

## ***Interactive comment on “Numerical modelling of methyl iodide in the Eastern Tropical Atlantic” by I. Stemmler et al.***

**C. Völker (Referee)**

christoph.voelker@awi.de

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### **General comments**

The paper by Iris Stemmler et al. presents a one-dimensional model study on the production of methyl iodide in the ocean.  $\text{CH}_3\text{I}$  is a halogen compound that plays an important role in atmospheric chemistry. It is not known, however, whether the main production pathways in the ocean are more from direct production by phytoplankton, or from photochemical degradation of coloured dissolved organic matter. Stemmler et al. approach this question by performing a number of different model runs using different literature-based assumptions on the generation of  $\text{CH}_3\text{I}$ , and comparing the results to ship-based profiles of  $\text{CH}_3\text{I}$  measured recently in the North-East Atlantic. This approach then allows to assess which assumptions on the generation of  $\text{CH}_3\text{I}$  are

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Interactive Discussion

Discussion Paper



compatible with the existing data and which are not, at least as long as the different source processes are themselves described correctly in the model.

The central result I learned from the manuscript is that neither inherent biological production by phytoplankton nor photochemical production can be excluded given the model results, but that a production by phytoplankton only under stress is unlikely because it leads to a strong maximum of  $\text{CH}_3\text{I}$  near the surface while the observations show rather a subsurface maximum. I think this is an interesting result and in principle warrants publication of the manuscript in Biogeosciences.

Nevertheless I have some reservations remaining about the results: I find that the authors do not show clearly enough that the model describes the source and sink processes with enough confidence, so that a mismatch between model and data can be interpreted. There are several aspects to this:

Firstly, the model contains a term that describes a photochemical destruction of  $\text{CH}_3\text{I}$ , and this term is assumed to be proportional to UV radiation (it is never mentioned in which wave length band, neither is the attenuation constant given), based on results in the atmosphere. I would consider this a not-so-well-known process. Unlike for the production terms, however, the authors have not performed sensitivity studies with respect this process, e.g. by varying  $k_{UV}$  or the attenuation constant  $a_{UV}$  (which is effectively assuming a different wavelength-dependent quantum yield). Here a small set of additional sensitivity studies could help.

Secondly I find, as the other reviewer, the evaluation of the biological state of the model (on p 1131) a bit weak. What is the vertically integrated net primary production and the vertical carbon flux in the model, and are there in-situ values to compare with? Could you perhaps use satellite Chl and NPP data to compare with, too? If the model-predicted biomass is off this would project onto the production rate of  $\text{CH}_3\text{I}$  as well. I think this mainly needs some evaluation and validation of the model runs, although the authors might also consider re-running the model not with monthly climatological

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forcing, but with daily or 6-hourly reanalysis data; this might improve the match of the physical and biological model.

Thirdly, the ecosystem model used is relatively simple and does not include prochlorococcus (mentioned by the authors as the probable main producer of  $\text{CH}_3\text{I}$ ) as a separate model variable. This is okay as long as the authors acknowledge this as a possible caveat and discuss whether this might contribute to model-data differences. Specifically, it is known that there are different groups of prochlorococcus that are adapted to different light regimes. As the focus of the paper is on producing the subsurface peak in  $\text{CH}_3\text{I}$ , this may be important. Perhaps the authors might have a look into the paper by Salihoglu and Hoffmann (2007), *J. Marine Res.* 65, 219-273 that discuss a model including an explicit description of different prochlorococcus and synechococcus subtypes.

Generally the manuscript is well written but could be somewhat more to the point sometimes. The discussion is still weak and does not discuss possible caveats enough.

I recommend publication after revision as indicated above. Minor comments follow below.

