

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2021-232

Anonymous Referee #1

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Referee comment on "Bacterial and eukaryotic intact polar lipids point to in situ production as a key source of labile organic matter in hadal surface sediment of the Atacama Trench" by Edgart Flores et al., Biogeosciences Discuss.,  
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This manuscript reports on the intact polar lipid content of surface sediments in the Atacama Trench and bathyal sediments off the coast of Chile. IPL data from three trench stations (ca. 8000 m) and seven bathyal stations (ca. 500-1200 m) are reported, and they are compared with previously published data from six depths in the overlying water column. These are novel data that are potentially useful for understanding the decomposition/preservation of IPLs in sinking particulate organic matter as well as inputs of fresh organic matter from deep-dwelling/benthic microbes. The authors use several statistical techniques to describe relationships among the lipid classes at the different sample locations and report on the similarity/dissimilarity of lipid distributions at the different locations.

The authors first report IPL head group distributions as sample clusters showing that similar lipid distributions are not a function of geography (trench vs. bathyal) or depth interval (0-1, 1-2, 2-3 cm) in the trench surface sediment. The authors then report the distribution of specific IPLs (based on tail groups) in these samples and similarity of specific IPL distributions in the headgroup clusters. The dominant IPLs are reported based on their number of acyl carbons and double bonds. The degree of unsaturation in sediment IPLs is compared with water column IPLs showing a greater degree of unsaturation in the sediments. The final component of the manuscript focuses on identifying IPLs that are distinctive to the trench (hadal) environment and considers sources of sedimentary lipids, whether produced in situ in the sediments, transported vertically from the water column, or horizontally from other sedimentary locations. Overall, the manuscript is well structured and well written. It reports IPL distributions for an understudied marine environment and applies statistical analyses that have the potential to strengthen interpretations of lipid distributions.

Specific Comments:

The lipid extraction and analytical methods are appropriate for the sample types in the study. Please report the type of chromatographic column in the methods section.

I appreciate the use of deuterated standards to account for matrix effect on quantifying lipids. Would it be possible to include an assessment of the importance of this treatment for the benefit of planning future studies on sedimentary IPLs? How was the correction applied? What was the overall impact of including this step on the final data reported? Were there different effects observed for the different lipid classes? Since deuterated standards were added just for PC, PG, PE and DGTS, was there a way to correct the concentration calculations for the other lipid classes as well? If not, how does this affect quantitative comparisons between lipid classes?

Figure 3 is very helpful for understanding the clusters described in Figure 2. It shows which specific IPLs are the main controls on the differences in lipid class composition that defines clusters. The font size in figure 3 is very small, though, so could that figure be rotated 90 degrees to increase its size?

In Table 2 and in the text, how did the authors decide to use the DAG designation for PE and PG abbreviations at some places and not others? PC also has AEG species, so should PC-DAG be used? It also appears that SQ and SQDG are used interchangeably. I'm mostly thinking about using consistent naming, so the reader isn't looking for a distinction between different abbreviations used for the same compound.

In sections 4.2, 4.3, and 4.4, I really like the introduction paragraphs for each IPL class describing possible biological sources.

How do the authors define IPLs with short vs. long-chain fatty acids? Is there a consistent definition, or does it vary by IPL class? In lines 472-473 and 575-577 there is overlap in the ranges.

Figures 7, 8, and 9 are very difficult to read at the size of a printed page. Would it be possible to provide a simpler summary figure in the manuscript and include the three figures as nine individual figures in the supporting information?

The conclusions section could better represent the work. Conclusion 1 (801-804) is well supported. The middle part of conclusion 2 (806-814) repeats part of conclusion 1. The five IPLs listed on line 808 are in such low abundance that I don't think they warrant their own conclusion as possible distinctive biomarkers of trench communities. They could very well be in other biota but unreported because they are quantitatively insignificant in an organism's lipidome.

Why not focus conclusions more on drawing specific conclusions based on the significant differences in PG, PC, MGDG, DGDG, SQDG, DGCC, DGTS, and DGTA that are so clear in Figures 7-9? That's really interesting and should be a major part of the conclusions.

Technical Corrections:

Line 596: Add heading for Potential Sources of Betaine Lipids.

Line 649: Are the 21-38 carbons just on the fatty acid or also on the ceramide chain?

Line 694-696: Correct incomplete sentence.