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Interactive Comment

Interactive comment on "Glyoxal and methylglyoxal in Atlantic seawater and marine aerosol particles: method development and first application during the Polarstern cruise ANT XXVII/4" *by* M. van Pinxteren and H. Herrmann

Anonymous Referee #1

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This paper reports on glyoxal and methylglyoxal measurements in Atlantic seawater and marine aerosol particles by means of a newly developed analytical procedure based on PFBHA derivatisation, solvent extraction and GC-MS (SIM) analysis. Some data from a first application during a Polarstern cruise, in spring 2011, are presented.

In this study, the PFBHA derivatisation has been selected due to its easy-to-use approach combined with the sensitivity and robustness of GC-MS analysis. It is suggested that the aim of this work was to validate (and apply) this analytical method for measuring GLY and MGLY in sea-water and in aerosol samples.

In this context, I regret that the experimental section does not provide more details about this new analytical method such as possible interferences, . . . Would sulfur compounds interfere with this derivatisation method? Etc. . . If finally, the method is not new then maybe some rewording at several locations in this manuscript would be required. Also, why just focusing on these two aldehydes (which are of course important)? The chromatograms showed in figure 2 do exhibit several other peaks that might be of interest. Have you made any attempt to identify them?

However, I had the impression that the core part of this manuscript corresponds to the field measurements and not the analytical developments. The field measurements are very interesting as they combine bulk water, surface microlayer and aerosol analysis – sometimes co-located. The data are nicely presented and some attempts have been made to interpret the observations. Some conclusions are drawn, which may appear to be constructed on shaky basis due to the limited data set. But clearly, this first application comes along with big promises for the future.

It is concluded that the enrichment factor of the two carbonyls in the SML implies photochemical production (with no indications of biological carbonyl production or depletion), supported by the trend of a higher enrichment at higher temperatures. However, a direct correlation to global radiation was not strong. But is the global radiation the best factor to look at? What about some J values of some selected DOC components in the bulk or surface waters? Would then the correlation be stronger? Finally a correlation between air temperature and EF is shown but not with the carbonyls concentrations. Is this coherent? Was there any record of the temperature of the different waters being collected (BW vs. SML)?

In the aerosol phase, a negative correlation of GLY and MGLY to Chla was observed for GLY and less pronounced for MGLY. This is being explained by the fact that at higher biological activity, the latter represents rather a sink than a source for these compounds.

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Interactive Discussion

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But as in seawater no such correlation between Chla and carbonyl concentrations were found, I'm wondering how valid this hypothesis is. In fact, is this not pointing to the fact that some other process is involved, maybe gas phase chemistry, changing the local oxidation capacity and therefore affecting the local (gaseous) production rates of GLY and MGLY? If so, then maybe there is even such a negative correlation between Chla and oxalate?

On the same trend, is the positive correlation between the carbonyls in the SML and in aerosol particles really indicative of phase exchanges of the carbonyls? The same correlation would exist with out-gassing of gaseous precursors of these carbonyls?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 15301, 2013.