the upper water column of the tropical Atlantic Ocean" *by* I. Hense and B. Quack

Anonymous Referee #1

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General Comments

This paper studies the mechanisms controlling bromoform concentrations and fluxes in the tropical Atlantic Ocean. Given the scarce knowledge about $CHBr_3$ production and destruction mechanisms, the model discussed in this study is relatively crude, because both production and destruction processes are poorly constrained. However, this is one of the first studies describing $CHBr_3$ dynamics, and given its importance for atmospheric processes such as in ozone destruction, this early work is of importance for future climate studies.

My main criticism about this work is the lack of observational data used to validate the

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results of this study. Given the wealth of experience the authors possess in the field of bromoform measurements and the amounts of cruises described in their previously published work, more data could have been used better to validate 1) the model used 2) their findings (see specific comments below). For example, neither the biogeochemical nor the physical fields produced by their model are validated with observations (see specific comments below). Whereas annual means may be difficult to validate due to the scarcity of observational data, monthly profiles could help the reader to better assess the quality of the model and of the results.

Overall, this is interesting work and the paper is well written and well within the scope of Biogeosciences. I consider this manual suitable for publication, after some revisions have been carried out.

Specific Comments:

P 4921 L 27/8: The bacteria studied in Wahman et al. 2005 are *Nitrosomonas europaea*, a common model for a soil- and water-dwelling nitrifier. Please add the species name, as not all *Nitrosomonas* 1) may be able to co-metabolize bromoform or 2) occur in seawater.

P4922 L 2: I suggest you remove this footnote, as it doesn't contribute much to the general context.

P4923 L7: Please explain why you found no need to change the parameters. Have you checked chlorophyll-a concentrations? Verified that the biomass is within the observed range? As far as I know, there is quite a difference in the ecosystem composition to be expected between BATS and the Mauretanian upwelling area. Comparing e.g. community composition as described in Steinberg et al. 2001 for BATS with e.g Zubkov et al. 2000 for AMT, I see that both areas are dominated by Prochlorococcus and Synechococcus species, but there may be considerable differences in diatom/flagellate abundances (compare e.g. Tilstone et al. 2003 for the Northwest Iberian Upwelling). Hence, this choice should be better justified.

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Interactive Comment

P 4924 L 16-21: 'Supporting evidence..' This is no evidence, as the presence of the enzyme only does not tell you anything about if, how, when and where bromoform is produced. As an example from the marine sulfur cycle, the DMSP-cleaving enzyme DMSP-lyase has been identified in several plankton species, but their DMSP content of those species varies over orders of magnitude. And there are several DMSP producers that showed no DMSP-lyase activity at all in the laboratory. Furthermore, the second assumption is most likely wrong. I think it would be more realistic to say that, given that you have only one phytoplankton group, you can use only one value, and the aim was to use a value somehow related to observational estimates. Also, mention that the community you study is dominated by small picophytoplankton, some of which have been shown to express bromoperoxidase. Except for *Nitzschia sp*, the species you derive your values from are cold-water species. You should mention that this is a possible limitation of the applicability of the values you find.

P 4925 L 8: In your conclusion you mention the production of dibromomethane from bromoform as a process described already in Quack et al. 2004. Why has this process not been considered here?

P 4925 L 22: Why have you chosen your parameters in such a way that the estimated half-life lies outside the measured range?

P 4926 L 9-18: I am not particularly happy with the parameterization used here: Shouldn't nitrification also be controlled by 1) ammonia concentrations 2) oxygen abundances and 3) temperature? Why have you used light here? As oxygen is not modeled by your NPZD model, I understand that you couldn't use this as a proxy, but you do have nitrogen concentrations and temperature? Please justify your approach.

P 4927 L 17: Please justify why you think that Cape Verde is representative for the entire eastern tropical Atlantic.

P 4928 L 1-14: Here you definitely need to show some validation data. The reader cannot judge whether your model 'simulates temperature and salinity fields reasonably

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well', if you don't show the data. I suggest not showing annual means, but using some of your cruise data for an(some) individual month(s) instead. Furthermore, you should absolutely compare phytoplankton biomass to observations. You could e.g. estimate chlorophyll-a from N/C (Redfield) and C/Chl-a ratios of small phytoplankton (125 g C/ g Chl-a) and compare to satellite or in situ data (as, e.g. ,visualized in Quack et al. 2004).

P 4928 L 26: This could be formulated more precisely. The main point here is not that 'remineralization occurs above 200m' but that the strong temperature sensitivity of your remineralization ($1.066^{22} = 3.6; 1.066^{8} = 1.67$) leads to lower rates at low depths.

