

***Interactive comment on “Concentration maxima of volatile organic iodine compounds in the bottom layer water and the cold, dense water over the Chukchi Sea in the western Arctic Ocean: a possibility of production related to degradation of organic matter” by A. Ooki et al.***

**Anonymous Referee #1**

Received and published: 14 August 2015

General comments: In this paper, the authors discuss distribution of VOIs and the factors in the Arctic Ocean. The authors show the correlations between VIOs and biogeochemical indicators. I basically agree with the statement of the authors, however, some parts of the contents are difficult to understand. The author, therefore, should consider and revise the following points. Moreover, I should indicate that the quality of presentation needs to be improved and I strongly recommend that English editing will be done before the publication.

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Specific comments: 1. Page 11246, Lines4-5, “VOIs are believed...”: I suppose the sentence doesn't need to be included in Abstraction section. I recommend that the sentence is omitted from the paragraph.

2. Page 111247, Lines 22-23: Please include the references which conducted the measurements of VOIs in the western Arctic Ocean.

3. Page 11250, Line 3: As far as I understand, headspace is usually not made for trace gas measurement. Why did the authors remain the 0.5-mL headspace for their measurement?

4. Page 11250, Line 14: Please include the reference for the purge efficiencies for VOIs here.

5. Page 11251, Lines 15-18: The authors should explain why we could regard the N-deficit as an indicator of the influence of organic matter decomposition, that is, “conceptual meaning” of using N-deficit values for their discussion.

6. Page 11252, Lines 18-19: The author defined the subsurface layer as the layer between MLD and CDW or ocean floor. Is this definition general in the research region? I confused this definition, because the authors used “subsurface” as different meaning in other parts, for example, “... though the subsurface CDW...” in Abstract section.

7. “Results” section The author reuse the sentence “We regard any \*\* concentration above \*\* pmol L<sup>-1</sup> as “high” in the Arctic Ocean” in the Results section. Usually, significant difference should be considered from not only the value above the average but also the value over the deviation of the average. That is, the authors’ definition has no significance.

8. Page 11258, Line 25: I’m confused from this sentence. Chl a is one of the indicators of phytoplankton abundance in situ, and phytoplankton abundance basically contributes to biological productivity. If the biological productivity is the main controlling factor for VOIs distribution in seawater, why did the Chl a have no relationship for the

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distributions? The authors should describe the possibilities to explain their results and why it does not agree with the previous studies.

9. “Discussion” section The authors should indicate not only “R” value but also “n” and “P” values for the statistical correlation analyses.

Technical correction: 1. Page 11259, Line11: Here, “Method” should be written as “Methods”

2. Page 11252, Line 3: Basically, the number of stations is shown in numerical order. The authors should revise it in the parts following as well.

3. Page 11254, Line 26: Please revise “~” as “-”.

4. Figure 2, 3 and 4: The labeling of the figure should be revised. For example, in Figure 2, “Vertical distributions of potential density (a), NH<sub>4</sub><sup>+</sup> (b), N-deficit (c), . . .” should be shown as “Vertical distributions of (a) potential density, (b) NH<sub>4</sub><sup>+</sup>, (c) N-deficit, . . .”.

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Interactive comment on Biogeosciences Discuss., 12, 11245, 2015.

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12, C4340–C4342, 2015

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