

Interactive comment on “Methyl iodide production in the open ocean” by I. Stemmler et al.

Anonymous Referee #1

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This paper describes a study in which the authors test the sensitivity of simulated CH₃I to different production mechanisms in seawater. In general, I find this study to be marginally valuable, as there are enormous sources of uncertainty and unaccounted for variability that are not addressed. As such, it is not quite possible to sustain the conclusions in this paper based on the interpretation as presented, statistical or otherwise. Major corrections are necessary to permit publication.

General Comment:

1) The text is in many ways poorly written. It could be that the authors are non-native English speakers. In any case, many parts of the paper need extensive revision for clarity, correct grammar and punctuation. This undermines a good deal of my ability to properly interpret the material. Unfortunately, I am not patient enough to provide point-by-point recommendations for correction. In instances where the errors are egregious,

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I have included specific comments.

2) This paper does, in my view, present a solid review and evaluation of available observations. This is a valuable part of this study.

3) It may be my fault, but I cannot find where the atmospheric conditions are described. What I gather is that time-invariant atmospheric concentration were used. Why? The atmospheric boundary conditions must have an enormous impact on the MBL simulation results; and given the reaction rates in the atmosphere, and the overall lack of observations, some effort to evaluate the impact of the use of fixed conditions must be included.

4) Would it not be relevant to include atmospheric observations to confirm local air-sea exchange estimations?

5) How does the uncertainty compare between the source functions, their dependencies, air-sea exchange limitations, the resulting CH₃I concentrations, and their deviations compared to observations? What is the potential impact of atmospheric oxidation?

6) Are vertical gradients considered in the analysis? With soluble species such as CH₃I, local vertical gradients can be large, no?

7) Is mixed layer depth not a factor here? It would be a means of reconciling most of the ‘dependencies’ controlling the production variability. Mixed layer depth, combined with the vertical structure of production and concentration fields would be a useful analysis.

8) Error/Uncertainty estimates of model results are not included. For a model study like this, it is essential. As stated before, this is a major short-coming of this paper.

9) There is excessive reliance on supplemental information to support fundamental assertions. For example, P11/L6-7 (see specific comments, below).

10) Some specific non-spatial comparisons of model to obs. data should be added:

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For example, scatter plots would be of enormous benefit to interpretation of mapped & tabulated results. It is not possible to discern the model's performance from the difficult-to-interpret maps of cruise tracks.

11) The 'closest observed value' method outlined starting at P13/L26 is interesting but, in my opinion, flawed. For this analysis to have merit, an uncertainty analysis of all of the corollary data, dependent and independent sources of variability, compared to observations would have to be included that is able to state that the difference between model & observation is (1) statistically significant, and (2) to what degree the results are driven by difference in model skill in reproducing the CH3I production drivers (e.g. DOC, phytoplankton, dynamics, gas-exchange, etc.). The authors start to perform this analysis in Section 3.2, but the interpretation is weak and is not used in the interpretation of the results from the 'closest observed value' analysis.

Specific comments:

P8/L11: Define nutrient levels. What is "plenty"?

P8/L25 – P9/L2: This should be in methods.

P9/L9: Why is production reduced in the storm tracks?

P9/L16-17: By definition, isn't Labile & semi-Labile DOC bioavailable? Is microbial uptake not considered in the cycling?

P9/L27: It seems you're stating that CH3I produced from 'refractory' DOC? How is this possible? Is it a recognized mechanism?

P10/L21: In my view, mixed-layer depth is a prime candidate for these results.

P10/L22: Define 'significant'. Is there a p-value for this? Also on P15/L6.

P11/L6-7: The statement that the model reproduces observations "well" is a qualifying statement that should be avoided unless considerable quantitative interpretation is included. Supplemental information is cited here. But, I'd argue this is a fundamental

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piece of information that should be in the main text . P11/L26: Don't abbreviate language in main text. (approx. should be approximately). "Approximate" also suggests a statistic that is not given.

P12/L1: Statement not clear.

P13/L20-24: The authors assert that seasonality produced by the model but not seen in the observation is the fault of the data. It is reasonable to cite the need for more observations; but if a model result isn't supported by the obs., then it can be argued more likely the result of the model mechanics rather than lack of observation.

P13/L23-24: This statement is speculative, and not supported directly by results or analysis.

P15/L7-12: This is good. Tuning is a serious and unaccountable problem in modeling.

Section 3.2: The results from this section should be tabulated. Perhaps an ANOVA would help.

P16/L5: Can radiation and phytoplankton be considered independent variables? If not, the a correlation analysis is invalid.

Table 3: The table is hard to interpret. What is being shown in the 1st three rows? What are the units? I understand that the caption states this, but it should be clear in the table. Also, is a median RMSD the median of the root mean squared deviation? That seems self-contradictory. If the data are not normally distributed, is a RMSE even valid? I may be wrong here. Apologies if I am; but perhaps this should be clarified in the text.

Interactive comment on Biogeosciences Discuss., 10, 17549, 2013.

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