P4929 L 2: 'Escaping bromoform': Is there any parameterization of particle sinking in your model (I don't think so) or doesn't bromoform just simply accumulate at depth and experience some sort of advection and diffusion? Reformulate this sentence for increased clarity.

P 4929 L 9-10: 'Erodes the subsurface maximum..' Please reformulate, as we do not know in which way this should 'erode' the maximum.

P 4929 L 12-15: Isn't this due to your NPZD model being in steady-state, with an eternally reproducing seasonal cycle? I mean, don't phytoplankton production and consumption processes need to be equal in the annual mean, so that you do no longer see a net change in concentration over the years? Then it would be obvious rather than 'disappointing' that you do not see differences in bromoform patterns for those 2 sources after 50 years, if bromoform is proportional to phytoplankton production/consumption. Again, here it could be interesting to look at monthly rather than annually averaged profiles, because I would think that Q_1 and Q_2 have different seasonality, as you briefly mention in footnote 3. Hence, I suggest including the statement in footnote 3 in the main text and elaborating a bit more on the mechanisms.

P 4930 L7-12: If bromoform is of phytoplanktonic origin and if bromoform dynamics and phytoplankton dynamics are tightly coupled in your model, this outcome is hardly

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surprising. You could overlay both observed and simulated chlorophyll-a/biomass onto your plots and see whether they coincide. If so, bromoform could be described entirely using something like

B = alpha(z,T) x P

and you would be able to test whether the rather complicated source and sink terms you use are really needed.

P 4930 L 11-12: 'Given...' Please carefully revise this sentence. Which assumptions are supported by which feature of the model results? (see comment above)

P4930 L 20: Please reformulate, as it seems confusing to talk about the 'thickness of a profile'. Suggestion 'width, half-width', etc.

P4930 L22: Please compare the width of the subsurface biomass maximum with experimental data to support this hypothesis (see comments above on model evaluation).

P 4931 L22: I suggest comparing the simulated seasonality of bromoform with the seasonality of biomass, MLD and solar radiation (and wind speed) in Figure 5/6 as these are the variables your model seems most sensitive to. Could you derive simple diagnostic dependencies between environmental/ecosystem variables?

P 4932 L2: Please show biomass seasonality as predicted by your model in Figure 6. In addition, none of the subplots of Fig 6 are labeled a) - c).

P 4932 L2f: Can the high seasonality in $ChBr_3$ fluxes be supported by atmospheric data? Cape Verde Observatory? Cruises?

P4933 L13: 'in the presence of *some* nitrifiers'

P4933 L14-17: See remark above. Why don't you mention this mechanism already in the introduction and explain why you chose not to model it?

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Interactive Comment

P4933 L18: If the seasonality in $ChBr_3$ is really high, wouldn't you expect this to be reflected in BrO concentrations? However, BrO concentrations exhibit a much smaller seasonal cycle (max. factor 2, Read et al., 2008). Can you explain this? Can you show differences between summer/winter fluxes in $ChBr_3$ from cruise/atmospheric data?

Technical comments:

P 4938: Is there a way of labeling the 'observations' in a clearer way in the table? E.g. use 'M55 data' or something similar.

P 4928 L 1-14/ P 4939: Figure 1 a) - d) are not labeled

P 4929 L ... : Figure 3: a) - d) are not labeled. Also consider plotting these graphs in color, as it is nearly impossible to distinguish the different loss processes in d).

P 4942, Figure 4: Are your observations discreet or continuous? If they are ordinary measurements, please consider marking them with points. Otherwise the observations look like model results. I understand that the observational profiles have been generated from a multitude of vertical profiles, but the black line does not look continuous to me.

P 4944: Label subplots and change labeling in caption of Figure 6 (a) and c) are inverted).

Literature

Steinberg et al. Overview of the US JGOFS Bermuda Atlantic Time-Series Study (BATS): A decade-scale look at ocean biology and biogeochemistry. Deep-Sea Res. II - Topical Studies in Oceanography, 48(8-9):1405-1447, 2001.

Zubkov et al. Picoplankton community structure on the Atlantic Meridional Transect: a comparison between seasons. Progress In Oceanography, 45(3-4):369-386, 2000.

Tilstone et al. Phytoplankton composition, photosynthesis and primary production during different hydrographic conditions at the Northwest Iberian upwelling system. Marine

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ecology. Progress series, 252:89-104, 2003.

Interactive comment on Biogeosciences Discuss., 5, 4919, 2008.