

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 #1

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General comment The manuscript "A three year time-series of volatile organic iodocarbons in Bedford Basin, Nova Scotia : a Northwestern Atlantic fjord, os-2018-70" by Shi and Wallace, measured the concentrations of CH₃I, CH₂ClI, and CH₂I₂ in seawater as well as bio-chemical compounds, such as oxygen and nitrogenous nutrient, at a fjord in NW Atlantic. They found that dihalomethanes are the main constituent among the three iodocarbons. They estimated a sea-to-air flux of CH₃I, and net production rate of CH₃I in the surface layer. In the deep layer at 60 m depth, the CH₃I concentration gradually increased and plateaued for several weeks, and then, the CH₂I₂ concentration increased in duration of the CH₃I plateau. They suggested a “switch” of iodocarbon production type from “methylation type”, which produces CH₃I only, to “haloform type”,

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which mainly produces CH₂I₂ with decreasing oxygen concentration. This is the first report of weekly measurement of iodocarbon in seawater from the surface to bottom layers at an observation station for three years. The long-term observation data is valuable for publication, however, the observed trends were similar to the previous observations. Note that the increase of similar knowledge about the temporal trends of iodocarbon in coastal area is valuable. The interpretation of production of iodocarbon via HOI or I₂ formation by this study is also similar to the previous studies. This study supported the previous interpretations. The original finding of this study is a “switch” of iodocarbon production type from “methylation type” to “haloform type”, based on the concentration changes in iodocarbon at 60 m depth. And, the authors explained that several changes in iodocarbon concentration were attributed to water intrusions from open ocean to the fjord.

Important point (1) Nevertheless of importance of concentration changes at 60 m depth, it is difficult to follow the explanation described in the manuscript comparing with the Figure 4d (iodocarbon at 60 m depth) and Figure 3 (salinity profile). It is necessary to add some detailed figures to explain timings of concentration changes of iodocarbon and water intrusions (based on analysis of salinity changes), etc.

Important point (2) Authors calculated the mass balance of CH₃I in the top 10m of water column except for winter, however, mass balance must be calculated in the surface mixed layer defined by vertical variance of density. The thickness of the surface layer, in which water can contact with air, is crucial to estimate the balance between sea-to-air out-flux and net-production in the surface layer. The weekly observation of this study make it possible to set the thickness appropriately. I believe that it makes this paper more valuable.

Specific comments Abstract and discussion “hypothesis that near-surface iodocarbon production is linked to reduction of iodate to iodide” I agree that dihalomethane production is supposed to associated with I₂ (or HOI) production and subsequent reaction with organic matter. However, there is no evidence that I₂ production is attributed to

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iodate reduction. Some previous studies supposed that I₂ is produced as a result of iodide oxidation. Iodate reduction might occur in the surface seawater with depleting DIN. I can approve the hypothesis that near-surface iodocarbon production is linked to reduction of iodate to iodide at a discussion chapter, however, the hypothesis should not be described in the abstract.

Abstract Line 24: (2- 3 X) Does it mean “two to three times” ? I am not sure that 2- 3 X is appropriate in scientific writing.

Introduction Line6: “CH₂CII (hours) and CH₂I₂ (minutes)” is correct.

Page 7, Line 8: dissolved inorganic nitrogen (DIN) The definition of DIN (= NO₃ + NO₂ + NH₄) should be described at here, instead of page8 line 22.

Page 8, line 6- 12, etc. The authors explained degradation of organic matter using oxygen concentration. I think that AOU is better to explain the degradation of organic matter.

Page 9, Line 22: “Missing from the bottom water time-series, were the ca. 1 month duration variations seen in summertime surface water.” I cannot understand the meaning of this description.

Page 16, line 16-18: “The correlations between [CH₂CII] and [CH₂I₂] in Bedford Basin (table 1) are consistent with this photochemical transformation” How is the correlation (CH₂CII vs CH₂I₂) consistent with photochemical transformation? I think that the photochemical transformation of CH₂I₂ to CH₂CII should decrease the correlation under the low CH₂I₂ production rate, and photochemical transformation should increase the correlation under the high CH₂I₂ production rate, which is sufficiently exceeding photo-degradation of CH₂I₂. Authors should explain about the link between photochemical transformation and correlation (CH₂CII vs CH₂I₂).

4.3.1. and 4.3.2 I cannot clearly understand the difference between the seasonal variations and the sub-seasonal periodicity. Were there any periodicity within sub-seasonal

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scale during summer? If there is not a periodicity, “4.3.2 sub-seasonal periodicity ” is not necessary.

The authors described both “methyl iodide” and “CH₃I” in the manuscript. Authors should choice either “methyl iodide” or “CH₃I”.

Page23 - Page 24. I cannot follow the description of the manuscript (page 23 -24) comparing with the figures. For example, from a description of “a mid-depth intrusion of salty, offshore water (as denoted by the 31 salinity contour)”, I cannot read the intrusion timing comparing with temporal variation of CH₃I. The authors should make a major revision in page 23 -24 adding some figures to support the explanation, as commented in “general comment”.

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