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Ocean Science Discussions

Interactive comment on "A three year time-series of volatile organic iodocarbons in Bedford Basin, Nova Scotia: a Northwestern Atlantic fjord" *by* Qiang Shi and Douglas Wallace

Anonymous Referee #2

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The manuscript describes a 3-year time series of weekly measurements of volatile iodocarbons in the cold-temperate waters of Bedford Basin. The authors use this time series, with associated data, to explore the controls on the concentrations of the three principal volatile iodocarbon compounds, to calculate air-sea flux rates of iodocarbons over the three years and to compare their results to previous time series in different regions. Directly determining the production and loss terms for these compounds and hence, being able to predictively model their emission from the oceans, is challenging and time series such as this provide useful insights into the key processes involved and key data for model validation. The 3-year duration of the study provides a unique insight into inter-annual variability of their production and emission to the atmosphere.

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Few other studies have examined the occurrence of these compounds in the deep water of fjordic basins, as carried out in the Bedford Basin. The study is appropriate material for Ocean Science and certainly worthy of publication but requires attention to the presentation of the results and would benefit from a more focused and more succinct revision.

Concerns:

1. Manuscript format: The manuscript could be improved by more careful attention to the referencing of results. Often the reader is referred to the wrong Figure or Table. It would also be worth reviewing the sequence in which the results are presented in relation to the sequence of the text.

2. Iodide/Iodate influence. There is an overemphasis, in my view, on the link between iodide concentrations and the production of iodocarbon compounds in relation to what is presented in the paper. Although it is plausible that there may be an influence of iodide concentrations, the authors present no new information to support this, nor do they convincingly link studies that have examined iodide/iodate transformation or iodide concentrations to their own datasets. For instance, the statement made in the abstract on this issue P1L17-20 has limited bearing on what is actually presented in the paper. The same is true for the sentence in the Conclusion P27L15-17.

3. Net production of CH3I. P11L21+ It is not clear from the mass-balance calculation of net CH3I production that the mixed layer depth is taken into account? Not accounting for the mixed layer depth may have been appropriate for the study in Kiel Fjord study (Shi et al. 2014) but in the deeper waters of the Bedford Basin for the mass-balance to hold, the net production should account for the full water depth that potentially exchanges CH3I with the atmosphere. The authors should also explain why they only attempt to estimate the net production of CH3I and not the other two compounds?

4. Air-sea flux calculation: P10L15+. Previously it has been shown that accounting for the air-side resistance to air-sea transfer has an appreciable decrease in the estimated

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flux of the more soluble volatile iodocarbons. For instance, the flux of CH2I2 was reduced by $\sim$ 30 % compared to water-side resistance-only calculations (Archer et al. 2007). The authors should state why they do not include the air-side resistance in their calculation, they may have good reason. However, it should be considered when comparing flux estimates of the different compounds and total iodocarbon flux estimates from different studies.

5. Deep-water section. Section 4.5, P21L10 to P25L2. 'Temporal variability in near-bottom water (60m)' could be usefully reduced in length in order to make it more readable and the message(s) more clear. The switch from alkylation processes that produce mono-iodinated compounds to a haloform-type process (P25L1) that is possibly linked to oxygen concentrations, is interesting but the message is lost in the complexity of the explanation of changing iodocarbon concentrations and hydrography that follows.

6. Unclear Conclusions. (Section 5) At present, the concluding section consists of a series of largely unconnected points that have been extracted from the Discussion and that lack a coherent structure. It would be more useful if the really important points were picked out and their significance described in the narrative. The easiest to read, conclusion-like comments come at the end of the section.

7. Figure and Table Legends. The figure legends and table legends could be more informative in general.

Additional points:

1. P3L5+. At some point the authors should point out that the three compounds they focus on are not the only volatile iodocarbons that are likely to be present, with minor contributions to the total from several other compounds including CH3CH2I, CH2BrI, and CHI3.

2. P7L7+: Temperature is the first environmental parameter described but does not appear in Figure 2. It would be clearer if the sequence in the text and figures corre-

sponded. Plus, SST is not shown in either figure but is referred to in the text.

3. P7L13. No information on the seasonality of irradiance is provided until Figure 7C but it is an important part of the explanations that the authors provide for seasonal trends in both polyiodinated compounds and CH3I. I recommend removing SSS from the current Figure 2, as it is effectively repeated in Figure 3, and replacing it with irradiance information.

4. P8L1. Figure 2b shows DIN not SSS, similarly for P7L18.

5. Table 1. Please explain what the numbers actually are in the legend, Pearson's correlation coefficient? An indication of the statistical significance would also be useful.

6. Table 2. Again, more information is required in the legend that describes exactly what data is being correlated and the significance of the correlations.

7. Table 3. The legend needs rephrasing, plus more information on when theses samples were obtained would put the results in context with the seasonal study. The sequence of Tables does not match the text. The information in Table 3 is not addressed until the Discussion, after Table 4 . How was the lack of significant differences between measurements determined (P13L10)?

8. Figure 5. Please add an explanation of what the bars actually show, weekly data presumably but from single surface iodocarbon values, wind speed averaged over the week, water temperature from a single measurement during the week; these all impact on the flux value.

9. The ratio of dihalomethanes to the total volatile organic iodine is interesting but would be a stronger point if backed up with a statistical test of significance and/or an indication of the range of values between regions.

10. P15L22+ this paragraph makes some interesting points but seems to have a mixed message regarding the potential limitation of iodide concentrations on volatile iodocarbon formation rates.

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11. P18L3-8. This paragraph describes what happened in the Kiel Fjord study, but what is the relevance to the current study. Were lagged correlations used for the Bedford Basin?

12. P19L14+. Section 4.3.2. Please clarify the points that are being made in this section, at present it is confusing.

13. P26L9+ CH3I production etc.. Of the environmental variables shown for Bedford Basin, there is a similar pattern to the temporal change of water temperature and CH3I concentrations (Table 2, Figure 7). Yet this receives little attention, despite the fact it appears to be considerably more closely related to the CH3I temporal pattern than irradiance? Why this may be the case would be worth considering.

14. P27L13. The sentence beginning 'Iodocarbon concentrations. . .' is difficult to interpret.

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