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Biogeosciences Discussions

Interactive comment on "Temporal variations in abundance and composition of intact polar lipids in North Sea coastal marine water" *by* J. Brandsma et al.

Anonymous Referee #2

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The work by Brandsma and colleagues presents an extensive data set of intact polar diacylglycerollipids (IP-DAGs) from a coastal environment in the North Sea. To date, such data are not available and are an important addition to the already published IPL papers from open marine environments. Samples were collected over a time period of one year and the relative IP-DAG distributions are displayed and discussed. Apart from the IPL data, the authors included information on the microalgal communities, nutrient availability, productivity, and chlorophyll-a. Finally, statistical analysis has been performed and the authors found that the overall IPL composition did not change significantly within the year. Even in the major microalgal bloom in april the authors pointed out that only minor changes occurred in the IP-DAG distribution, though one hapto-

phyte algae (Phaeocystis globosa) was the dominant algae in the bloom period. In the following are some comments which may be considerd by the authors for their revision:

A) As already mentioned by B. Van Mooy in his comments I also wondered if the algal community data and those of the IP-DAG data have also been included in the PCA test. At least in the major bloom period in april some IP-DAGs seem to be more prominent than before and after the major bloom in april. Not only the DGTs and the SQDGs were dominating in the Phaeocystis bloom, but also some specific IPLs out of these two groups and out of the PG group showed up in this time period. From beginning of april (4/4) to mid of april (23/4) the colonial form of Phaeocystis was predominating over the single cell form. All other algaea only played a minor role. Some compounds, as for example 36:5 DGTS, 32:3 DGTS, and 35:0 PG, showed a significant increase compared to the pre- and post-bloom periods. In the second part of the bloom from 23/4 to beginning of may only one compound (35:2 DGTS) appeared to be more prominent than in the pre- and post-bloom periods. Moreover it appears to me as if the IP-DAGs containing PUFA fatty acid chains are as well more prominent in the bloom. It may be true that the overall IPL distribution does not allow an assignment to any organism at high diversity, but in the major bloom period an assignment of few IPLs to specific microalgae may be possible. The authors should check their data once again and verify if there are any specific IP-DAGs at least in the april bloom.

B) A technical comment to table S2. This table is quite hard to read. Only on the first page the days can be found where the samples were taken. Please provide the date for each page separately.

C) Page 8908, lines 14/15 the expression "microbial" is used for microalgae and bacteria, but I would prefer the expression microalgal community. The role of bacteria is only poorly discussed and not the major focus of the paper.

D) Page 8908, lines 18-20: Even if there is only little change it can be significant enough that it should be discussed more thoroughly by the authors.

E) Technical comment Table S1/S2: The 35:5 PC and 37:6 PC are missing in Table S2, as well as the 35:0 PG. The authors should include these data or omit them from Table S1.

F) I agree principally that IP-DAGs do indeed have a limited chemotaxonomic potential, but the authors should check once again if some of the IP-DAGs may be more useful as source-specific biomarkers (see comment above). If this is not the case I suggest to stress this finding already in the title.

Interactive comment on Biogeosciences Discuss., 8, 8895, 2011.