

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

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The results presented by Brandsma et al. in this paper fill a number of significant gaps in our understanding of intact polar diacylglycerolipids (IP-DAGs). First, this is one of the first reports of IP-DAGs from a coastal environment, and this is important because the majority of existing literature on IP-DAGs comes from studies conducted in off-shore environments. Thus the study of Brandsma et al provides a much-needed new end-member for our field. Second, there are no other time-series studies of IP-DAGs, and thus the work in this paper provides an important new perspective on the linkages between IP-DAGs, planktonic community structure, and environmental stressors. Fi-

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nally, the IP-DAG data are presented alongside very detailed data on algal community composition; again, this is something that has yet to be presented in any other paper. Altogether, these three new types of information comprise a significant advance in the study of IP-DAGs.

There are a couple points that the authors might consider addressing in revision:

1) Is there a way to take greater advantage of the detailed data on algal community composition? It is not clear if the data in Fig 3 were integrated into a PCA test. Just perusing the IP-DAG data in Table S2, it looks like some of the molecules might be more abundant during the *Phaeocystis* blooms.

2) The authors note that there was no correlation between the non-phosphorus lipids and phosphate concentrations, which is important to point out (they would be remiss if they did not). They also suggest that this means that non-phosphorus lipid substitution was not occurring in response to phosphate scarcity. However the nutrient data they present is really insufficient to conclude that phosphate was scarce from a physiological standpoint. So, doesn't the observed lack of a correlation between non-phosphorus lipids and phosphate concentrations suggest instead that phytoplankton were simply not stressed by phosphorus scarcity? It seems to me that this latter conclusion is more strongly supported by their data. Perhaps the authors should consider some batch culture experiments with *Phaeocystis*: these are the algae that dominated when phosphate concentrations were lowest. If they found that these algae do not substitute, then they would have a much stronger case (and an even more significant paper on their hands).

3) The conclusion that IP-DAGs have limited chemotaxonomic potential is very solid, and could be much more prominent in the paper. Perhaps this conclusion should be included in the title?

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