### Specific comments

p 1113, line 16: Was that an *atmospheric* global chemistry-transport model?

p 1113, line 25: 'observations'; you mean in the ocean here, don't you?

p 1114, line 10-12: why mention that GOTM can use very different parameterizations of turbulent transport, but not which one you are using?

p 1114, line 14: Which version of HAMOCC are you using? Are the standard parameter values for the model taken from one of the cited publications?

p 1115, line 2:  $\partial/\partial t(A_V \partial c/\partial z)$ ; the first derivative need to be with respect to  $z$ , not  $t$ .

p 1115, line 10-11: Are there indications that the production rate is linear in PAR and in

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Interactive Discussion

Discussion Paper



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DOC? Also, the unit for  $k_{photo}$  is such that the formula should rather be written as linear in DOP, not DOC. Implicitly it assumed here that DOC has a redfield-like composition, isn't it? This is very likely not the case, so it might make more sense to write the equation in DOC and to convert the unit of  $k_{photo}$  accordingly.

p 1115, line 14-15: This formulation sounds as if the model includes terrestrial production of DOC, which I believe not to be the case.

p 1116, line 2: It might be necessary to check whether the constancy of refractory DOC near the surface is still the state of knowledge. Perhaps check the overview paper by Hansell et al. (2009), *Oceanography* 22, pp. 202-211.

p 1116, line 16: Does inclusion of the respiration rate give more reliable results? I would think it is probably unimportant compared to the error in the calculation of the growth rate, and thus can be omitted.

p 1117, line 7-8: does it make sense to give the production rate to four digits of accuracy? Probably at most the first two are significant.

p 1118, line 14-17: chloride concentration in seawater is linearly proportional to salinity to a very high precision. Why do you use a constant value instead of having it vary with S? Probably the effect is negligible.

p 1119, line 3-5: I do not understand why photolysis is prescribed as proportional to UV, while the photoproduction is proportional to PAR. Probably for both processes the maximum quantum yield is in the UV spectral range, but UV is attenuated much more strongly. Can you give arguments why one process is better described through PAR, and the other through UV? Which spectral band of UV, anyhow?

p 1119, line 14: Is the atmospheric concentration of methyl iodide roughly constant over a seasonal cycle (or very low)?

p 1120, line 9-10: This is a very general statement. Is it needed?

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p 1120, line 13: If I got it right, GOTM assumes that the lower model boundary is at the sea floor and calculates a bottom boundary layer. Could this influence values in the mixed layer? Probably not, but check!

p 1120, line 18-20: Why do you use *monthly* values? Why a climatology and not a reanalysis? That might make the comparison to the in-situ profiles better.

p 1121, line 19-27: To me it is not obvious in which parameter space the optimization searches. It is also not clear to me why you present the cases with fixed (i.e. not optimized) parameter values at all, if you later also have a set of model runs with respect to the same parameter and could just present the best one.

p 1122, line 21-22: Why do you mention station 311 at all, if you later argue that you cannot model it because of strong upwelling?

p 1131, line 10-14: If that is the cause of the greatest mismatch, then why did you not use reanalysis data instead of a climatology?

p 1131, line 20-22: If you use an empirical C:Chl ratio to convert Chl observations to C biomass, why is it impossible to do the same in the other direction?

p 1132, line 6-8: should that not be mentioned early in the main text?

Table 1: 'Grazing rate' should be 'maximum grazing rate'. The 'phytoplankton half-saturation rate' is not a rate. Also it would be good to mention for which process it is the half-saturation constant. Nutrient uptake?

Table 2: It would make it simpler to grasp the differences between the different experiments if the parameters that are not applicable for a specific experiment were indicated by a dash instead a zero.

Figure A2: Showing phytoplankton biomass in phosphorous units is somewhat uncommon and makes it hard to quickly grasp an order of magnitude. Would it not be better to show carbon units, or perhaps even Chl units (which is probably what the data were

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in, originally)?

### Technical corrections

p 1123, line 21: comma after Opt3 can be deleted

p 1128, line 13: order of magnitude of -6? probably  $10^{-6}$ ?

p 1128, line 23: Syneococcus → Synechococcus

p 1128, line 28: Erros → Errors

p 1130, line 2: 'the strength OF modelled sea-air fluxes'

p 1132, line 8: 'will not be' should be 'are not'

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