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Biogeosciences Discussions



# *Interactive comment on* "Response of halocarbons to ocean acidification in the Arctic" *by* F. E. Hopkins et al.

**Anonymous Referee #2**

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This paper describes the temporal dynamics of 11 different halocarbons during a period of 27 days in 9 CO2-manipulated mesocosms that contain polar seawater from the west coast of Spitsbergen. Over the course of the experiments, the mesocosms received additional nutrients twice. As a consequence, phytoplankton parameters show three distinct biomass peaks and hence all results are discussed within the structure of these three phases. Except maybe for M1, which appears an outlier with respect to halocarbon dynamics, all mesocosms appear to perform very similar and reproducible, which is an achievement in itself. As far as I can judge, the methods applied in this paper are sound and accurately carried out and the result is a solid database of a large range of climatically important halocarbons. The authors do a good job in organising the results of the halocarbons in 3 different groups based on their common biological production pathways and removal mechanisms: the I-monohalocarbons, the



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I-polyhalocarbons and the Br-polyhalocarbons. In the Discussion, only the dominant or most important halocarbon of each group is discussed. In this way, the reader does not drown in all data, but is presented with the results that are of interest. This is a clear-cut structure and the reader is presented with valuable information on production and removal processes of these selected compounds.

There are a couple of issues that are unclear to me and on which I advice revision:

i) Tables 2, 4 and 5 present correlations of relationships between halocarbons and selected biological parameters. However, it is not possible to deduce whether the relationship is positive or negative. Moreover, the authors themselves seem to be confused too (see comments below). Please add this information in the tables and make explicit in the text when dealing with a positive and when with a negative correlation.

ii) Section 4.1.2: 1) The text on p. 8216 suggests that Table 4 indicates positive correlations only, but figures 3 E-H show a more complex relationship and on p. 8217 the relationship between CH2I2 and bacteria is deemed negative, whereas it clearly is positive. Please clarify. 2) The ratio of CH2I2 to a number of biological parameters were found to be correlated with pCO2. This is however due to the fact that these biological parameters vary with pCO2, not CH2I2! This ratio correlation can be found for any parameter that did not change with pCO2 and does not give causal information on trends in CH2I2. Hence fig 5 plus discussion can be deleted (btw: 5c does not exist). 3) The suggestion that up-regulation of CH2I2 might be an indication of an antioxidant function and hence perturbed cell physiology with increasing pCO2 is interesting, but also puzzling, because, if anything, one would expect a reduction in ROS production with increasing pCO2, not an increase. So, are we looking here at the response to an unknown stress factor inducing ROS? This nuance should be added when discussing this potential function.

iii) Section 4.1.3, last paragraph: This is a relatively lengthy discussion of observed differences in CHBr3 between mesocosms at 1 point in time (t21). The difference



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is attributed to bacterial activity and not numbers, since those do not deviate. But if bacterial activity is so important from one day to the other, than why doesn't it get any attention in the rest of the discussion? If bacterial activity does change at other instances, without any impact on halocarbons, that would be interesting to know as well and would put the importance it is given now into perspective.

Minor details:

- Sections 2.5.3 and 2.5.4 are in fact subsections of 2.5.2. Please renumber. Also I was wondering whether this means that the samples for "chla and additional phytoplankton pigments" were NOT taken from the same cast used for halocarbons. And if not, do you expect any differences. It would make sense to write a paragraph on sampling the ancillary parameters right under 2.5 and then delete 2.5.2.

- In a printed version, figure 2 is far too small. Please reformat.

- Section 3.2: "To simplify analyses and to give an overview of general trends, the halocarbons were assigned to three groups. . ." Not only that: you also present means of all treatments. Please add this in the wording, since it is now stated nowhere.

- Section 3.3: What is exactly meant with "cumulative concentration"? How was it calculated? And why is it a better representation than concentration only? R is exactly the same.

- Discussion, 3rd line: Table 6 is in fact the 2nd Table referred to. Please change in order of appearance in the text.

- Page 8215, Line 23: There are several reasons why UVR is relatively low in June at Ny-Alesund, but not the solar zenith angle, which is at it's highest at that time of the year. Delete that part of the sentence.

- Figure 6A: Y axis indicates "net loss rate minus flux" but figure legend indicates net-loss rates only. Please clarify/change.



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- The very last sentence of the conclusions suggests an increase in importance of halocarbons with retreating sea ice, but there is really no evidence for that presented in this paper. Firstly because there is no indication of increased production of halocarbons with increased biological productivity, which is predicted to take place with the loss of sea ice. And secondly because this paper doesn't show what the effect of sea ice on halocarbon production is. This work enhances our understanding of halocarbons all right, but not in the context of reducing sea ice.

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Interactive comment on Biogeosciences Discuss., 9, 8199, 2012.